HOW DO RIGHTS REVOLUTIONS OCCUR? FREE SPEECH AND THE FIRST AMENDMENT

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Abstract Does obscenity law affect moral values and does it matter? Using random judge assignment and all U.S. obscenity precedents since 1958, we present four main results. Progressive laws liberalized sexual attitudes and behaviors, reduced child abuse by 56 arrests per 100,000 population, and increased asymptomatic STDs by 50 incidents per state per year. We document that newspapers reported on obscenity cases. Data entry workers randomly assigned to transcribe newsreports of progressive decisions shift attitudes and second-order norms, consistent with a model where laws sanctioning activity increase its perceived prevalence and laws shape values when sanctioned activities are prevalent.

Keywords: Law and norms, expressive law, cultural change

JEL codes: J12, Z1, N32

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1 Introduction

Laws do not shape values in neoclassical models of law and economics (Becker 1968). A large body of work in psychology, however, suggests that laws can affect values (Tyler 2006). A fundamental rationale of free speech regulation has been to protect the moral fabric of society. Does obscenity law affect moral values and does it matter? We examine obscenity law in the U.S. to present causal evidence on four outcomes that have been commonly cited by judges to restrict free expression: breakdown of moral standards,¹ sexual violence,² child sexual abuse,³ and disease and drugs.⁴

To identify causal effects, the ideal experiment would randomize legal rules (Besley and Case 2000). There is a broad literature in law and economics that documents the cross-country association between legal rules and socioeconomic developmental outcomes (Glaeser and Shleifer 2002; Djankov et al. 2003; Anderson 2018; Ashraf et al. 2019). We leverage random assignment of judges, as their biographies predict rulings. A substantial body of work finds that judges matter (Lim et al. (2015, 2016); Mehmood (2022)). In our setting, Democrats vote differently from Republicans in Circuit Courts (Sunstein et al. 2006).⁵ Circuit Court rulings establish precedent for large U.S. regions that comprise up to nine U.S. states. Moreover, Circuit rulings comprise almost all of the precedents that future judges must follow since the U.S. has a common law system and because the Supreme Court hears less than 2% of Circuit cases. Judges are repeatedly randomly assigned to panels of three, and the composition of these panels varies by case. We analyze all free speech precedents pertaining to obscenity since 1958 collected by Sunstein et al. (2006) and Kastellec (2013). Throughout this period, Democrats have prioritized freedom of speech and expression while Republicans have prioritized minimizing the secondary harms of free speech. Conflicting policy goals have led judges to “weaponize” First Amendment jurisprudence, which governs much of U.S. free

¹*Fort Wayne Books v. Indiana*, 489 U.S. 46 (1989)

²*Amatel v. Reno*, 156 F.3d 192 (D.C. Cir. 1998)

³*Ginsberg v. New York*, 390 U.S. 629 (1968)

⁴50 AM. JUR.2d §§ I, 2 (1995)

⁵We refer to judges appointed by Democratic presidents as Democrats and those by Republican presidents as Republicans for brevity.

speech policy.⁶

Exploiting the quasirandom variation of judges assigned to the cases, we find that the assignment of judges affects outcomes in ways that reflect the preferences of judges proxied by their decision-making tendencies. Progressive free speech precedent increased progressive attitudes and behaviors. Conservative free speech precedent reduced sex crimes (with the exception of child abuse) and asymptomatic STDs, particularly chlamydia. The point estimates indicate that, on average, four years after progressive decisions, individuals are 0.8 percentage points more likely to say extramarital sex is OK, 1.4 percentage points more likely to say premarital sex is OK, and 0.3 percentage points more likely to say homosexual sex is OK. Progressive obscenity decisions also increase the likelihood of paid sex by 0.4 percentage points, the number of sexual partners per year reported by men by 0.28, the number of female partners reported by men by 11.3, the likelihood of extramarital sex reported by men by 0.7 percentage points, and the likelihood of being divorced or separated if older than 40 by 1.1 percentage points. In terms of crimes, progressive obscenity precedent increased prostitution (by 3 arrests per 100,000 population) and drug violations (by 36 arrests per 100,000 population), but decreased child abuse (by 56 arrests per 100,000 population). Finally, progressive obscenity precedent increased incidence of chlamydia by 50 (incidents per state per year). We present two main tests for identification: a balance test that provides evidence that random allocation of cases assumption here is warranted and a placebo check, i.e., the assignment of judges to cases in future years have no effect on decisions or outcomes in prior years.

Several studies have linked major court rulings with shifts in public attitudes (Hoekstra 2000). These studies suggest that media plays a prominent role in these shifts.⁷ Media and other information entrepreneurs, such as community organizations, raise awareness of judgments (Weinrib 2012). After obscenity precedent, American Civil Liberties Union (ACLU) attorneys mobilized individuals towards a view that speech should be protected regardless of its social value. Social media also helps communicate and promulgate court decisions (Clark

⁶“How Conservatives Weaponized the First Amendment”, *New York Times*, 06/30/2018.

⁷See, for example, Julia C. Mead, “Village Can Shut X-Rated Store,” *The New York Times*, Section 14LI, Column 5, June 19, 2005; Joyce Price, “‘Community Standards’ ruling stands; On-line porn judged by download site,” *The Washington Times*, p. A6, February 16, 1996.

et al. 2014). We complement these studies with evidence of newspapers reporting on Circuit obscenity decisions. Specifically, we randomized whether we provided information on a progressive or conservative free speech decision. Thus, changes in norms can also occur via media.⁸

We employ data entry workers to transcribe newsreports as a mechanism experiment (Ludwig et al. 2011). To distinguish deterrence (Becker 1968) from an information channel (Tyler 2006), we randomly assign summaries of liberal or conservative obscenity decisions. We verify that exposure to legal precedent affects values, but does not affect self-reported historical behavior within the short-time frame of the experiment.⁹ This suggests that the behavioral changes in the population-based analysis are not simply due to changes in openness to discussing certain behaviors. The role of material penalties is unlikely to be significant in the short time frame of our experiments. This evidence is inconsistent with deterrence as sole mediator for the effects of law. It shows that laws shape values simply through information.

We also explore heterogenous effects in line with the theory of law and norms (Bénabou and Tirole 2012). Laws sanctioning an activity issue a signal to individuals that the activity is more prevalent than previously thought. If the sanctioned activity is rare, then laws normalize the sanctioned activity. Our data entry experiment verifies the information channel that sanctioning an activity increases its perceived prevalence. Moreover, normalization can lead to law having unintended consequences, which we document in the early time period of our data. Our final empirical exercise examines the deterrence channel as directly as possible. We collected data on state-level sales of pornographic magazines that were often parties in obscenity litigation. We do not find that magazine circulation is affected by free speech decisions, though we cannot rule out a possible change in content or change in other channels for obscene content that reaches the public.

The remainder of the paper is organized as follows. Section 2 provides historical and legal

⁸Newspapers, advocates, and community organizers publicize Circuit Court decisions (Pastor 2007; Eagle 2007; Sandefur 2005). Public opinion is affected where the case originates (Hoekstra 2000). Municipalities increase or decrease regulations or modify existing ordinances in response to court decisions (Berliner 2003; Nader and Hirsch 2004).

⁹Ludwig et al. (2011) discuss the value of “mechanism experiments” to identify the central behavioral mechanisms even if the intervention that is tested (or its setting) does not correspond exactly to any realistic policy option.

context, briefly summarizes the theory, and describes the data. Section 3 details the empirical strategy. Section 4 presents the impacts of judge identity on obscenity rulings. Section 5 estimates the effects of obscenity precedents. Section 6 examines newspaper reports and the data entry experiment. Section 7 interprets the results through the lens of theory. Section 8 concludes.

2 Background

2.1 Historical and Legal Context First Amendment jurisprudence affects many aspects of society. Policies affected by these cases include the government’s ability to regulate mail, magazines, books, movies, internet, and phone calls. The regulations of these media are typically studied one at a time. One study attributes 7% of births to portrayals of intimate relations on television (La Ferrara et al. 2012). A second study attributes 10% of divorce to broadcast television critical of traditional values (Chong and Ferrara 2009). A third study finds 52% more pregnancies and 8% greater acceptability of domestic violence to cable television (Jensen and Oster 2009). A fourth study attributes 25-30% more female oral contraception use after one Supreme Court decision (Bailey 2010). And a fifth study attributes 3.2% of rapes and 2.5% of sex crimes and child sex abuses to internet broadband (Bhuller et al. 2013).

Recent general equilibrium modeling attributes 50% of the sexual revolution to individuals’ moral views on sexual rights (Fernandez-Villaverde et al. 2014). Peer effects generate positive feedback (Card and Giuliano 2011). If free speech precedent gives people more room for progressive expression and if more progressive community standards make it easier to subsequently challenge regulations that are deemed as restrictive, this dynamic could lead to multiple steady-states, in which abrupt shifts in norms can occur (Akerlof et al. 1996; Cooter et al. 2008).

Historical studies document backlash by conservatives to stop the Supreme Court from encroaching on state rights to control pornography during the 1950s and 1960s. From 1959 to 1966, bans on three books with explicit erotic content were challenged and overturned. Prior to this time, a patchwork of regulations, local customs, and vigilante actions governed

what could and could not be published. For example, the United States Customs Service banned James Joyce's *Ulysses* (1922) by refusing to allow it to be imported into the United States. Different cities and organizations had their own rules for allowable content. The Warren Court (1953-1969) greatly expanded civil liberties and in *Memoirs v. Massachusetts* and other cases curtailed the ability of municipalities to regulate the content of literature, plays, and movies. For six years, it reversed summarily—without further opinion—scores of obscenity rulings by lower state and federal courts, culminating in the 1969 decision *Stanley v. Georgia* (394 U.S. 557), which held that people could view whatever they wished in the privacy of their own homes.

The last ruling led the U.S. Congress to fund the President's Commission on Obscenity and Pornography. The 1970 Commission found that there was “no evidence to date that exposure to explicit sexual materials plays a significant role in the causation of delinquent or criminal behavior among youths or adults,” “no evidence that exposure to explicit sexual materials adversely affects character or moral attitudes regarding sex and sexual conduct,” and concluded that “legislation prohibiting the sale, exhibition, or distribution of sexual materials to consenting adults should be repealed.” These findings were roundly rejected and criticized by Congress. In the immediate aftermath, opposing groups authored minority reports that dissented with the Commission's view, which was subsequently cited by the U.S. Supreme Court in later conservative decisions. When Chief Justice Warren was to be replaced by Justice Fortas, a conservative group led by Senator Strom Thurmond organized the “Fortas Obscene Film Festival” (featuring transvestites), which led to the resignation of Justice Fortas and the nomination of Justice Burger. By 1973 Justice Burger issued the *Miller* test which repudiated the “utterly without redeeming social value” standard from *Memoirs* in favor of the markedly less liberal “lacks serious literary, artistic, political, or scientific value” (Boyce 2008).

Accordingly, since 1973, the legal standard defining obscenity in the U.S. has been the three-part *Miller* test set out in the Supreme Court decision *Miller v. California*, 413 U.S. 15 (1973). The *Miller* test defines material as obscene if “the average person, applying contemporary community standards” would find that the material (1) “appeals to the prurient

interest”; (2) has “patently offensive” depictions of sexual conduct; and (3) “lacks serious literary, educational, artistic, political, or scientific value.” Before the *Miller* test, the *Roth* test allowed banning obscenity when the average person, applying contemporary community standards, would consider that the dominant theme of the material, taken as a whole, appeals to prurient interests. Moral harms and their “secondary effects” (i.e., sexual violence, disease, and drugs) were discussed in the Supreme Court decisions *Young v. Adult Mini Theatres, Inc.* 427 U.S. 50 (1976) and *Renton v. Playtime Theatres, Inc.* 475 U.S. 41 (1986) regarding obscene speech. Further details on obscenity law can be found in the Appendix.

2.2 Empirical Setting There are three layers of federal courts in the U.S.: District Courts, Circuit Courts, and the Supreme Court. The 94 U.S. District Courts serve as the general trial courts, where there is a jury. If a party appeals the decision, the case goes up to a Circuit Court, which randomly assigns three judges to decide in a panel and where there are no juries. The 12 U.S. Circuit Courts, also known as federal appellate courts, only hear cases presenting new legal issues (under 20% of District Court opinions are appealed). Only Circuit and Supreme Court cases create precedent.

Each Circuit Court decides many thousands of cases per year, and only 2% of Circuit cases successfully appeal to the U.S. Supreme Court. Accordingly, each year Courts of Appeals determine the vast majority of decisions that set legal precedent. Circuit Court decisions are *binding precedent*, but only within that Circuit.¹⁰ Circuit Court decisions are *persuasive precedent* on state courts within the Circuit. Persuasive precedent must be adopted by the state courts to become binding precedent. State officials are instructed to establish and annually update a set of guidelines based on federal and state law to assist state agencies to avoid exposure to costly litigation after Courts of Appeals decisions (Frost and Lindquist 2010; U.S. Department of Transportation, Federal Highway Administration 2005; Pollak 2001).

Each case in the Courts of Appeals receives *three randomly assigned judges* out of a pool

¹⁰When Circuits choose to adopt the precedent of another Circuit, it is typically with some delay: before an opinion can be issued in the new Circuit, a case bringing the same issue of law must be filed in a District Court, appealed to the Circuit Court, and decided upon.

of life-tenured judges, numbering roughly 8 to 40 depending on the size of the Circuit.¹¹ According to interviews, each court implements randomization differently. In some Circuits, two to three weeks before the oral argument, a computer program randomly assigns available judges to panels who will hear cases. In other Circuits, judges are randomly assigned to panels up to a year in advance; cases that arise are randomly assigned to panels. Some judges take a reduced caseload if retired or visiting, but all are randomly assigned by a computer algorithm. Chen and Sethi (2011) formally tests for randomization by showing that case characteristics as determined by District Courts are not correlated with the characteristics of the Courts of Appeals judges assigned to the case. Further details on randomization can be found in the Appendix.

2.3 Conceptual Framework The law and norms model (Bénabou and Tirole 2012)¹² assumes three motivations for human behavior: (1) intrinsic motivations, where people perform an action simply because they believe it is the right thing to do; (2) extrinsic motivations, where material incentives and deterrence influence actions; and (3) social motivations, where values, norms, and social sanctions affect actions. People accrue honor or stigma for actions outside the norm. Two different views of free speech emerge: (1) law shifts social motivations towards what the law values, i.e., it reinforces the potential deterrent effects provided by the legal sanction; or (2) law shifts social motivations away from what the law values and it undermines the law’s intention. As shorthand, we label the former as an expressive effect and the latter as backlash.

The intuition is that material penalties indicate that the judge sees a problem. The judge has information about some underlying activity and issues a penalty when he or she believes it should be deterred. Upon observing the precedent, community leaders and individuals update their beliefs about the underlying distribution. If the activity was very scarce, then backlash occurs. Previously stigmatized activities become normalized. If the activity is common, expressive effects occur. In Appendix A, we link the model to the empirical specification.

¹¹Their positions and decisions are highly esteemed. Except for retirement, Courts of Appeals judges typically leave the bench only for a position in the U.S. Supreme Court.

¹²A fuller account of the model is in Appendix A.

The model is operationalized in the General Social Survey (GSS), where people respond to questions about the morality of particular actions. By reporting their perceived morality of an action, respondents report the difference in the social perception of someone who chooses an action vs. the social perception of someone who does not choose an action.

2.4 Legal Data We collected four legal datasets. Our first two datasets comprise the universe of Circuit and District rulings on obscenity. Sunstein et al. (2006) and Kastellec (2011) collect data from 1958 to 2004, which we extended to 2008, a total of 175 rulings, which are listed in Appendix Table 1. The authors first selected major Supreme Court precedents.¹³ Then, they selected Circuit Court cases citing these cases and restricted to three-judge cases that deliberated on the topic substantively. The authors coded a vote as progressive if the judge found that individual interest in free expression outweighed the state’s interest in protecting individuals from the effects of speech. We follow their method to collect all District Court cases, a total of 2,960 rulings. Additional background is provided in Appendix B.

We also collected data from the Administrative Office of the U.S. Courts (AOC) and the Public Access to Court Electronic Records (PACER) filings on District Court cases to merge judge identities.¹⁴ The administrative data facilitates additional randomization checks. Our data on judge biographical characteristics come from the Appeals Court Attribute Data, District Court Attribute Data,¹⁵ Federal Judicial Center, and our own data collection. Variables include: geographic history; education; occupational history; governmental positions; military service; religion; race; gender; and political affiliations. Raw data on religion come from Goldman (1999).¹⁶ Judges whose religions remained missing or unknown were coded as having no publicly known religious affiliation. We filled in missing data by searching transcripts of Congressional confirmation hearings and other official or news publications on Lexis. Ta-

¹³*Miller v. California*, 413 U.S. 15 (1973), *Roth v. United States*, 354 U.S. 476 (1957), and *A Book Named “John Cleland’s Memoirs of a Woman of Pleasure” v. Attorney General of Massachusetts*, 383 U.S. 413 (1966)

¹⁴Sixteen years of PACER filings are available on open source sites for 33 Districts. We used PACER data to obtain judge identities that are missing in the AOC data.

¹⁵<http://www.cas.sc.edu/poli/juri/attributes.html>

¹⁶Additional religion data are available at <http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm>.

ble 1 displays summary statistics. Roughly two-thirds of these are conservative decisions. The share of progressive decisions declines after 1973.¹⁷ A dramatic spike is observed, which Songer and Haire (1992) attribute to the causal impact of a 1973 Supreme Court decision.¹⁸

2.5 Outcomes Data We collect eight datasets to measure the impacts of legal decisions. We use the GSS with state identifiers. We use data on attitudes (e.g., towards homosexual sex, extramarital sex, and premarital sex) and behavior (e.g., number of partners last year, extramarital sex, or paid sex).¹⁹ For attitudes, we constructed a binary indicator for the response “not wrong at all”.²⁰ This binary indicator corresponds to $\Delta(v)$ in the law and norms theory. Since the Supreme Court has instructed the courts to define obscenity according to community standards, we also constructed a measure for community standards using the survey response to whether sexual materials lead to breakdown of morals, the closest proxy to the community standards in the model. We construct demographic controls like age, gender, educational attainment, and race. As standard in the literature, we also use survey weights provided by GSS in our regressions.

We also collated mentions of Courts of Appeals decisions in articles from the major newspaper for the city in which each Circuit Court resides.²¹ These are: *The Boston Globe*, *New York Times*, *Philadelphia Inquirer*, *Richmond Times Dispatch*, *Times-Picayune*, *Cincinnati Post*, *Chicago Tribune*, *St. Louis Post-Dispatch*, *San Francisco Chronicle*, *Denver Post*, *Atlanta Journal and Constitution*, and *The Washington Post*. We collected data from 1979 to

¹⁷ Appendix Figure 4 plots the quantity of free speech cases that were decided progressively or conservatively over time.

¹⁸Our results are robust to removing this spike.

¹⁹The GSS is an individual-level survey that was conducted annually from 1973 to 1994 (except for 1979, 1981, and 1992), and biannually from 1994 to 2004. For each year, the GSS randomly selects a cross-sectional sample of residents of the United States who are at least 18 years old. The GSS had roughly 1,500 respondents per survey year for 1973-1992 and roughly 2,900 respondents per survey year for 1994-2004, yielding a total of 44,897 individuals. We shift the survey responses by one year because people can be surveyed at any time during the year and the number of partners last year may include information from the previous calendar year.

²⁰The other three response choices are “always wrong”, “almost always wrong”, “wrong only sometimes”.

²¹Appendix Figure 2 is a map of the 12 Circuits.

TABLE I.— Summary Statistics

	Mean [Standard Deviation]
Free Speech Cases (1958-2008)	
Number of Judges	16.79 [8.42]
Number of Free Speech Panels	0.30 [0.73]
Proportion of Circuit-Years with No Free Speech Panels	80%
Proportion of Progressive Free Speech Decisions for Circuit-Years with Free Speech Panels	35%
Expected # of Democratic Appointees per Seat for Circuit-Years with Free Speech Panels	0.46 [0.16]
N (circuit-years)	612

2008 from NewsBank using the search term: (obscen*) w/100 (judgment OR "court ruling") AND Circuit AND NOT "Supreme Court".

We employ data entry workers to transcribe newspaper summaries of free speech decisions, randomized to be progressive or conservative. Through three experiments we explore the effects of free speech precedent on 1,345 subjects. First, we should expect an effect on self-reported behaviors of data entry workers if the GSS results merely reflect openness in discussing topics (e.g., paid sex) previously considered to be private. Second, we measure attitudes. The role of material penalties is unlikely to be significant in the short time frame of our experiments. If we see an effect on attitudes of data entry workers, it would be consistent with informational effects of law. Third, we measure beliefs about the prevalence of underlying activity, which is the key mechanism of the law and norms model.²²

To more directly assess the deterrence channel, we obtain state-level data on sales of the pornographic magazines *Playboy* and *Penthouse* from the Audit Bureau of Circulations. Their circulation data was collected annually for a single month's issue, 1955-2010 for *Playboy* and 1970-2010 for *Penthouse*. To assess the societal outcomes that motivate judges, we collected annual data on crime incidents from the FBI's Uniform Crime Reports (UCR), which begins in 1960. County-level arrest data are available for prostitution, rape, and drug-related incidents and are constructed to be arrests per 100,000 people. One UCR measure mirrors the GSS: prostitution arrests and (self-reported) paid sex.

Along with the UCR, we collect the standard controls for studying crime: unemployment rate; per capita real income; police employment; the proportion of the population that is nonwhite; percent urban; infant mortality; and the age profile of the population in each state and year. These variables are obtained from official U.S. government publications.²³ County population is used as weights.

Finally, we collected data on diseases from the Centers for Disease Control and Preven-

²²Our measures of second order norms, like the GSS, are not incentivized for accuracy (Chen and Yeh 2014).

²³Some of the data are available here: <http://bpp.wharton.upenn.edu/jwolfer/data/DeathPenalty/StatePanel.dta>. We extend this series using earlier and later volumes of U.S. government statistical yearbooks.

tion²⁴ for 1984 to 2008 and extend it back to 1960 using Klick and Stratmann (2003). We collected incidence (i.e., new cases) of sexually transmitted diseases—chlamydia, syphilis, and gonorrhea—for each state. Annual state population is used as weights.²⁵

3 Specification

We use regressions of the form:

$$(1) \quad Y_{ict} = \theta_c + \theta_t + \sum_{n=0}^L \beta_{1t-n} Law_{ct-n} + \sum_{n=0}^L \beta_{2t-n} 1[M_{ct-n} > 0] + \eta X_{ict} + \varepsilon_{ct}$$

where β_1 captures the effect of progressive vs. conservative precedent, $\beta_1 + \beta_2$ captures the effect of progressive precedent vs. no decision, and β_2 captures the effect of conservative precedent vs. no decision. All our estimates include β_2 , but we only discuss β_1 , which is the focus of the model and our causal research design leveraging the random assignment of Circuit judges. Y_{ict} is the outcome (attitudes, behaviors, crime, and disease) of individual (or state) i in Circuit c and year t . Law_{ct} is the share of progressive precedents. It is typically 0 or 1, a single verdict. We specify a distributed lag since we are interested in effects over time. Our baseline specification has four years of lags and one lead ($n = -1$ to 4). We extend our specification to include the presence of a decision, $1[M_{ct-n} > 0]$, where M is the number of cases, which is typically 0 or 1. Since random assignment is at the Circuit-year level, clustering standard errors yields roughly identical results when clustering at the Circuit or Circuit-year level.²⁶

Appendix C presents random assignment checks. Our 2SLS can be described more formally as follows. We seek an instrumental variable for Law_{ct} using judges' biographical character-

²⁴U.S. Department of Health and Human Services, Centers for Disease Control and Prevention, National Center for HIV, STD and TB Prevention (NCHSTP), Division of STD/HIV Prevention, Sexually Transmitted Disease Morbidity 1984-2008, CDC WONDER On-line Database, November 2009. <http://wonder.cdc.gov/std-v2008.html> on October 30, 2010.

²⁵<http://www.census.gov/popest/states/>.

²⁶Barrios et al. (2012) show that random assignment of treatment addresses serial and spatial correlation across treatment units, since “if the covariate of interest is randomly assigned at the cluster level, only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals.” We check results using randomization inference that assigns the legal variation to another Circuit and the robustness of our results to using wild bootstrap. The coefficients on the leads serve as an omnibus falsification check for spurious significance.

istics. Let N_{ct} be a biographical characteristic, e.g., the number of Democrats assigned to free speech panels. Let $p_{ct} = \frac{N_{ct}}{M_{ct}} * \mathbf{1}[M_{ct-n} > 0]$, i.e., defined to be 0 when $\mathbf{1}[M_{ct-n} > 0] = 0$. Since Circuit Courts handle thousands of cases per year (a small fraction of which are free speech cases) and the pool of judges available to be assigned comprise $\mathbf{E}(p_{ct})$, there is substantial variation between p_{ct} and $\mathbf{E}(p_{ct})$. Our moment condition for causal inference is: $\mathbf{E}[\frac{N_{ct}}{M_{ct}} \varepsilon_{ict} | \mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0]] = 0$. Each lag of Law_{ct} gets a corresponding lagged instrument. All 2SLS estimates use the limited information maximum likelihood (LIML) estimator because of its better small sample properties.

In robustness checks, we also include controls, such as the crime or GSS controls described earlier. We average the five- to six-year lag of community standards because our main specification includes four lags of the law. We also construct characteristics of the pool of judges available to be assigned.²⁷ Finally, we constructed Circuit-specific time trends to allow different Circuits to be on different trajectories with respect to outcomes. Any omitted variable is likely to be small in practice.

It is also worth noting that, for our legal domain, allowing vs. disallowing free speech exercise is arguably the most salient aspect of a precedent.²⁸ Moreover, newspaper headlines of Circuit Court opinions typically refer to the court and not the identity of the judges on the panel.²⁹ Violations of the exclusion restriction are also likely to be minimal.

To address the possibility that $\mathbf{1}[M_{ct-n} > 0]$ responds to previous years' legal decisions, we instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. Appendix D

²⁷We calculate the expectations based on the composition of the Circuit pool of judges available to be assigned in any Circuit-year, assuming that all judges have an equal probability of assignment. Expected number of judges per seat is a proportion varying from 0 to 1. Senior judges sit less frequently and we weigh their characteristics accordingly in calculating expectations. The results are not sensitive to omitting senior judges and using the exact months in which judges are appointed or retire to calculate their availability.

²⁸An interesting feature of the institutional setting, however, is that it is possible to assess this hypothesis (in conjunction with another auxiliary assumption). If there are other aspects of free speech precedent that are sensitive to judges' biographical characteristics, and if these other aspects of free speech doctrine affect societal outcomes, we should observe correlations between 2SLS residuals and Circuit-year biographical characteristics not used in the first stage. They are not, which suggests that the allowing vs. disallowing free speech dimension of these cases is the primary channel through which free speech jurisprudence has an effect, or that other aspects of free speech jurisprudence are not polarized along judicial demographic characteristics.

²⁹Badawi and Chen (2014) also show there is no stock market response to the identity of the judges when their identities are revealed in Delaware Court of Chancery, which handles corporate disputes and are followed closely by the markets.

presents additional details. The demographic characteristics of District judges predict with whether the judge is reversed by Circuit Courts (Haire, Songer, and Lindquist 2003; Sen 2015; Barondes 2010; Steinbuch 2009), so expected reversal rates could encourage litigants to pursue an appeal. We find that in practice the potential endogeneity of $1[M_{ct-n} > 0]$ does not appear to be significantly affecting the estimates of β_1 .³⁰

4 The Effect of Judge Identity on Court Outcomes

Table II shows that Republicans were less likely to vote for a progressive verdict.³¹ Panel A shows, at the judge-level, Democrats were 10 percentage points more likely to vote for a progressive verdict in Column 1. The point estimate is unaffected with Circuit and year fixed effects in Column 2, share of Democrats, $\mathbf{E}(p_{ct})$, in Column 3, and all controls in Column 4. Panel B shows, at the panel-level, moving from an all-Republican panel to an all-Democrat panel increases the likelihood of a progressive verdict by 26 percentage points in Column 4. Panel C shows, at the Circuit-year level, moving from an all-Republican panel to an all-Democrat panel increases the proportion of progressive decisions by 36 percentage points in Columns 3-6. Columns 1 and 2 verify that increasing the sample size by including $1[M_{ct} > 0]$ does not affect the first stage F-statistic strength for the Democrat instrument. Anderson-Rubin weak instruments-robust test statistics are quite strong. Weighting the regressions by the number of cases in a Circuit-year, where weights are the geometric mean of $M_{c(t-n)} + 1$

³⁰ The results of our mechanism experiment where data entry workers are randomly exposed to obscenity precedent can be interpreted in relation to the population analysis. The population TOT of the Circuit = (Experimental: TOT_{direct}) * P(exposure_{direct}) + (TOT_{indirect} of individuals) * P(exposure_{indirect}). The experiments estimate TOT_{direct} for individuals. The known parameters are TOT_{Circuit} and TOT_{direct} . The unknown parameters are TOT_{indirect} and the probabilities.

³¹ Table II notes presented here due to space constraints: Heteroskedasticity-robust standard errors are in parentheses and clustered at the Circuit level. Controls include: fixed effects (dummy indicators for Circuit and year); expectations (expected proportions of Democratic appointees on a given panel); and trends (Circuit-specific). Proportions during Circuit-years with no cases are defined to be 0. Panel D: GSS (1973-2004) weights are sampling weights. Individual-level controls are: age; gender; race; and college education. Panel E weights are population of state or reporting agency. State-level controls are: percent urban; infant mortality; percent age 15-19; percent age 20-24; percent nonwhite; police employment; unemployment rate; and real per capita income.

over the distributed lag, greatly strengthens the instrument and the 2SLS results. Likewise, were we to use the predicted estimate from the first stage as the instrument, we greatly increase the F-statistics. The first-stage becomes a lot stronger with predicted first stage as opposed to judge identity dummies (Kling 2006), while the identifying variation is the same (Evdokimov and Kolesár 2017).

Panel D shows that, after merging with the GSS and clustering standard errors (Bertrand et al. 2004), moving from an all-Republican panel to an all-Democrat panel increases the proportion of progressive decisions by roughly 60 percentage points in Column 6. We would expect similar point estimates with Panel C, if the number of individuals per Circuit is constant. Panel E shows similar patterns with the CDC data and UCR data.

U.S. Circuit Courts only hear cases with new legal issues that present an opportunity to provide a new definition or distinction on precedent and therefore shape judicial decisions. Therefore, we should not expect the assignment of judges in a previous year to predict the decisions in a subsequent year. Table III shows that the proportion of progressive precedents is not related to the assignment of Democrat judges to free speech panels in the one or two years before and after the true instrument.³² Since each instrument is affecting the corresponding contemporaneous endogenous variable, we will be isolating the causal effects of Law_{ct} in a distributed lag specification where we instrument for all lags and leads of Law_{ct} . Appendix Figures 5A and 5B illustrate the identification strategy. The jagged line displays N_{ct}/M_{ct} and the smooth line displays $\mathbf{E}(N_{ct}/M_{ct})$ for the entire U.S. in Figure 5A and for each of the 12 Circuits in Figure 5B. This figure illustrates how, since only a small fraction of Circuit Court cases involve free speech, there is substantial variation between the composition of the *assigned* judges and the composition of the pool of judges *available to be assigned*. Appendix Figure 7A shows the first stage relationship, that is, the relationship between the assigned N_{ct}/M_{ct} and the proportion of progressive decisions. Appendix Figure 7B shows the placebo relationship between $\mathbf{E}(N_{ct}/M_{ct})$ and the proportion of progressive

³²These specifications are analogous to the ones in Table II, Panel C, Column 6. There is a small loss in data due to lags and leads of judicial assignments being outside the data range.

TABLE II
FIRST STAGE: RELATIONSHIP BETWEEN PROGRESSIVE FREE SPEECH JURISPRUDENCE AND
DEMOCRATIC APPOINTEES ON APPELLATE FREE SPEECH PANELS, 1958-2008

Panel A: Judge Level	Outcome: Progressive Free Speech Vote					
	(1)	(2)	(3)	(4)		
Democratic Appointee	0.0983+	0.113**	0.0947+	0.102**		
	(0.0474)	(0.0348)	(0.0446)	(0.0316)		
N	525	525	525	525		
R-sq	0.010	0.288	0.011	0.292		
F-statistic of instrument	4.310	10.564	4.511	10.470		
Circuit-year controls	N	Fixed Effects	Expectations	Both		
Panel B: Case Level	Outcome: Progressive Free Speech Decision					
	(1)	(2)	(3)	(4)		
Democratic Appointees per Seat	0.162	0.296*	0.177	0.257*		
	(0.0979)	(0.114)	(0.104)	(0.113)		
N	175	175	175	175		
R-sq	0.009	0.315	0.010	0.317		
F-statistic of instrument	2.732	6.738	2.875	5.188		
Circuit-year controls	N	Fixed Effects	Expectations	Both		
Panel C: Circuit-Year Level	Outcome: % Progressive Free Speech Decisions					
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Appointees per Seat	0.336*	0.336*	0.355**	0.357**	0.362**	0.357**
	(0.130)	(0.129)	(0.113)	(0.110)	(0.115)	(0.111)
N	124	612	612	612	612	612
R-sq	0.043	0.365	0.427	0.427	0.436	0.437
F-statistic of instrument	6.726	6.759	9.893	10.480	9.963	10.411
Circuit-years with no cases	Dropped	Dummied	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	N	N	Fixed Effects	FE, Expect	FE, Trends	All
Panel D: Circuit-Year Level (Merged with Individual-Level GSS Data)	Outcome: % Progressive Free Speech Decisions					
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Appointees per Seat	0.529*	0.529*	0.530**	0.589**	0.590**	0.588**
	(0.231)	(0.230)	(0.168)	(0.163)	(0.163)	(0.164)
N	11777	44897	44897	44897	44613	44613
R-sq	0.107	0.366	0.494	0.521	0.521	0.520
F-statistic of instruments	5.244	5.288	9.992	13.072	13.137	12.912
Circuit-years with no cases	Dropped	Dummied	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	N	N	Fixed Effects	All	All	All
Individual controls	N	N	N	N	Y	Y, weighted
Panel E: Circuit-Year Level (Merged with State-Level CDC/UCR Data)	Outcome: % Progressive Free Speech Decisions					
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Appointees per Seat	0.344*	0.336*	0.359*	0.393**	0.332*	0.589**
	(0.149)	(0.130)	(0.131)	(0.110)	(0.125)	(0.168)
N	2193	2193	2193	2192	94137	71979
R-sq	0.386	0.444	0.454	0.483	0.464	0.527
F-statistic of instruments	5.347	6.635	7.516	12.797	7.042	12.335
Circuit-years with no cases	Dummied	Dummied	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	N	Fixed Effects	All	All	All	All
State-year controls	N	N	N	weighted	weighted	Y, weighted
Time Frame		CDC 1963-1980; 1984-2008			UCR 1977-2007	

Notes: Significant at +10%; *5%; **1%. Additional table notes in text.

TABLE III

PLACEBO INSTRUMENT: RELATIONSHIP BETWEEN PROGRESSIVE FREE SPEECH JURISPRUDENCE AND COMPOSITION OF FREE SPEECH PANELS IN OTHER YEARS, 1979-2004

Circuit-Year Level	Outcome: Proportion of Progressive Free Speech Decisions _t			
	(1)	(2)	(3)	(4)
Democratic Appointees per Seat _t	0.335* (0.125)	0.326* (0.129)	0.362** (0.110)	0.361** (0.108)
Democratic Appointees per Seat _{t-1}	-0.129 (0.0977)	-0.137 (0.100)		
Democratic Appointees per Seat _{t-2}		-0.0526 (0.0886)		
Democratic Appointees per Seat _{t+1}			-0.0917 (0.0865)	-0.0753 (0.0944)
Democratic Appointees per Seat _{t+2}				0.160 (0.101)
N	600	588	600	588
R-sq	0.436	0.438	0.444	0.452
Circuit-years with no cases	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	All	All	All	All

Notes: Significant at +10%; *5%; **1%. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the Circuit level. Proportions of progressive free speech jurisprudence and judicial type per seat during Circuit-years with no cases are defined to be 0 and dummied out. Circuit-year controls also include Circuit fixed effects, year fixed effects, Circuit-specific time trends, and expected Democratic Appointees per seat.

decisions.³³

We also employed LASSO to select biographical features as instruments for Law_{ct} (Belloni et al. 2012), and the results are similar. The F statistics increase up to 104 for the GSS.³⁴ We find that non-religious judges, Democrats, and attendance at elite schools were important

³³Appendix Figure 7A presents nonparametric local polynomial estimates of the first stage. Estimation proceeds in two steps. In the first step, we regress the proportion of decisions that were progressive on Circuit and year fixed effects and we regress the instrument, p_{ct} , on the same. Next, we take the residuals from these two regressions and use a nonparametric local polynomial estimator to characterize the relationship between the instrument and progressive decisions. As placebo, Appendix Figure 7B shows that there is no relationship between the proportion of Democrat judges $\mathbf{E}(p_{ct})$ in the Circuit-year and the proportion of progressive decisions.

³⁴The thirty biographical characteristics we collected are: Democrat; male; male Democrat; female Republican; non-White; Black; Jewish; Catholic; No religion; Mainline Protestant; Evangelical; BA received from same state of appointment; BA from a public institution; JD from a public institution; having an LLM or SJD; elevated from District Court; born in the 1910s, 1920s, 1930s, 1940s, 1950s; appointed when president and congress majority were from the same party; ABA score; above median wealth; appointed by president from an opposing party; prior federal judiciary experience; prior law professor; prior government experience; previous assistant U.S. attorney; and previous U.S. attorney. Adding panel-level interactions (e.g., fraction of judge seats assigned to Democrats multiplied by fraction of judge seats assigned to Blacks) yielded a total of 450 possible instruments. At the Circuit-year level, the LASSO procedure selected the following three instruments: the interaction between the number of male Democrats per seat and the number of judges born in the 1920s per seat; the interaction between the number of female Republican per seat and the number of judges having an LLM or SJD per seat; and the interaction between the number of female Republican per seat and the number of judges with above median wealth per seat.

predictors of progressive free speech precedents. In our results, we report estimates using just the Democrat instrument or the instruments selected by LASSO, which is the preferred specification. Earlier drafts also presented a “visual Hausman” test (displaying the distribution of 2SLS estimates using individual IV that were strong in first stage), suggesting the estimates are not due to the LASSO selection of unusual instruments. Using alternative sets of instruments suggests that causal effects of Law_{ct} remain similar.

5 Estimating the Impact of Obscenity Law

5.1 Attitudes and Behavior Table IV shows that progressive obscenity precedent increases acceptability of extramarital sex, premarital sex, and homosexual sex. The table present summaries of the full specification with distributed lags presented in the online appendix. The average lag effect is the average of each of the individual lags. Each lag Law_{ct} is defined as the Proportion of Progressive Decisions. For exposition, one can think of a decision that is progressive or conservative. The point estimates in Column 4 show that, on average, four years after progressive decisions, individuals are 0.8 percentage points more likely to say extramarital sex is OK, 1.4 percentage points more likely to say premarital sex is OK, and 0.3 percentage points more likely to say homosexual sex is OK. The lags are jointly significant. These are a small relative to the mean dependent variable displayed in Column 6. To put this estimate in perspective, Aksoy, Carpenter, De Haas, and Tran (2020) shows the roll-out of same-sex relationship recognition policies is associated with significant improvements in attitudes towards sexual minorities in the European Social Survey. The introduction of a relationship recognition law for same-sex couples is associated with a statistically significant 3.5 percentage point increase in the likelihood that a respondent agreed that gay men and lesbians should be free to live their own life as they wish. This effect is about five percent of the baseline average. These results mean that the adoption of expanded relationship recognition policies for same-sex couples can explain 35 percent of the ten-percentage point increase over our sample period in the share of adults agreeing that gay men and lesbians should be free to live their own life as they wish. Abou-Chadi and Finnigan (2019), using the same setting, finds that the probability of strong agreement is 3.1 percentage points higher after

TABLE IV
THE EFFECT OF FREE SPEECH JURISPRUDENCE ON ATTITUDES

<i>Average Lag effect</i>	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	LASSO IV (4)	Obs (5)	Mean Dependent Variable (6)
Extramarital Sex is OK	0.005	0.001	-0.027	0.008	18874	0.097
Joint P-value of lags	0.002	0.001	0.639	0.001		
Joint P-value of leads	0.936	0.968	0.576	0.315		
Premarital Sex is OK	0.000	-0.057	0.047	0.014	18801	0.633
Joint P-value of lags	0.126	0.666	0.815	0.000		
Joint P-value of leads	0.041	0.174	0.949	0.307		
Homosexual Sex is OK	0.001	0.017	-0.043	0.003	18073	0.267
Joint P-value of lags	0.805	0.000	0.574	0.000		
Joint P-value of leads	0.810	0.228	0.732	0.510		

Notes: Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instrument for proportion of progressive free speech jurisprudence is Democratic appointees per seat assigned to appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

the passing of same-sex marriage legalization. Redman (2018), using the World Value Survey, finds that as a country passes a new level of same-sex partnership recognition, surveyed attitudes indicating that homosexuality can be justified increases by 0.28. In countries that have not passed any legislation believing in the frequency of justification is 8.36, which increases to 8.64 with recognition of civil unions, and again to 8.92 with same-sex marriage.

Table IV shows that progressive obscenity precedent increases the likelihood of paid sex by 0.4 percentage points, the number of sexual partners per year reported by men by 0.28, the number of female partners reported by men by 11.3, the likelihood of extramarital sex reported by men by 0.7 percentage points, and the likelihood of being divorced or separated if older than 40 by 1.1 percentage points. The last outcome offers a contrast, since individuals younger than 40 are less likely to be divorced or separated by 3.9 percentage points. Some of these outcomes are also stock variables and may reflect the willingness to report or exaggerate, but this is also a relevant social norm.

5.2 Crime Turning to crime, we study a concrete outcome that has motivated policymakers. We begin with the UCR. Arrest data may reflect people’s willingness to come forward

TABLE V.— The Effect of Free Speech Jurisprudence on Behavior

<i>Average Lag effect</i>	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	LASSO IV (4)	Obs (5)	Mean Dependent Variable (6)
Paid Sex	0.003	0.006	0.006	0.004	16659	0.003
Joint P-value of lags	0.022	0.075	0.100	0.001		
Joint P-value of leads	0.434	0.789	0.247	0.263		
# Partners per Year	0.066	0.517	0.193	0.132	15346	1.129
Joint P-value of lags	0.348	0.001	0.000	0.181		
Joint P-value of leads	0.306	0.598	0.014	0.477		
# Female Partners	2.450	1.252	5.292	5.028	13833	6.296
Joint P-value of lags	0.095	0.961	0.000	0.000		
Joint P-value of leads	0.881	0.791	0.725	0.347		
# Partners per Year (reported by Men)	0.134	1.453	0.193	0.278	6626	1.421
Joint P-value of lags	0.095	0.581	0.000	0.017		
Joint P-value of leads	0.662	0.153	0.042	0.894		
# Female Partners (reported by Men)	5.730	7.366	12.756	11.342	6077	14.041
Joint P-value of lags	0.001	0.049	0.000	0.000		
Joint P-value of leads	0.709	0.341	0.514	0.514		
Extramarital Sex (reported by Men)	0.056	0.113	0.048	0.069	7170	0.161
Joint P-value of lags	0.014	0.968	0.000	0.003		
Joint P-value of leads	0.635	0.801	0.966	0.437		
Divorced or Separated (older than 40)	0.009	0.043	0.028	0.011	10778	0.237
Joint P-value of lags	0.460	0.674	0.000	0.008		
Joint P-value of leads	0.157	0.370	0.301	0.496		
Divorced or Separated (40 or younger)	-0.020	0.027	-0.084	-0.039	6368	0.174
Joint P-value of lags	0.060	0.123	0.000	0.003		
Joint P-value of leads	0.053	0.534	0.425	0.216		

Notes: Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instrument for proportion of progressive free speech jurisprudence is Democratic appointees per seat assigned to appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

to report a crime, law enforcement’s openness to investigating crimes, or local community leaders making people aware of what constitutes a crime. The data are susceptible to under-reporting, particularly by victims in sex-related crimes. However, the UCR and GSS record prostitution and (self-reported) paid sex, which mirror each other. The UCR can be viewed as audits of self-reported behavior.

Table VI shows that progressive obscenity precedent increased prostitution (by 3 arrests per 100,000 population) and drug violations (by 36 arrests per 100,000 population), but decreased child abuse (by 56 arrests per 100,000 population). While incidence of prostitution and child abuse may substitute according to some empirical studies (Ciacci and Sviatschi 2019), the incidence of property crime likely does not, so we can use property crime as a placebo outcome. In fact, no discernible effect is found on property crime. The lead effects are always insignificant.

Table VII presents a series of robustness checks on the child abuse results to show that the research design addresses omitted variables and reverse causality. The results are robust to the removal of Circuit-specific time trends, clustering standard errors at the state level, removing state-level controls, removing population weights, removing community standards, dropping 1 Circuit at a time, and varying the distributed lag structure. Effects arise one year after a precedent, but are the largest two years later. Notably, the lead effects are individually and jointly insignificant in the final row.

To illustrate the magnitudes of our estimates, we emulate Bhuller et al. (2013)’s study in showing the actual time trends for various crime outcomes, as well as the predicted counterfactual time trends in the absence of internet broadband. Figure 1 presents a graphical analysis of the counterfactual in the absence of obscenity law. The solid line is the actual crime rate and the dashed line is the counterfactual crime rate, which is the actual crime rate minus the predicted effect of obscenity law on crime. Going clockwise from the upper-left, the graphs report counterfactuals for prostitution, drug violations, forcible rapes, and property crime. The impact on property crimes (a placebo) is imperceptible. Other counterfactual calculations are presented in Appendix E.

TABLE VI
THE EFFECT OF FREE SPEECH JURISPRUDENCE ON CRIME

<i>Average Lag effect</i>	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	LASSO IV (4)	Obs (5)	Mean Dependent Variable (6)
Offenses Against Family and Children	-11.002	-44.588	-47.575	-56.475	43992	46.063
Joint P-value of lags	0.422	0.000	0.000	0.001		
Joint P-value of leads	0.170	0.201	0.418	0.985		
Community Vices	1.309	9.641	8.620	2.998	43992	5.104
Joint P-value of lags	0.094	0.000	0.000	0.081		
Joint P-value of leads	0.229	0.096	0.737	0.381		
Drug Violations	30.956	69.391	90.613	35.542	43992	286.987
Joint P-value of lags	0.038	0.002	0.000	0.002		
Joint P-value of leads	0.594	0.148	0.633	0.750		
Forcible Rapes	-0.413	4.614	2.609	2.190	67017	10.044
Joint P-value of lags	0.367	0.268	0.103	0.268		
Joint P-value of leads	0.097	0.154	0.833	0.885		
Property Crimes	-17.811	-59.631	-98.440	-96.232	67017	559.876
Joint P-value of lags	0.205	0.438	0.241	0.769		
Joint P-value of leads	0.118	0.481	0.648	0.598		

Notes: Data consist of UCR arrests reported by ORI agencies (at the state-county level). All crime numbers are per 100,000 population. Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and state controls: percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income. Instrument for proportion of progressive free speech jurisprudence is Democratic appointees per seat assigned to appellate free speech cases in a Circuit-year. Population weights are population reporting to ORI agency.

TABLE VII
IMPACT OF PROGRESSIVE FREE SPEECH PRECEDENT ON CHILD ABUSE
ROBUSTNESS OF 2SLS DISTRIBUTED LAG ESTIMATES

The Effect of Appellate Free Speech Precedent on Offenses Against Family and Children per 100,000										
	(t0)		(t1)		(t2)		(t3)		(t4)	(t5)
No Trends	-91.353 (64.462)		-81.141 (45.029)	+	-94.558 (38.112)	*	-75.751 (44.801)		-65.686 (54.096)	
No FE	-82.056 (60.700)		-78.434 (62.034)		-75.302 (48.448)		-46.958 (36.288)		-33.439 (27.757)	
State Cluster	-56.888 (36.520)		-51.841 (38.504)		-69.982 (37.600)	+	-55.258 (37.435)		-33.322 (41.573)	
No Ind Control	-101.894 (121.993)		-80.435 (83.931)		-117.014 (117.420)		-90.922 (123.947)		-65.367 (122.816)	
No Weights	-13.422 (13.066)		-16.093 (12.059)		-36.758 (6.881)	**	-38.544 (10.626)		-15.718 (11.695)	
No Community Standards	-58.394 (32.994)	+	-51.890 (15.079)	**	-70.319 (7.617)	**	-55.459 (10.225)	+	-33.165 (18.893)	+
No Controls except 1[$M_{ct}>0$]	-226.714 (259.576)		-191.154 (243.387)		-201.168 (224.136)		-109.214 (155.064)		-97.769 (126.684)	
Drop Circuit 1	-79.711 (56.486)		-63.593 (32.739)	+	-83.160 (17.712)	**	-64.068 (20.529)		-39.174 (21.009)	+
Drop Circuit 2	-59.057 (32.773)	+	-53.648 (15.847)	**	-69.657 (8.054)	**	-57.449 (15.537)	+	-30.632 (18.628)	
Drop Circuit 3	-51.053 (23.966)	*	-42.069 (9.930)	**	-68.778 (5.019)	**	-48.348 (7.475)	*	-51.910 (10.390)	**
Drop Circuit 4	-53.679 (35.170)		-50.913 (18.408)	**	-68.941 (7.055)	**	-52.930 (10.221)		-39.347 (16.099)	*
Drop Circuit 5	-62.407 (38.628)		-52.638 (18.477)	**	-66.414 (8.788)	**	-56.349 (16.076)		-25.557 (20.075)	
Drop Circuit 6	-4.340 (18.612)		-3.666 (15.229)		-31.343 (24.071)		-46.655 (33.380)		-24.286 (36.556)	
Drop Circuit 7	-60.410 (44.221)		-60.801 (24.821)	*	-77.127 (10.951)	**	-58.833 (20.536)		-37.586 (36.401)	
Drop Circuit 8	-8.701 (35.268)		-6.972 (20.811)		-16.677 (17.162)		-21.846 (13.570)		7.046 (15.235)	
Drop Circuit 9	-87.683 (64.317)		-102.192 (115.462)		-96.512 (16.615)	**	-75.410 (68.031)		-48.865 (56.414)	
Drop Circuit 10	-56.827 (35.172)		-52.147 (17.691)	**	-70.156 (7.426)	**	-56.426 (12.664)		-35.038 (17.195)	*
Drop Circuit 11	-49.149 (26.377)	+	-52.186 (15.151)	**	-70.039 (8.674)	**	-50.317 (9.769)	+	-31.980 (17.630)	+
Drop Circuit 12	-56.888 (32.379)	+	-51.841 (15.681)	**	-69.982 (6.784)	**	-55.258 (10.742)	+	-33.322 (18.044)	+
1 current 1 lag	3.662 (9.083)		-21.926 (13.151)	+						
1 current 2 lag	-3.711 (13.626)		-28.316 (10.936)	**	-32.645 (17.248)	+				
2 leads 4 lags	-56.447 (43.201)		-63.901 (27.651)	*	-84.808 (58.359)		-69.766 (44.716)		-52.605 (72.366)	
1 lead 5 lags	-51.692 (30.496)	+	-53.219 (14.185)	**	-70.399 (4.493)	**	-53.089 (12.023)	+	-27.914 (18.456)	-18.82 (22.167)
4 leads 1 lag	20.923 (20.030)		-6.330 (21.678)		-13.216 (25.401)		-24.437 (53.931)		30.848 (27.848)	3.625 (32.504)

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are population reporting to ORI agency.

FIGURE 1.— What if these legal precedents did not exist?

The majority of laboratory experiments find support for secondary effects (Donnerstein and Linz 1986; Allen et al. 1995; Zillman and Bryant 1984) concerning endangerment of women (Radin 1996; MacKinnon 1987).³⁵ Bhuller et al. (2013) and Baron and Straus (1984) report a link to sex crimes.³⁶ These findings suggest that the increased consumption of obscene content increased sex-related crimes.

5.3 Disease The spread of venereal diseases, which have been mentioned as a secondary effect justifying obscenity regulation, may indicate riskier sexual practices (Nelson and Williams 2007). Table VIII shows that progressive obscenity precedent increased incidence of

³⁵Most studies find that pornography, especially violent pornography, increases sexual aggression (Donnerstein and Linz 1986; Allen et al. 1995), though some experiments find no effect or a reduction in sexual aggression after exposure to pornography (see, e.g., Zillman and Bryant (1984)).

³⁶Baron and Straus (1984) find a strong positive association between the circulation of eight pornographic magazines across U.S. states and crime, after controlling for a number of possible confounders.

TABLE VIII
THE EFFECT OF FREE SPEECH JURISPRUDENCE ON DISEASE

<i>Average Lag effect</i>	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	LASSO IV (4)	Obs (5)	Mean Dependent Variable (6)
Chlamydia	13.029	87.392	74.130	49.636	1117	207.509
Joint P-value of lags	0.014	0.000	0.979	0.000		
Joint P-value of leads	0.435	0.299	0.755	0.501		
Gonorrhea	13.367	40.036	221.957	186.113	2141	243.911
Joint P-value of lags	0.404	0.263	0.987	0.980		
Joint P-value of leads	0.842	0.368	0.900	0.888		
Syphilis	-3.601	-0.243	1.853	0.681	2141	6.748
Joint P-value of lags	0.172	0.946	0.598	0.756		
Joint P-value of leads	0.906	0.609	0.599	0.562		

Notes: Data on STD incidence reported by CDC (at the state level). Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, and a dummy for whether there were any cases in that Circuit-year. Instrument for proportion of progressive free speech jurisprudence is Democratic appointees per seat assigned to appellate free speech cases in a Circuit-year. Population weights are state population.

chlamydia by 50 (incidents per state per year), but did not significantly increase gonorrhea or syphilis. Chlamydia, known as the “silent” disease, typically produces no symptoms for several years, and is the fastest increasing in recent years among these STDs. In one study, 86% of the infected partners of infected women were also found to be asymptomatic (Fish et al. 1989).³⁷ The differential results are not due to differences in screening by public health officials, since screening for different STDs typically occurs simultaneously. The differential results are more likely to be related to sorting or screening sexual partners based on their disease status, a mechanism suggested by Kremer (1996).³⁸

5.4 Deterrence Pornography media providers were often parties in free speech litigation. *Playboy* and *Penthouse* were competitors at the boundaries of community standards through the 1970s. We found weak to no evidence of any impact of free speech decisions on magazine

³⁷In contrast, about 90% of men infected with gonorrhea display symptoms within days of infection, and 40-70% of infected women have symptoms within 10 days (Kretzschmar et al. 1996). Syphilis symptoms include sores within 10 to 90 days and rashes within 1 to 6 months of the primary infection.

³⁸Condom use does not differentially affect transmission rates across the three STD types (Holmes et al. 2004).

circulation. We emphasize that we evaluate the effects of obscenity law rather than pornography itself. Since Bhuller et al. (2013) found that internet broadband increased child abuse, whereas we find the opposite result, this difference is consistent with deterrence not being the sole mediator for our effects. Deterrence would mean a progressive precedent makes obscene content more accessible, which would increase child abuse according to Bhuller et al. (2013). If progressive precedent issues a signal that increases paid or extramarital sex, this can substitute for child abuse (Ciacci and Sviatschi 2019).

6 Newsreports and Mechanism Experiment

To support the causal channel from court rulings to preferences, previous studies have linked Supreme Court precedents with subsequent changes in public opinions about abortion (Franklin and Kosaki 1989). Hoekstra (2000) suggests that local media are more likely to report on cases in their community and that local residents are more likely to be aware of those cases than cases in other jurisdictions. The salience of obscenity law, in particular, was potentially even greater during the 1960s, which is suggested by the large number of law review articles written in response to obscenity decisions during that time period (Kalven 1960; Magrath 1966; 1960). This salience is echoed even since 1979 in our simple search of newspaper data detailed previously. Appendix Figure 3 displays a plot correlating the number of obscenity decisions and the number of newspaper articles about obscenity decisions from 1979 to 2008.³⁹ The correlation is statistically significant, and remains so with Circuit and year fixed effects. The newspaper database does not exist before 1979, but given the emotional salience of these Circuit rulings we have no reason to believe they would have gone unreported.

We then implement a mechanism experiment (Ludwig et al. 2011). Djourelova (2020))

³⁹ Not every newspaper is available for every year, so we divide the number of newspaper articles by the proportion of newspapers available (e.g., if only half of the typical newspaper coverage is available because of data limitations, we multiply by a factor of two to make a consistent series in the figure). This allows us to compare graphically the number of Circuit decisions and newspaper articles about obscenity over time.

documents that media can have immediate and persistent effects, even on policy support. We randomly expose data entry workers to transcribe newsreports of obscenity decisions and assess their attitudes and behaviors using the same questions as in the General Social Survey. We hired 1,345 workers across three replications. Additional details of the first two replications are in Appendix F. We see marginally significant effects on acceptability of homosexual sex as worded in the General Social Survey and echoing the population-based analysis. We report the results of each experiment separately. The pooled results would be stronger.

The experiments also show that self-reported behaviors did not shift in response to progressive free speech precedents. The short timeframe of the study precludes actual behaviors from changing. The null result suggests that self-reporting norms are unlikely to explain the results in the population-based analyses. Moreover, exposure to conservative obscenity precedents increased perceived prevalence of extramarital sex by 2.5 percentage points. This result verifies the information channel in the law and norms theory: when legal authorities increase sanctions against a particular activity, people infer that more people are engaged in this activity. The third replication is reported in

Chen and Yeh (2014), which finds that liberal obscenity decisions liberalize individual and perceived community standards and increase utility. Yet religious workers become more conservative in their values, identify as more Republican, view community standards as becoming more liberal, and report lower utility. Workers update beliefs about the prevalence of sexual activities differently in response to liberal or conservative decisions.

7 Backlash then Expressive

The theory of law and norms (Bénabou and Tirole 2012) suggests that backlash should occur when relatively few individuals engage in law’s sanctioned activities, whereas expressive law should occur when it is the norm. Put differently, liberal laws (further) liberalize attitudes when the underlying activity is already the norm, but liberal laws create backlash when the underlying activity is rare.

Sexual norms have changed dramatically since 1958. Fernandez-Villaverde et al. (2014) note several stylized facts. In 1958, 35% of U.S. women engaged in premarital sex by the age of 19 compared to 75% today. In 1968, only 15% of women viewed premarital sex as acceptable, but by 1983 this increased to 45%. In 1957, 57% of Americans believed that adults who preferred to be single were “immoral,” but today, being single is no longer considered a moral issue and more than 50% of adults are single. Bearing children out-of-wedlock was once extremely rare, but today more than half of births to women under 30 occur outside of marriage (Klinenberg 2012). This is true especially in the U.S. South.⁴⁰

Interpreting those facts through the lens of the theory yields predictions for heterogeneous effects over time. Early conservative precedents cause people to update their beliefs that the sanctioned activities are more common than previously thought. Normalizing the sanctioned activity undermines the initial purpose of the conservative precedent, which the theory calls “backlash.” In the aftermath of the sexual revolution, progressive free speech decisions have expressive effects, where the informational effects and the material penalties reinforce each other.

Table IX presents analyses of GSS and UCR for 1973-1993 vs. 1980-2000.⁴¹ We confirm that first stage F-statistics remain high for the two time periods. Column 2 suggests backlash effects in the earlier time period. Paid sex, prostitution, partners per year, and acceptability of homosexual sex all increase following conservative free speech precedent. The opposite is true in later years. Note that self-reports of paid sex and arrests for prostitution move in tandem.

⁴⁰<https://www.cdc.gov/nchs/pressroom/sosmap/unmarried/unmarried.htm>

⁴¹The results are robust to variation in these cutoffs.

TABLE IX
THE EFFECTS OF FREE SPEECH PRECEDENTS OVER TIME

	1973-1993		1980-2000	
	OLS	Appellate IV	OLS	Appellate IV
<i>Average Lag effect</i>	(1)	(2)	(3)	(4)
Paid Sex	0.004	-0.002	0.003	0.005
Joint P-value of lags	0.083	0.000	0.036	0.123
Joint P-value of leads	0.643	0.217	0.514	0.824
Community Vices	7.463	-2.050	1.364	9.181
Joint P-value of lags	0.108	0.000	0.056	0.050
Joint P-value of leads	0.074	0.724	0.240	0.089
Partners Per Year	-0.724	-0.169	0.043	0.468
Joint P-value of lags	0.101	0.047	0.348	0.031
Joint P-value of leads	0.057	0.242	0.535	0.601
Homosexual Sex is OK	-0.003	-0.050	0.001	0.017
Joint P-value of lags	0.394	0.008	0.771	0.000
Joint P-value of leads	0.018	0.680	0.783	0.227

Notes: Significant at +10%, *5%, **1%. Attitudinal and behavioral data consist of individual GSS responses. Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to appellate obscenity cases in a Circuit-year. Survey weights are provided by GSS. Crime data consist of UCR arrests reported by ORI agencies (at the state-county level) and population weights are population reporting to ORI agency.

8 Conclusion

Social scientists and philosophers have long debated whether law shapes values. Judges recognize the possibility that laws can have effects through the moral messages that they convey. We bring causal analysis of the impact of law on norms. Our theoretical framework allows for both backlash and expressive effects to occur, depending on the underlying distribution of law’s sanctioned activity.

Using data on all U.S. obscenity precedent in Courts of Appeals, we show that Democrats decide free speech cases in a manner more closely linked to prioritizing individual self-expression, and that they vote to protect free speech. Republicans decide cases in a manner more closely linked to a focus on secondary effects, and they vote to constrain free speech. Through the quasi-randomization of rulings from judge assignment, we find that prioritizing individual self-expression increased the value and exercise of free speech rights. Relative to conservative free speech precedent, progressive precedent was associated with more progressive attitudes and behaviors on non-marital sexual activity, prostitution, and drug violations. Likewise, decisions that focus on secondary effects reduced crime with the exception of child abuse. They also reduced STDs, particularly chlamydia, which is often asymptomatic. Finally, conservative court precedents increased the perceived prevalence of extramarital sex, a key mechanism for the model of law and norms (Bénabou and Tirole 2012).

The research can be extended in a number of directions. Methodologically, the twinned experimental and empirical framework developed here and in other papers helps distinguish deterrence from information channels for the causal effects of law. We hope it proves fruitful for policy-makers and judges interested in assessing the impact of court-made law, as well as for scholars and theorists interested in evaluating theories of behavioral responses to the law.

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Web Appendix:

Appendix Figure 1: Distribution of Intrinsic Motivations

Appendix Figure 2: Map of U.S. Circuit Courts

Appendix Figure 3: Number of obscenity decisions and the number of newspaper articles about obscenity decisions from 1979 to 2008

Notes: We collated mentions of Courts of Appeals decisions in articles from the major newspaper for the city in which each Circuit Court resides. These are: *The Boston Globe*, *New York Times*, *Philadelphia Inquirer*, *Richmond Times Dispatch*, *Times-Picayune*, *Cincinnati Post*, *Chicago Tribune*, *St. Louis Post-Dispatch*, *San Francisco Chronicle*, *Denver Post*, *Atlanta Journal and Constitution*, and *The Washington Post*. We collected data from 1979 to 2008 from NewsBank using the search term: (obscen*) w/100 (judgment OR "court ruling") AND Circuit AND NOT "Supreme Court". Not every newspaper is available for every year, so we divide the number of newspaper articles by the proportion of newspapers available (e.g., if only half of the typical newspaper coverage is available because of data limitations, we multiply by a factor of two to make a consistent series in the figure).

Appendix Figure 4: Appellate Obscenity Decisions, 1958-2008

Notes: Quantity of obscenity cases that were decided progressively or conservatively over time.

Appendix Figure 5A: Illustration of Identification Strategy

Notes: The jagged line displays the realized proportion of seats in obscenity cases assigned to Democratic Appointees. The smooth line displays the expected proportion of seats in obscenity cases assigned to Democratic Appointees. Since only a small fraction of Circuit Court cases involve obscenity, there is substantial variation between the composition of the *assigned* judges and the composition of the pool of judges *available to be assigned*.

Appendix Figure 5B: Illustration of Identification Strategy by Circuit

Notes: For each Circuit, the jagged line displays the realized proportion of seats in obscenity cases assigned to Democratic Appointees. For each Circuit, the smooth line displays the expected proportion of seats in obscenity cases assigned to Democratic Appointees. Since only a small fraction of Circuit Court cases involve obscenity, there is substantial variation between the composition of the *assigned* judges and the composition of the pool of judges *available to be* assigned in each Circuit.

Appendix Figure 6: Randomization Check - P-Values of Democrat Appointee random strings test.

Notes: We assess deviations from random assignment by examining whether the sequence of proportions of judges is similar to a random process. With a truly random process, the collection of all Circuit p-values for autocorrelation, mean reversion, and maximum runs relative to 1000 randomly generated strings should be uniformly distributed.

Appendix Figure 7A

Appendix Figure 7B

Notes: Appendix Figure 7A presents nonparametric local polynomial estimates of the first stage between the actual proportion of Democrat judges and the proportion of progressive decisions. Nonparametric local polynomial estimates are computed using an Epanechnikov kernel. Rule-of-thumb bandwidth is used. Shaded area indicates 90 percent confidence bands. The residuals are calculated removing circuit and year fixed effects. As placebo, Appendix Figure 7B shows that there is no relationship between the expected proportion of Democrat judges and the proportion of progressive decisions.

A Theory

A.1 Model We present a simplified version of Bénabou and Tirole (2012). The model assumes three motivations for human behavior: (1) intrinsic motivations, where people perform an action simply because they believe it is the right thing to do; (2) extrinsic motivations, where material incentives and deterrence influence actions; and (3) social motivations, where values, norms, social sanctions provided by society affect actions. People accrue honor or stigma for actions outside the norm. For example, if very few people use drugs, then drug users receive stigma; if very few people donate millions, then donors receive honor. Information is conveyed by legal decisions on the norms—the distribution of actions in the community. Two different views of free speech emerge: (1) law shifts social motivations towards what the law values, i.e., it reinforces the deterrent effects provided by the sanction, or (2) law shifts social motivations away from what the law values, i.e., it undermines the law’s intention. As shorthand, we call the former an expressive effect and the latter, backlash.

Individuals maximize the following utility function:

$$U(a) = (v_a + y)a - C(a) + e\bar{a} + \mu E(x | a)_s$$

where v_a is intrinsic motivation (over the range of $[v, \bar{v}]$), y is extrinsic payoff, $C(a)$ is the cost of the action, $e\bar{a}$ is the public good aspect of the good, and μ is the positive weight agents put on social perceptions, $E(x | a)_s$, which is other people’s perception of the actor’s intrinsic motivations. Society uses a rule s to calculate their expectation of the actor’s intrinsic motivations based on his or her action a . In rational expectations equilibrium, society’s expectations will be correct and the last term will be $\mu E(v_a | a)$.

The principal—the social planner or judge—maximizes over the contract and y :

$$(1) \quad W(y) = f(\bar{U}(y) + (1 + \lambda)ya(y) + \sigma_j \bar{a})$$

The judge set the costs and $\sigma_j \bar{a}$ represents the systematic component of judge j ’s decision-making that leads them to value the public good $\bar{a}$ more or less than other judges. λ is the shadow cost of resources used as incentives, like enforcement costs.

Due to random assignment of judges with different σ_j , we have exogenous variation in y in our empirical application. So, we focus on the behavior of the agent.

In the simple example of two actions ($a = 0, 1$), the actor receives:

$$(2) \quad \begin{cases} \text{if } a = 1 : & U(1) = v_a + y - C(1) + e\bar{a} + \mu E(x | 1)_s \\ \text{if } a = 0 : & U(0) = -C(0) + e\bar{a} + \mu E(x | 0)_s \end{cases}$$

Exercising free speech rights corresponds to $a = 0$ and abstaining from free speech corresponds to $a = 1$. $e > 0$ captures judicial concerns that exercising free speech leads to some harm.

With two actions, the social perception of the actor's intrinsic motivations follows a cutoff rule. Normalize $c = C(1) - C(0) - y$, which is the extrinsic cost difference between the two actions; with ordinal utilities, we rewrite net utilities as:

$$(3) \quad \begin{cases} \text{if } a = 1 : & U(1) = v_a - c + \mu E(x | 1)_s \\ \text{if } a = 0 : & U(0) = \mu E(x | 0)_s \end{cases}$$

This expression provides a cutoff rule, since if a person chooses to take action $a = 1$ at some v_a , then the person also chooses $a = 1$ at any $v > v_a$, holding others' actions fixed in equilibrium. This is because the social motivation and the extrinsic motivation are fixed, while the intrinsic motivation increases. Thus the cutoff rule will satisfy:

$$(4) \quad v^* - c + \mu E(v_a | 1) = \mu E(v_a | 0)$$

The expression motivates a sufficient condition for a fixed point. The fixed point solves the equation:

$$(5) \quad v^* + \mu \Delta(v^*) = c$$

where we define:

$$(6) \quad \Delta(v) = E(v_a | v_a > v) - E(v_a | v_a < v)$$

At the cutoff value v , people choose action 1 if their v_a is bigger than v , and they choose action 0 if their v_a is smaller than v , so

$$(7) \quad \Delta(v) = E(v_a | 1) - E(v_a | 0)$$

A sufficient condition for a fixed point is if $1 + \mu \Delta'(v) > 0$, in which case $[\underline{v}, v^*]$ share of the population exercise free speech.

To understand this sufficient condition, note that $v^* + \mu \Delta(v^*)$ is the marginal benefit of exercising free speech for people at the cutoff. The marginal benefit is the sum of intrinsic motivation and social motivation. c is the marginal cost. The intuition for the sufficient condition is as follows. If $1 + \mu \Delta'(v) > 0$, then as the cut-off increases, the marginal benefit will eventually equal the marginal cost c , which is constant, and

that cutoff will be a fixed point. The more people who exercise free speech, the more honor associated with abstaining from free speech, which means the less others will exercise free speech. While $1 + \mu \Delta'(v) > 0$ is a sufficient condition for a fixed point, it is not a necessary condition. In particular, $\Delta'(v) < 0$ is possible, when a small perturbation leads to rapid social changes as society moves from one steady state to another.

See Appendix Figure 1 for a distribution of intrinsic motivations. Under Jewitt's lemma, the shape of Δ mirrors the density of v . Δ initially decreases, then increases. Intuitively, this is because adding a small mass around the cutoff will shift one truncated mean more than the other. When v^* is small (most people choose $a = 1$), raising v^* increases $E(v_a | 0)$ more than $E(v_a | 1)$, as $E(v_a | 0)$ includes very few points on the left tail of the v -distribution. Slightly increasing the support of the truncated distribution to the right adds a large share of individuals with high v 's. In contrast, $E(v_a | 1)$ is less affected.

In words, the more people who exercise free speech, the more normalized it becomes, so the more others will exercise free speech as well: $\Delta'(v) < 0$. Multiple equilibria can arise if complementarity is strong enough or μ is large enough. When $1 + \mu \Delta'(v)$ is negative, there may be unstable equilibria.

Explicit sanctions indicate that the judge sees a problem. The judge has information about v^* because of the *Miller* community standard test, which incentivizes litigants in an adversarial system to bring information on v^* to the judge. The judge issues a sanction when they believe v^* is too high. Upon observing the decision, community leaders and individuals update their beliefs about the underlying distribution. When exercise of free speech is common, v^* is on the right side of the distribution, so free speech decisions have expressive effects.

The model implies: (1) laws have expressive effects when v^* is high (the density of v is falling) and (2) laws have backlash effects when v^* is low (the density of v is increasing).

We map $\Delta(v)$ to the General Social Survey (GSS), where people respond to questions about the morality of particular actions. By reporting what is their perceived morality of an action, respondents report the difference in the social perception of someone who chooses $a = 1$ vs. the social perception of someone who chooses $a = 0$, which is a motivator for their action (behavior).

B Background on U.S. Obscenity Law

Major doctrinal developments in obscenity law include:⁴²

Regina v. Hicklin (1868, Eng) 3 QB 360. - “I think the test of obscenity is this, whether the tendency of the matter charged as obscene is to deprave and corrupt those whose minds are open to such immoral influences, and into whose hands a publication of this sort may fall.” Applied in the U.S. as illustrated in *Commonwealth v. Friede* 271 Mass 318, 171 NE 472 (1930).

United States v. One Book Entitled "Ulysses" 72 F2d 705 (1934, CA2 NY) - “We believe that the proper test of whether a given book is obscene is its dominant effect. In applying this test, relevancy of the objectionable parts to the theme, the established reputation of the work in the estimation of approved critics, if the book is modern, and the verdict of the past if it is ancient, are persuasive pieces of evidence; for works of art are not likely to sustain a high position with no better warrant for their existence than their obscene content.”

Roth v. United States 354 US 476, 1 L ed 2d 1498, 77 S Ct 1304 (1957) - "Obscene material is material which deals with sex in a manner appealing to prurient interest." The opinion also quoted with approval the test from Tentative Draft No 6 of the Model Penal Code, presented to the American Law Institute: A thing is obscene if, considered as a whole, its predominant appeal is to prurient interest, i.e., a shameful or morbid interest in nudity, sex, or excretion, and if it goes substantially beyond customary limits of candor in description or representation of such matters (expressly rejecting the *Hicklin* test).

Memoirs v. Massachusetts, 383 U.S. 413, 86 S.Ct. 975, 16 L.Ed.2d 1 (1966) - For a work to be considered obscene, three elements must coalesce: it must be established that (a) the dominant theme of the material taken as a whole appeals to a prurient interest in sex; (b) the material is patently offensive because it affronts contemporary community standards relating to the description or representation of sexual matters; and (c) the material is utterly without redeeming social value.

Miller v. California, 413 US 15, 93 S Ct 2607, 37 L Ed 2d 419 (1973) - The test to determine whether a work is obscene is (a) whether “the average person, applying contemporary community standards” would find that the work, taken as a whole, appeals to the prurient interest, (b) whether the work depicts or describes, in a patently offensive way, sexual conduct specifically defined by the applicable state law; and (c) whether the work, taken as a whole, lacks serious literary, artistic, political, or scientific value (rejecting “without redeeming social value” element of *Memoirs*).

The full list of precedents in our data frame are below:

⁴²We focus on decisions in federal appellate courts and not state courts, where the judges are not always randomly assigned. Appellate courts would decide on larger doctrinal issues setting new precedent.

Appendix Table I: List of Free Speech Appellate Precedent

Citation	Case Name	Circuit Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
Citation					
252 F.2d 333	Ginzman v. Schaffer	2	1958	0 prohibition on sending payment for obscene material through the mails	"obscene material"
262 F.2d 357	United States v. Padell	2	1958	0 prohibition on mailing obscene material	books containing "dirty pornography"
259 F.2d 54	United States v. Keller	3	1958	1 prohibition on mailing obscene material	postcards containing references to adultery
260 F.2d 670	Capitol Enterprises, Inc. v. Chicago	7	1958	1 Chicago, IL obscenity ordinance	sexually explicit film
271 F.2d 140	Alexander v. United States	8	1959	0 shipment of obscene materials via common carrier	sexually explicit books
273 F.2d 799	Flying Eagle Publications, Inc. v. United States	1	1960	1 prohibition on mailing obscene material	"lewd, lascivious, vile, indecent ..."—partially clothed illustration of a woman accompanying
283 F.2d 780	Collier v. United States	4	1960	0 prohibition on mailing obscene material	circuits telling where obscene material might be found; pornographic photographs
276 F.2d 433	Grove Press, Inc. v. Christenberry	2	1960	1 prohibition on mailing obscene material	Lady Chatterley's Lover—book containing explicit sex
274 F.2d 598	Cain v. United States	5	1960	0 prohibition on mailing obscene material	letter containing sexually explicit language
273 F.2d 529	Empire Pictures Distributing Co. v. Ft. Worth	5	1960	1 Fort Worth, TX city ordinance banning a movie theater from showing ex	pornographic films
277 F.2d 631	United States v. Hochman	7	1960	0 transportation of obscene materials in interstate commerce using a comm	sexually explicit books
294 F.2d 204	Womack v. United States	12	1961	0 mailing obscene matter	sexually explicit books
289 F.2d 455	Manual Enterprises, Inc. v. Day	12	1961	0 mailing obscene matter	pornographic ads
290 F.2d 517	United States v. Oakley	6	1961	0 prohibition on mailing obscene materials	pornographic magazines
293 F.2d 449	Ackerman v. United States	9	1961	0 mailing obscene material	pornographic photographs
309 F.2d 362	Excellent Publications, Inc. v. United States	1	1962	1 prohibition on mailing obscene material	sexually explicit letters
300 F.2d 78	Kahn v. United States	5	1962	0 prohibition on mailing obscene materials	photos of nude/partially nude women
316 F.2d 813	United States v. Darneil	2	1963	0 prohibition on mailing obscene material	pornographic written materials, advertisements for those materials
316 F.2d 873	United States v. Zuideweld	7	1963	0 prohibition on mailing obscene materials	private letter using swear words
338 F.2d 12	United States v. Ginzburg	3	1964	0 prohibition on mailing obscene material	sexually suggestive magazines, membership in a sexual pen pal club
333 F.2d 963	Outdoor American Corp. v. Philadelphia	3	1964	0 PA obscenity statute	pornographic magazine
350 F.2d 155	United States v. Klaw	2	1965	1 prohibition on mailing obscene material	pornographic magazines
353 F.2d 614	United States v. Davis	2	1965	1 prohibition on mailing obscene material	pornographic illustrations
340 F.2d 59	Haldean v. United States	10	1965	1 mailing obscene matter	records and record labels which depicted sex in some way
358 F.2d 935	Books, Inc. v. United States	1	1966	0 prohibition on mailing obscene material	sexually explicit pamphlets and advertising
359 F.2d 402	Wenzler v. Pictress	9	1966	0 prohibition on mailing obscene material	book explicitly describing characters' sexual adventures
357 F.2d 885	United States v. West Coast News Co.	6	1966	0 CA obscenity law	pornographic videos
367 F.2d 889	United States v. One Carlton Positive Motion Picture Film	2	1966	0 prohibition on mailing obscene material	sexually explicit book
385 F.2d 209	Culbertson v. California	9	1967	1 prohibition on importation of obscene material	sexually explicit film which also depicts self-mutilation
373 F.2d 635	United States v. 56 Cartons	9	1967	1 CA statute prohibiting sale of obscene material	photographs of "scantly clad women"
373 F.2d 633	United States v. 392 Copies of Magazine	4	1967	0 importation of obscene material	pornographic magazines
384 F.2d 694	Ampilo v. United States	4	1967	0 importation of obscene material	pornographic magazines
404 F.2d 196	United States v. A Motion Picture Entitled "I am Curious-Yellow"	9	1967	0 mailing obscene material	sexually explicit letters
389 F.2d 200	Luros v. United States	2	1968	1 prohibition on importation of obscene material	film with sexually explicit scenes
418 F.2d 1051	United States v. Baranov	8	1968	1 prohibition on mailing obscene materials	nudist magazines and sexually explicit novels
418 F.2d 82	Grove Press, Inc. v. Philadelphia	3	1969	1 mailing obscene material	booklets containing pornographic photos
422 F.2d 34	United States v. Wild	3	1969	1 PA obscenity statute and common law nuisance	pornographic film
435 F.2d 228	Drive in Theatres, Inc. v. Huskey	2	1969	0 prohibition on mailing obscene material	slides of pornographic images
436 F.2d 1289	Overstock Book Co. v. Barry	4	1970	1 NC state obscenity law as interpreted by the Rutherford County Sheriff	any movie not rated G
431 F.2d 655	Miller v. United States	2	1970	0 distribution of pornography	books, magazines, etc. which included "hard-core pornography"
433 F.2d 1252	United States v. Dellapla	9	1970	0 mailing obscene material	obscene books, magazines, and ads
433 F.2d 932	United States v. Jacobs	2	1970	1 prohibition on mailing obscene material	pornographic films
431 F.2d 272	Childs v. Oregon	9	1970	0 mailing obscene material	pornographic photographs and ads
432 F.2d 705	United States v. 35 MM. Motion Picture Film etc.	2	1970	0 disseminating obscene matter in violation of OR state law	sexually explicit book
432 F.2d 420	United States v. Ten Erotic Paintings	9	1970	1 importation of obscene material	sexually explicit film
470 F.2d 386	Huffman v. United States	12	1971	0 DC obscenity ordinance	sexually explicit paintings
448 F.2d 583	United States v. Manarite	0	1971	0 prohibition on mailing obscene material	pornographic magazines
445 F.2d 945	United States v. Ewing	10	1971	0 mailing obscene matter	pornographic magazines, films, and playing cards
467 F.2d 41	United States v. Pellegrino	9	1972	1 mailing obscene material	pornographic material and advertisements
465 F.2d 282	Talman v. United States	7	1972	0 uttering obscene language on the radio	advertisements for two sexually explicit books
465 F.2d 1096	United States v. Young	9	1972	0 uttering obscene language on the radio	language is not described
455 F.2d 899	United States v. Miller	9	1972	0 mailing obscene material	obscene advertisements
459 F.2d 282	Southeastern Promotions, Ltd. v. Oklahoma City	10	1972	1 Oklahoma City's refusal to lease its auditorium	obscene advertisements
454 F.2d 280	United States v. Freemaneyer	9	1972	0 transporting in interstate commerce obscene material	the musical "Hair"
467 F.2d 1126	United States v. Smith	7	1972	1 uttering obscene language on the radio	undear
481 F.2d 605	United States v. Gates	7	1972	1 uttering obscene language on the radio	used profane language on a radio broadcast
486 F.2d 894	Southeastern Promotions, Ltd. v. Conrad	5	1973	0 prohibition on mailing obscene materials	a letter which included sexually explicit language
475 F.2d 65	United States v. Palladino	6	1973	0 TN obscenity common law and statutes	a performance of the play "Hair"
473 F.2d 1297	Cinecom Theaters Midwest States, Inc. v. Ft. Wayne	7	1973	1 prohibition on mailing obscene material	books and brochures depicting and describing porn and sex
481 F.2d 307	United States v. Hamling	9	1973	1 Fort Wayne, IN city ordinance prohibiting nudity in drive-in movies	films involving nudity
481 F.2d 206	United States v. One Reel of Film	9	1973	0 mailing obscene matter	obscene advertisements and books
487 F.2d 331	United States v. Millican	0	1973	0 prohibition on importation of obscene material	pornographic film
485 F.2d 574	United States v. Cote	0	1973	0 prohibition on mailing obscene materials	pornographic film and magazine advertising the film
484 F.2d 1149	United States v. Theius	0	1973	0 prohibition on mailing obscene materials	pornographic films, magazines, and advertisements for those films and magazines
494 F.2d 499	United States v. Grover	5	1974	1 transporting obscene material on a common carrier in interstate commer	pornographic magazines
502 F.2d 973	Brakker v. Board of Education	1	1974	0 receipt of obscene matter transported through interstate commerce	a brochure describing Woodstock and its sexual excess
487 F.2d 1300	Patterson v. United States	7	1974	1 dismissal of teachers for distributing obscene material to minors	a letter containing pornographic photographs
502 F.2d 1300	United States v. Rafter	5	1974	0 federal obscenity statute	advertisements for pornographic materials
490 F.2d 499	United States v. Palladino	1	1974	1 prohibition on mailing obscene material	book and brochure which depicted/described pornographic photos
507 F.2d 294	United States v. Harding	10	1974	0 federal obscenity statute	obscene books and films
490 F.2d 78	United States v. Sulaman	5	1974	0 mailing obscene material	pornographic ads and films
505 F.2d 1247	United States v. Miller	9	1974	0 mailing obscene material	pornographic books and magazines
507 F.2d 1100	Miller v. United States	9	1974	0 mailing obscene material	pornographic books and magazines
491 F.2d 956	, Sharple, Inc.	2	1974	0 importation of obscene material	pornographic film
502 F.2d 391	United States v. Pryba	12	1974	0 interstate transportation of obscene materials	pornographic film

Citation	Case Name	Circuit	Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
500 F.2d 733	United States v. Hill	5	1974	0 prohibition on mailing obscene materials; transportation of obscene matk	pornographic films	
506 F.2d 1251	United States v. Carter	6	1974	0 prohibition on mailing obscene materials; use of common carrier to trans	pornographic films	
505 F.2d 824	Smith v. United States	6	1974	0 interstate transportation of obscene materials	pornographic films	
490 F.2d 76	United States v. Thevis	5	1974	0 transporting obscene material on a common carrier in interstate commer	pornographic magazines	
506 F.2d 511	United States v. Friedm	8	1974	0 prohibition on mailing obscene material	pornographic magazines	
509 F.2d 368	United States v. Wornack	12	1974	0 interstate transportation of obscene matter, mailing obscene matter	pornographic magazines	
502 F.2d 419	Huffman v. United States	12	1974	1 DC obscenity ordinance	pornographic magazines	
503 F.2d 189	United States v. Gower	12	1974	0 DC obscenity ordinance	pornographic photographs and film	
498 F.2d 934	United States v. Alexander	2	1974	0 prohibition on interstate transportation of obscene material	pornographic photos	
490 F.2d 73	United States v. New Orleans Book Mart, Inc.	5	1974	0 transporting obscene material on a common carrier in interstate commer	pornographic publications and film	
515 F.2d 397	Illinois Citizens Committee for Broadcasting v. FCC	12	1974	0 broadcasting obscene material	radio call-in show	
496 F.2d 441	Amato v. Divine	7	1974	1 WI state obscenity law	sexually explicit magazines	
491 F.2d 714	United States v. Ewing	10	1974	1 mailing obscene matter	unclear	
491 F.2d 697	United States v. Harding	10	1974	1 receipt of obscene matter transported through interstate commerce	unclear	
504 F.2d 1012	United States v. Wasserman	5	1974	1 prohibition on mailing obscene materials	unclear—somehow pornographic	
524 F.2d 1244	United States v. Wasserman	5	1975	0 prohibition on mailing obscene materials	"obscene advertising brochures"	
514 F.2d 923	Clique v. United States	5	1975	1 prohibition on mailing obscene materials	letter containing sexually explicit language	
523 F.2d 3	Walker v. Dillard	4	1975	1 VA state law criminalizing cursing at someone over the phone	Mrs. Walker swore at her neighbor over the phone	
518 F.2d 20	United States v. Dachstehner	9	1975	0 mailing obscene matter	obscene advertisements	
520 F.2d 913	United States v. Marks	6	1975	0 interstate transportation of obscene materials	pornographic films	
526 F.2d 48	United States v. American Theater Corp	8	1975	0 transporting in interstate commerce obscene material	pornographic films	
513 F.2d 264	McKinney v. Parsons	5	1975	0 Birmingham, AL obscenity ordinance	pornographic magazines and films	
523 F.2d 369	United States v. Danley	9	1975	0 federal obscenity laws	unclear	
541 F.2d 810	United States v. Osbense Magazines, Films & Cards	9	1976	1 forfeiture of obscene materials but unclear what underlying offense is	"exhibits"	
543 F.2d 723	Wasserman v. Municipal Court of Alhambra Judicial Dist.	9	1976	0 CA state law criminalizing distribution of obscene material	obscene brochure	
533 F.2d 192	United States v. Linetsky	5	1976	1 prohibition on mailing obscene materials	pornographic advertisements and films	
526 F.2d 989	United States v. Thevis	5	1976	0 prohibition on mailing obscene materials	pornographic magazines, books, and advertisements	
528 F.2d 784	United States v. Friedman	10	1976	0 interstate transportation for purpose of sale and distribution	sexually explicit book	
538 F.2d 325	United States v. Baranov	4	1976	0 prohibition on mailing obscene material		
560 F.2d 720	Robinson v. Parsons	5	1977	0 Birmingham, AL obscenity ordinance	"obscene materials"	
565 F.2d 566	United States v. 2200 Paper Back Books	9	1977	1 importation of obscene material	obscene books	
549 F.2d 1369	United States v. Christian	10	1977	0 interstate transportation of obscene material	pornographic film	
564 F.2d 1294	United States v. Tupler	9	1977	1 interstate transportation of obscene materials	pornographic films	
562 F.2d 185	United States v. Various Articles of Obscene Merchandise, Schedule 1303	2	1977	0 importation of obscene material	pornographic photos	
556 F.2d 9	Pacific Foundation v. Federal Communications Commission	12	1977	1 FCC ruling	seven "patently offensive" words	
562 F.2d 954	United States v. Glasman	5	1977	0 interstate transportation of obscene materials	sexually explicit films	
558 F.2d 364	Amato v. Divine	7	1977	1 WI obscenity law	unclear	
581 F.2d 244	United States v. Bludner	10	1978	0 mailing obscene matter	obscene advertising	
575 F.2d 1303	United States v. Dost	10	1978	0 mailing obscene matter	obscene advertising	
582 F.2d 1016	United States v. Bush	5	1978	1 transporting obscene material on a common carrier in interstate commer	pornographic films	
585 F.2d 164	United States v. Marks	6	1978	0 interstate transportation of obscene materials	pornographic films	
583 F.2d 1030	United States v. Cohen	8	1978	0 mailing and use of common carriers to transport obscene material	pornographic films	
605 F.2d 210	United States v. Sandy	6	1979	0 interstate transportation of obscene materials	pornographic films	
600 F.2d 394	United States v. Various Articles of Obscene Merchandise, Schedule 1769	2	1979	0 importation of obscene material	pornographic films and other materials	
610 F.2d 428	Sovereign News Co. v. Corrigan	6	1979	0 OH obscenity statute	unclear—somehow pornographic	
602 F.2d 1192	United States v. Grasi	5	1979	0 transporting obscene material on a common carrier in interstate commerce; mailing obscene material, etc.	adult movie theaters	
631 F.2d 497	Entertainment Concepts III v. Madejewski	7	1980	1 Westmont, IL city ordinances	adult movie theaters	
610 F.2d 1353	Penthouse International Ltd. v. McAuliffe	5	1980	0 GA state obscenity law	pornographic magazines	
648 F.2d 1020	Red Bluff Drive-In Inc. v. Vance	5	1981	1 TX obscenity statute	adult entertainment providers raise a facial challenge to constitutionality of TX statute	
653 F.2d 381	United States v. Osbense Magazines, Book & Advertising Materials, et al.	9	1981	0 importation of obscene material	obscene magazines and a book	
638 F.2d 762	Reeves v. McComm	5	1981	0 Houston noise amplification ordinance prohibiting the amplification of ob	obscene words	
646 F.2d 237	United States v. Battista	6	1981	0 interstate transportation of obscene materials	pornographic film	
649 F.2d 783	Piepenburg v. Cutler	10	1981	0 UT statute prohibiting exhibition of pornographic films	pornographic film	
613 F.2d 787	United States v. Thomas	10	1981	0 mailing obscene material	pornographic films and a catalog	
675 F.2d 1365	Felhaber v. North Carolina	4	1982	0 NC state obscenity nuisance law	"pictorial obscenity"—plaintiffs here are owners of adult bookstores	
688 F.2d 1088	United States v. Langford	7	1982	0 sending child pornography through the mails	photographs and negatives depicting child pornography	
679 F.2d 826	United States v. Bagnell	11	1982	0 interstate transportation of obscene material with common carrier; interi	pornographic films	
678 F.2d 433	United States v. Bagnell	2	1982	0 interstate transportation of obscene material	pornographic films/magazines	
684 F.2d 616	United States v. Gilman	9	1982	0 mailing obscene material	sexually explicit magazines and brochures	
674 F.2d 486	Sovereign News Co. v. Falke	6	1982	0 OH obscenity statute	unclear—somehow pornographic	
674 F.2d 484	Turuso v. Cleveland Municipal Court	6	1982	0 OH obscenity statute	unclear—somehow pornographic	
722 F.2d 1274	Janich v. Pizza	6	1983	0 Toledo, OH obscenity ordinances	unclear; consolidated appeals	
705 F.2d 41	United States v. Various Articles of Obscene Merchandise, Schedule 2127	2	1983	0 importation of obscene material	plaintiffs are clerks at an adult bookstore	
702 F.2d 925	Penthouse International, Ltd. v. McAuliffe	11	1983	0 GA obscenity law	pornographic magazines	
709 F.2d 132	United States v. Various Articles of Obscene Merchandise, Schedule 2102	2	1983	1 importation of obscene material	the movie Caligula	
747 F.2d 1191	United States v. Thoma	7	1984	0 mailing child pornography for the purpose of sale	child pornography film	
747 F.2d 824	United States v. Petrov	2	1984	0 prohibition on mailing obscene material	pornographic photos	
746 F.2d 458	United States v. Merrill	9	1984	0 mailing obscene materials	pornographic playing cards	
744 F.2d 1061	Oison v. Lecke	4	1984	0 SC state obscenity law	pornographic printed material	
750 F.2d 596	United States v. Various Articles of Merchandise, Seizure No. 170 & 182	9	1984	1 importation of obscene material	sexually explicit magazines	
725 F.2d 482	J-R Distribs. v. Eikenberry	9	1984	1 WA obscenity law	unclear—consolidated appeal	
780 F.2d 1389	Upper Midwest Booksellers Assoc. v. Minneapolis	8	1985	0 Minneapolis city ordinance	pornographic pamphlets and magazines	
779 F.2d 1177	Brooks v. Selter	6	1985	1 OH state law preventing prisoners from receiving "obscene" or "inflaming	pornographic pamphlets and magazines	
801 F.2d 740	Hoover v. Byrd	5	1986	0 TX obscenity statute	"commercial obscenity"	
804 F.2d 1104	BSA, Inc. v. King County	9	1986	1 WA county ordinances	barroom nude dancing	
795 F.2d 765	United States v. Hurt	9	1986	0 mailing obscene materials	pornographic films	

Citation	Case Name	Circuit Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
803 F.2d 174	United States v. Marchant	5	1986	0 knowingly receiving child pornography	pornographic magazines featuring children
791 F.2d 463	Paduch v. Investment Entertainment, Inc.	6	1986	1 Paduch, KY obscenity ordinance	pornographic movie theaters, adult bookstores, etc.
826 F.2d 708	Moses v. County of Kenosha	7	1987	0 Kenosha County, WI obscenity ordinance	adult bookstores
819 F.2d 451	United States v. Guglielmi	4	1987	0 prohibition on mailing obscene material; use of common carrier to transport films depicting bestiality	materials sold at adult bookstores
816 F.2d 1326	Polykoff v. Collins	9	1987	0 AZ obscenity statute	a pornographic videotape
848 F.2d 923	United States v. Zangger	8	1988	1 mailing obscene material	"mainstream" pornographic materials
868 F.2d 1043	Rippinger v. Collins	9	1989	1 AZ obscenity statute	pornographic materials
867 F.2d 1188	Dworkin v. Hustler Magazine, Inc. v. King County	9	1989	1 none-Andrea Dworkin sued Hustler for libel; invasion of privacy; among other things, Hustler published photographs of Dworkin in his magazine	sexually explicit illustrations and photographs
911 F.2d 80	Walker v. Kansas City	8	1990	0 Kansas City zoning ordinance	exotic dancing at a bar
900 F.2d 748	United States v. Pytha	4	1990	0 RICO and state obscenity law	pornographic books and videos
902 F.2d 513	Kucharek v. Hanaway	7	1990	0 WI obscenity law	pornographic books and videos
901 F.2d 630	Sequela Books, Inc. v. Ingenunson	7	1990	0 IL obscenity statute	pornographic films, magazines, photographs, etc.
943 F.2d 825	Alexander v. Thornburgh	8	1991	0 RICO with obscenity violations as predicate offenses	sexually explicit magazines, books, etc., sold by adult bookstore (plaintiff)
927 F.2d 1442	United States v. Easley	8	1991	0 mailing obscene material	pornographic videos and magazines
952 F.2d 155	United States v. ABC, Inc.	8	1991	0 transportation of obscene materials in interstate commerce using a common carrier	sexually explicit videotapes and magazines
960 F.2d 134	Luke Records v. Navarro	11	1992	1 Florida county sheriff claiming the song is obscene	rap song by 2 Live Crew
10 F.3d 263	United States v. Investment Enterprises, Inc.	5	1993	0 interstate transportation of obscene materials	sexually explicit box covers and video tapes
25 F.3d 1314	United States v. Skinner	6	1994	0 engaged in business of selling or transferring obscene matter	adult bookstores
18 F.3d 1181	Eckstein v. Meison	4	1994	0 federal obscenity statute	pornographic books/magazines
31 F.3d 135	United States v. Schein	3	1994	0 prohibition on mailing obscene material	sexually explicit film
74 F.3d 701	United States v. Thomas	6	1996	0 federal obscenity laws	an electronic bulletin board on which Thomas sold sexually explicit photos
230 F.3d 649	United States v. Various Articles of Merchandise, Schedule 287	3	2000	1 importation of obscene material	nude magazines from France and Germany
237 F.3d 251	United States v. Loy	3	2001	1 receiving and possessing child pornography; after conviction, Loy was precluded from viewing any pornographic material	images depicting child pornography
248 F.3d 394	United States v. Fox	5	2001	0 receipt of child pornography through the internet	Landham made obscene phone calls to his wife solely to harass her
251 F.3d 1072	United States v. Landham	6	2001	1 making obscene interstate phone calls	explicit photographs and language used in emails to minors
377 F.3d 49	United States v. Gravenhorst	1	2004	0 use of the internet to solicit minors	violent porn
426 F.3d 765	United States v. Ragsdale	5	2005	0 mailing obscene materials	depictions of "ascivious conduct"
459 F.3d 80	United States v. Fabrizio	1	2006	0 child porn statute	obscene phone calls
466 F.3d 938	United States v. Eckhardt	11	2006	0 prohibition on making harassing phone calls	promoting (obscene) child porn
444 F.3d 1286	United States v. Williams	11	2006	1 statute banning promotion of child porn	simulated sexual acts--something defined by Miller as obscene and therefore regulable
470 F.3d 1074	Giovani Carandola, Ltd. v. Fox	4	2006	0 NC statute regulating erotic dancing	violent/sexually explicit video games
469 F.3d 641	Enmt Software Ass'n v. Blagovetich	7	2006	1 statute regulating video games	child porn which also qualified as "obscene" under Miller
550 F.3d 326	United States v. Whorley	4	2008	0 child porn statute	child porn which also qualified as "obscene" under Miller
546 F.3d 965	United States v. Schales	9	2008	0 child porn statute	private intimate conduct
517 F.3d 738	Reliable Consultants, Inc. v. Earle	5	2008	1 TX ban on sale of sexual devices	

C Randomization

Even if judges are randomly assigned, because our data comprise published opinions, several additional issues need to be considered: settlement, publication, and strategic use of keywords or citation. In Courts of Appeals, judges are revealed very late, after litigants file their briefs, sometimes only a few days before the hearing, if there is a hearing, which gives little opportunity and incentive for settlement upon learning the identity of the panel. Most of the litigation costs are sunk by that point, and when the D.C. Circuit began announcing judges earlier, it did not affect settlement rates (Jordan 2007). Unpublished cases are not supposed to have precedential value. Unpublished cases are deemed as routine and easy: studies find that judicial ideology predicts neither the decision in unpublished cases (Keele et al. 2009) nor the decision to publish (Merritt and Brudney 2001). To rule out strategic use of keywords or citation of Supreme Court precedent, we propose an omnibus test to collectively address deviations from strict exogeneity: we examine how similar the string of actual panel assignments is to a random string, the details of which are developed in Chen (2013). To see random strings as an omnibus test: Suppose Democrats publish cases and Republican judges do not. In order for this to explain any effects, we should expect Democrat judges to violate the random strings test.

We assess deviations from random assignment by examining whether the sequence of proportions of judges is similar to a random process. Appendix Figure 5 shows that panel composition is not serially correlated. Formally, we are:

1. Proposing a statistic that can be computed from the sequence of numbers of Democrats per seat within a Circuit.
2. Computing the statistic for the actual sequence, s^* .
3. Computing the statistic for each of 1,000 bootstrap samples from the actual sequence, i.e., $s_1, s_2, s_3 \dots s_n$. Since there were changes in the expected number of Democrats per seat over time, we are treating our bootstrap samples as a vector of realized random variables, with the probability based on the expectation during the Circuit-year.
4. Computing the empirical p-value, p_i by determining where s^* fits into $s_1, s_2, s_3 \dots s_n$.
5. Repeating steps 1-4 and calculate p_i for each unit.

We use the following statistics:

Autocorrelation: We see if the value in the j^{th} case depends on the outcome in the $j-1^{\text{th}}$ case. This statistic can detect whether judicial assignments are “clustered,” meaning a higher than expected number of back-to-back seat assignments to a particular type of judge. This test tells us whether certain judges sought out free speech cases, perhaps in sequence.

Mean-Reversion: We test whether there is any form of mean reversion in the sequence, meaning that the assignment in the n^{th} case is correlated with the assignment in previous $n - 1$ cases. This test tells us

TABLE X
RANDOMIZATION CHECK: P-VALUES

Democratic Appointees assigned to Free Speech Cases					
	distance	size	90%	95%	99%
Autocorrelation	0.188	12	0.338	0.375	0.450
Mean Reversion	0.274	12	0.338	0.375	0.450
Longest Run	0.376	10	0.368	0.410	0.490

whether judges or their assignors were attempting to equilibrate their presence, considering whether a judge was “due” for a free speech case.

Longest-Run: We test whether there are abnormally long “runs” of certain types of judges per seat. This test tells us whether certain Circuits may have assigned certain judges with free speech cases during certain time periods (e.g., to achieve specialization).

Number of Runs: Instead of simulating 1000 random strings, we compute the exact statistic for number of runs. This test captures violations of randomization at the case level rather than Circuit-year. In power calculations, this test has less Type II error compared to the other tests, as well as to their regression analog.

With a truly random process, the collection of all unit p-values should be uniformly distributed. The 1001th random string should have a summary statistic that is equally likely to be anywhere from 1 to 1000. A visual examination suggests that the empirical distributions for our p-values approach the CDF of a uniform distribution. Appendix Figure 6 presents each Circuit as one dot. Table X shows that the Kolmogorov-Smirnov test statistic cannot reject the distribution of p-values is different from the uniform.

Random strings test complements standard randomization checks, e.g., examinations of (1) leads and (2) correlations between judicial composition and pre-determined case characteristics. If pre-determined covariates occur randomly over time, checks of (2) miss non-random serial correlation in judicial composition while the random strings test would miss correlations between judicial composition and pre-determined covariates.

We also stack the strings across Circuits and across biographical characteristics, and run an autocorrelation test and compare the F statistic with F statistics generated from randomly assigning available judges to cases. The results are consistent with randomization.

Other variations from random assignment include: remanded cases from the Supreme Court are returned to the original panel; en banc cases that are heard by the entire pool of judges (or a significant fraction in the Ninth Circuit); judges with conflict of interests opt out after random assignment, which is extremely rare. We do not use remanded or en banc cases, which are also relatively infrequent. Judges can also take sick leave or go on vacation, but this is determined far in advance.

Our identification strategy—like the identification strategy of papers that use the patent officer assign-

ment or disability application reviewer assignment, which are not explicitly random ([maestas2011does, galasso2015patents])—assumes that idiosyncratic deviations from random assignment are ignorable. Even a gold-standard random process—the roll of a die—has a deterministic element. If known with precision, the force and torque applied to the die, the subtle air currents, the hardness of the surface, etc., might allow us (or a physicist) to determine with certainty the outcome of these “random” rolls. Despite this obvious non-randomness, we would still have faith in the outcome of a trial with treatment assignments based on die rolls because we are certain that the factors affecting the assignment have no impact on the outcome of interest and hence are ignorable.

D District Courts

Litigants' decision to appeal may respond to previous years' legal decisions, however, so controlling for $\mathbf{1}[M_{ct} > 0]$ may bias the coefficient for Law_{ct} ; the bias is more severe for more distant lags and non-existent for the most advanced lead. We assess whether this potential endogeneity is a significant concern by comparing $\beta_{1(t-n)}$ when we instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. District judge demographic characteristics are correlated with reversal rates in the Courts of Appeals (Haire, Songer, and Lindquist 2003; Sen 2015; Barondes 2010; Steinbuch 2009); and expected reversal rates could encourage litigants to pursue an appeal. If $\mathbf{1}[M_{ct} > 0]$ and Law_{ct} are both identified, estimates should be roughly invariant to the inclusion or exclusion of additional lags and leads (including lags that are important predictors of the outcome improves statistical precision, but losing data at the beginning and end period reduces precision) and lead coefficients being 0 provide an omnibus check of our instrumental variable being endogenous to pre-existing trends.

District Courts assign one judge to a case randomly or rotationally (Taha 2009; Bird 1975). Cases being returned on remand from the Courts of Appeals are not randomly assigned. We do not use remanded cases in our dataset. For example, one District told us that random assignment occurs within 24 hours of a case filing, which is handled in the order of its arrival. Waldfogel (1995) reports that one District Court uses three separate randomization wheels, with each wheel corresponding to the anticipated case length. Related cases (meaning that one decision will substantially resolve all cases), if filed within a few weeks, may be consolidated. Waldfogel (1995) reports that plaintiffs can argue the case is related to another pending case and, if the judge agrees, the cases will be consolidated. A clerk reported 8% of filed cases were accepted as related in 1991 in SDNY. In another District Court, if a clerk identifies and two judges agree that a new civil case is related to another open civil case, the two cases will be consolidated in the interests of justice or judicial economy. The clerk brings the possible connection to the attention of the judge of the new case, who then confers with the judge of the earlier case to determine whether these are in fact related cases. Consolidation would only occur for relatively high-frequency case types. For the handful of District cases that do overlap such that they are consolidated, we assume the decisions about case relatedness occur in a manner exogenous to judge assignment.

To instrument for $\mathbf{1}[M_{ct} > 0]$, we define our District IV as follows. $w_{ct} = \frac{\sum_{d=1}^J K_{c dt} * \left(\frac{L_{c dt}}{K_{c dt}}\right)}{\sum_{d=1}^J K_{c dt}}$, where $K_{c dt}$ denotes the number of cases filed in District *court* d within Circuit c at time t (J goes from 5 to 13 depending on the District). $L_{c dt}$ denotes the number of judges with a particular characteristic assigned to cases. The intuition is that assigning District judges who are disproportionately appealed leads to an appeal in the Circuit, $\mathbf{1}[M_{ct} > 0]$. Note that this assumes $K_{c dt} > 0$. An approximation is to define $K_{c dt} * \left(\frac{L_{c dt}}{K_{c dt}}\right)$ as 0 if $K_{c dt} = 0$. Then, the instrument can be constructed if $\sum_{d=1}^J K_{c dt} > 0$, which holds as we have a large number of district cases.

Unlike for Courts of Appeals cases, the random strings test cannot be used as an omnibus assessment for violations of random assignment, because some Districts use rotational assignment or random drawing of judges from card decks without replacement. Consequently, we discuss the concerns qualitatively and suggest another empirical test. First, District Courts judges are revealed much earlier than Courts of Appeals judges. Ideally, we would use docket filings in the Administrative Office of the U.S. Courts, but judges are omitted for most cases prior to 2000, so we must use published District opinions to construct our District IV. Accordingly, we buttress the assumption that settlement, publication, and strategic use of keywords or citations are exogenous: 1) in District Courts, judges are much more constrained and ideology has been found to play hardly any role. Judicial ideology does not predict settlement rates (Ashenfelter et al. 1995; Nielsen et al. 2010), settlement fees (Fitzpatrick 2010), publication choice (Taha 2004), or decisions in published or unpublished cases (Keele et al. 2009)—this last fact is consistent with the District judge identity only affecting outcomes through the presence of an appeal but not through the District Court decision; this exclusion restriction, however, is not necessary for the primary counterfactual; 2) we examine these issues directly as follows.

Since the random strings test is ineffective for District Courts, we test whether District Court judicial biographical characteristics in *filed* cases jointly predict publication. We link PACER filing data, which has judge identity, to AOC data, which has information on publication. We obtained all freely available PACER data on District cases from 32 districts from 1980 to 2008 for a total of 359,595 non-duplicated cases. This data contains the name of the District where the case was filed, the filing and termination date (missing for 10% of cases), the assigned docket number, and the name of the District or magistrate judge presiding on the case. We merge the names of the judges into the AOC database. We use LASSO to select biographical characteristics and no characteristic was chosen. We assume that remaining deviations from random assignment, like vacation days, are ignorable.

E Additional Counterfactual Calculations

E.1 Summary and Counterfactuals Even though the differences in free speech activity seem to be aligned with the differences in judges' preferences revealed in their votes, the results we have discussed so far focus on the difference in outcomes after progressive as opposed to conservative precedent. Next, we examine progressive precedent vs. no decision and conservative precedent vs. no decision. Table XI summarizes the following parameters for each outcome: β_1 , $\beta_1 + \beta_2$, and β_2 , scaled by the number of cases per year to report the typical effect per year of free speech precedent.⁴³ This results in a smaller magnitude than the unscaled coefficients.⁴⁴ The first column summarizes the findings reported thus far.

The second column reports that progressive decisions—as opposed to no decision—still yield progressive impacts on attitudes and behaviors, but some of the effects on crime are reversed (the progressive precedent reduces sex crimes in three of the four categories). One reason for this is a form of displacement. The absence of a case serves as a super-control. Crépon et al. (2013) introduce this idea in the context of a national experiment that randomizes (a) the presence of an employment training program across cities and (b) the training of individuals when there was a program. In the federal courts, we seek (a) random presence of an appellate case and (b) random decision when there was a case.

Differences between trained and non-trained individuals reflect our first counterfactual, β_1 . Differences between trained individuals in treated cities and non-trained individuals in control cities reflect our second counterfactual, $\beta_1 + \beta_2$. Differences between the first and second counterfactuals are what Crépon et al. (2013) refer to as displacement. Trained individuals displace non-trained individuals from employment when there is a limited supply of positions. In our application, if there is a pre-defined set of free speech regulations, government actors may issue the regulation only in a favorable legal regime; alternatively, the supply (or arrests) of crime may be limited.

The lack of displacement-effects for attitudes and behavior is consistent with law providing norm-shifting information. There is no reason to expect individuals to delay their norm changes until a favorable legal regime. This interpretation is further supported by the mechanism experiment, which we describe below. On the other hand, some of the effects on crime in Column 1 may be due to displacement. Notably, the effects on child abuse do not change, which suggests some of its channels may be more attitudinal or less displaced. The third column shows the impacts of β_2 . Conservative free speech jurisprudence reduced crime (with the exception of child abuse) and disease.

⁴³To compute the effect of progressive precedent in a typical Circuit-year, we multiply the coefficient on Law_{ct} by $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$, the typical proportion of decisions that are progressive when there are Circuit cases, and by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of Circuit-years with a Circuit case. A similar calculation can be made for the typical effect of progressive precedent taking into account the presence of an appeal: $\mathbf{1}[M_{ct} > 0] * \mathbf{E}[\mathbf{1}[Progressive_{ct} > 0]] + Law_{ct} * \mathbf{E}[\mathbf{1}[Progressive_{ct} > 0]]$. These estimates can be used to simulate counterfactuals.

⁴⁴The statistical significance of the effects are the same as the coefficients and hence not repeated here.

TABLE XI
SUMMARY OF RESULTS

Typical Effects	Progressive vs. Conservative Decision	Progressive vs. No Case	Decision vs. No Case
Sexual Attitudes			
Extramarital Sex is OK	0.0005	0.0005	-0.0000
Premarital Sex is OK	0.0002	0.0004	0.0010
Homosexual Sex is OK	0.0001	0.0004	0.0013
Sexual Behaviors			
Paid Sex	0.0001	0.0000	-0.0002
Partners Per Year	0.003	0.005	0.013
Number of Female Partners	0.120	0.080	-0.103
Partners Per Year (reported by Men)	0.007	0.012	0.033
Number of Female Partners (reported by Men)	0.276	0.199	-0.157
Extramarital Sex (reported by Men)	0.002	0.001	-0.002
Crimes			
Prostitution	0.140	-0.116	-0.705
Drug Violations	1.665	-0.446	-5.402
Rape	0.143	0.086	-0.092
Offenses Against Family and Children	-2.646	-1.904	0.289
Sexually Transmitted Diseases			
Chlamydia Incidence	1.977	1.223	-0.991

Notes: This table summarizes β_1 , $\beta_1 + \beta_2$, and β_2 for each outcome, scaled by the number of cases per year to report the typical effect per year of free speech jurisprudence.

F Mechanism Experiment

We recruited workers through Amazon Mechanical Turk. We posted a single placeholder task containing a description of the work and a link for workers to follow if they want to participate. The subjects were then randomized, via stratification in the order in which they arrived at the job, to one of several treatment conditions. Treatment was not revealed at this early stage. All workers saw identical instructions.

We asked workers to transcribe paragraphs from a Tagalog translation of Adam Smith's *The Wealth of Nations*, as well as English paragraphs of dictionary definitions. This task is sufficiently tedious that no one is likely to do it "for fun," and sufficiently simple that all market participants can perform it. The source text was machine-translated to prevent subjects from finding the text elsewhere on the Internet. We minimize attrition through a commitment mechanism. In all treatment conditions, workers faced an identical "lock-in" task in order to minimize differential attrition before the treatment was revealed. The lock-in successfully reduces attrition.

1 of 3 Lock-in Tasks: Kaya sa isip o diwa na tayo ay sa mga ito, excites ilang mga antas ng parehong damdamin, sa proporsyon ng kasiglahan o dulness ng kuru-kuro. Ang labis na kung saan sila magbuntis sa kahirapan ng mga wretches nakakaapekto sa partikular na bahagi sa kanilang mga sarili ng higit pa sa anumang iba pang; dahil sa takot na arises mula sa kathang isip nila kung ano ang kani-kanilang mga sarili ay magtiis, kung sila ay talagang ang wretches kanino sila ay naghahanap sa, at kung sa partikular na bahagi sa kanilang mga sarili ay talagang apektado sa parehong miserable paraan. Ang tunay na puwersa ng mga kuru-kuro na ito ay sapat na, sa kanilang mga masasaktin frame, upang gumawa ng na galis o hindi mapalagay damdam complained ng.

The payment for each paragraph was 10 cents with workers able to receive much more in bonuses, including a 50-cent bonus for completing the survey from the GSS at the end. A paragraph takes about 100 seconds to enter, so the offered payment of 10 cents per paragraph is equivalent to \$86.40 per day. The federal minimum wage in the United States was \$58/day. In India, payment rate depends on the type of work done, although the "floor" for data entry positions appears to be about \$6.38/day.⁴⁵ An example paragraph was displayed on the first page of the external hosting site so workers were aware of the high payment prior to entering the study.⁴⁶

After the lock-in task of three paragraphs, treatment was revealed. Original newspaper articles are available on request.

⁴⁵Payscale, Salary Snapshot for Data Entry Operator Jobs, http://www.payscale.com/research/IN/Job=Data_Entry-Operator/Salary?, accessed June 17, 2011.

⁴⁶In fact, one worker emailed saying that 10 cents was too high and that the typical payment for this sort of data entry was 3 cents per paragraph.

Treatment 1 (Conservative): A federal court has ruled that the North Carolina legislature may ban the sale of hardcore pornography in bookstores. The North Carolina legislature had enacted the ban as a nuisance abatement measure. The legislature considered adult bookstores to be nuisances. Adult bookstore owners had challenged the North Carolina statute as unconstitutional. They argued that the statute would be restricting expression before they reach the public and before they are deemed obscene or not. In general, prior restraints on speech are unconstitutional under the First Amendment. However, the First Amendment does not protect obscene speech. The Fourth Circuit court said that statute's prior restraints on explicit photographs and films are acceptable, because they applied only to films and photos sold in hardcore pornography stores. The speech was not completely limited since other stores, such as regular newsstands, could still sell the material.

Treatment 2 (Conservative): Hillsborough County soon will begin enforcing its strict ordinances governing adult businesses now that a federal appeals court has ruled the restrictions are constitutional. County Attorney Renee Lee said the county does not yet have a timeframe for compliance. The ruling from the 11th Circuit U.S. Court of Appeals means that dancers at bikini bars will have to stay 6 feet away from patrons, and the sale or consumption of alcohol will be prohibited at adult businesses. Additionally, adult video stores would be prohibited from having private viewing booths and workers would have to pass a criminal background check before they are hired. Attorney Scott D. Bergthold, who represented Hillsborough, said the court's decision held that the county government "acted reasonably" in adopting the ordinances. This demonstrates that local governments have the ability to effectively regulate such establishments to control their negative effects on the community.

Treatment 3 (Progressive): A company may transport obscene magazines as long as the magazines have enough literary content and social value, according to the Fifth Circuit. Michael Travis and the Peachtree News Company appealed to the Fifth Circuit after prosecutors in a federal trial court convicted them of twelve counts transporting obscene magazines across state lines. The government may constitutionally regulate the interstate transport of materials that are defined as obscene. The First Amendment protects speech generally, making it harder for the government to regulate constitutionally protected speech. However, obscenity is excluded from First Amendment protections. According to the Fifth Circuit ruling, the magazines' pictures alone would be obscene. But six of the magazines also had short stories and discussions of lesbianism, homosexuality, nudity, censorship, photography, marital sexual problems, and fine art. These gave them enough social value to merit constitutional protection.

Treatment 4 (Progressive): The Boys of Cocodorm – Snow Bunni, J Fizzo, et al – are staying put, after a federal judge ruled that the gay porn website has a right to film out of its Edgewater home. Cocodorm.com features black and Hispanic men, known as "dorm dudes," who share a webcam-filled house together and have sex on schedule. For that they are paid at least \$1,200 a month, plus free room

and board. Miami has tried to shut the house down, arguing it constitutes an adult business illegally operation in a residential area. The city’s Code Enforcement Board in 2007 agreed, but Cocodorm responded to the code enforcement proceedings by suing in federal court. From the outside, the Cocodorm house looks like any other residence. Those who want to see Cocodorm’s “hottest and horniest” do so via the Internet, with a credit card.

Treatment 5 (Control): The IAU has so far recognized five dwarf planets differentiated from planets by a parameter of “planetary discriminant.” According to NationMaster Encyclopedia, dwarf planets follow orbits which are not free from other minor celestial bodies. Simultaneously, they always circle the Sun and not other celestial objects (they are not satellites). Several dwarf planets have already been scrutinized effectively. Their physical properties have been calculated through routine Earth-based observations. Dwarf planets, particularly Pluto, are often mistakenly described as “planetoids” or “comets”. This confusion stems mostly from their size and surface texture which, in accordance with varying parameters, can be attributed to various minor celestial bodies. The above names of particular dwarf planets have also been subject to numerous changes. Until today not all solar system bodies have been identified and remain unclassified. The list of dwarf planets as well as other celestial bodies will be constantly altered.

The empirical specification examines the effect of exposure to progressive free speech precedents:

$$Outcome_{it} = \alpha + \beta_1 Treatment_{it} + \beta_2 X_{it} + \varepsilon_{it}$$

$Treatment_{it}$ is defined as 1 (for progressive), 0 (for control), or -1 (for conservative) for individual i in treatment t . X_{it} are demographic controls. We control for whether the data worker is male and, in experiment 1 with 197 workers mostly from India and the U.S., a dummy indicator for being from India. The second experiment is essentially identical (with an additional question on beliefs) but was restricted to the U.S. and had 548 workers. We report the results separately. The pooled results would be stronger.

The third experiment was also restricted to the U.S. and had additional questions. The questions in this third experiment also included incentivized measures of second order norms analyzed separately in Chen and Yeh (2014). One group was asked to report their own standards while another group was asked to estimate the other workers’ standards, while being offered payment incentives for accuracy. One group was asked to report their own behaviors and another group to estimate the prevalence of the other workers’ behaviors, again with incentive pay for accuracy. This design differs from the two experiments reported here in that it: (i) used monetary incentives to measure belief-updating of others’ moral views (community standards); (ii) separated individual from community standards; and (iii) measured subjective utility. The

experimental findings on attitudes, behaviors, and perceived prevalence of extramarital sex were replicated with 600 U.S. workers. The study also provides suggestive evidence that legitimacy of law can affect utility and self-identification.

In both Tables XII and XIII, we see significant effects on acceptability of homosexual sex as worded in the General Social Survey and echoing the population-based analysis.⁴⁷ The experiments also show that self-reported behaviors did not shift in response to progressive free speech precedents. The short timeframe of the study precludes actual behaviors from changing. The null result suggests that self-reporting norms are unlikely to explain the results in the population-based analyses.

In addition, the short timeframe precludes exposure to materials censored or approved by the law, so the changes in stated values suggest that laws can have independent effects on attitudes and values outside of the deterrence channel. The second experiment with only American workers replicates the findings from the first experiment. Table XIII reports that exposure to progressive obscenity precedent increased the likelihood that people favor sex education in public schools and the acceptability of homosexual sex by 4 percentage points. These effects are notably larger than the population-level estimates, where the impact on the acceptability of homosexual sex ranged from 0.3 to 1.7 percentage points. But population estimates are the weighted average of the direct effects of exposure and the indirect effects of exposure. The unknown parameter is the probability of direct exposure.

Table XIII also shows that exposure to conservative obscenity precedents increased perceived prevalence of extramarital sex by 2.5 percentage points. This result verifies the information channel in the law and norms theory: when legal authorities increase sanctions against a particular activity, people infer that more people are engaged in this activity.

⁴⁷ The effects are similar in a probit specification. These effects are robust to dropping the control group.

These effects also remain when we exclude Treatment 4, which explicitly referred to homosexual sex.

TABLE XII.— The Effect of Progressive Free Speech Precedents on Attitudes and Behavior

Panel A: Attitudes		Premarital Sex is OK (1)	Extramarital Sex is OK (2)	Teen Sex is OK (3)	Homosexual Sex is OK (4)	Favor Sex Ed in Public School (5)
Progressive Free Speech		0.00568	-0.0403	-0.0292	0.0637+	-0.0537
Decision		(0.0363)	(0.0280)	(0.0304)	(0.0373)	(0.0392)
India		-0.386**	0.0528	-0.307**	-0.363**	-0.181*
		(0.0680)	(0.0524)	(0.0569)	(0.0697)	(0.0734)
Male		0.246**	0.0698	0.135*	0.138+	0.0631
		(0.0693)	(0.0534)	(0.0580)	(0.0711)	(0.0748)
Mean Dep. Var.		0.569	0.153	0.222	0.483	0.488
Observations		197	197	197	197	197
R-squared		0.163	0.030	0.142	0.133	0.042
Panel B: Behaviors		Nonmarital Sex in Last Year (6)	Casual Date Sex in Last Year (7)	Paid Sex in Last Year (8)	Saw X-rated Movie (9)	Sex Frequency Monthly or More (10)
Progressive Free Speech		-0.0131	-0.00403	0.0187	0.0419	0.0335
Decision		(0.0387)	(0.0286)	(0.0235)	(0.0380)	(0.0388)
India		0.124+	0.00969	-0.00506	-0.110	-0.213**
		(0.0724)	(0.0535)	(0.0440)	(0.0712)	(0.0726)
Male		0.0478	0.146**	0.149**	0.328**	-0.0173
		(0.0738)	(0.0546)	(0.0449)	(0.0725)	(0.0740)
Mean Dep. Var.		0.399	0.158	0.099	0.517	0.438
Observations		197	197	197	197	197
R-squared		0.021	0.040	0.057	0.098	0.050

Notes: Standard errors in parentheses. +p<0.10, *p<0.05, **p<0.01

TABLE XIII.— The Effect of Progressive Free Speech Jurisprudence on Values and Beliefs

Attitudes	Premarital Sex is OK (1)	Extramarital Sex is OK (2)	Teen Sex is OK (3)	Homosexual Sex is OK (4)	Favor Sex Ed in Public School (5)	Percentage of People who have Extramarital Sex (6)
Progressive Free Speech Decision	0.00942 (0.0190)	0.0145 (0.0156)	-0.0192 (0.0231)	0.0351+ (0.0209)	0.0425+ (0.0227)	-2.511* (0.979)
Male	0.0576 (0.0360)	0.0839** (0.0297)	0.150** (0.0439)	0.0213 (0.0398)	-0.000567 (0.0430)	-6.741** (1.861)
Mean Dep. Var.	0.803	0.124	0.392	0.739	0.655	44.532
Observations	548	548	548	548	548	548
R-squared	0.005	0.016	0.022	0.006	0.006	0.035

Notes: Standard errors in parentheses. +p<0.10, *p<0.05, **p<0.01

G Visual Hausman test

These figures present “visual Hausman” tests. We display the 2SLS estimates of $\frac{\sum_n \beta_{1n}}{n}$ using alternative instruments. The yellow line indicates the estimate from the “Appellate IV” specification where $Law_{c(t)}$ is instrumented for using the assignment of Democratic judges. The red dots indicate alternative estimates using other biographical characteristics whose first stage F-statistics in Circuit-year level regressions represent the top 50 in first stage strength. The patterns reveal that the 2SLS estimates using Democratic judges or LASSO IV are typically smaller in absolute magnitude than 2SLS estimates from alternative instruments. This is consistent with greater efficiency when using LASSO, which yields smaller estimates and smaller standard errors. For some outcomes, all of the alternative 2SLS estimates are of the same sign. For example, progressive free speech precedent reduces offenses against family and increases prostitution and drug violations. It also increases chlamydia and number of sexual partners in most models.

G.1 GSS attitudes

G.2 GSS behaviors

G.3 Crime data

G.4 Disease data

APPENDIX TABLE II.— The Effects of Free Speech Precedents on Attitudes

Dependent Variable	Extramartial Sex is OK					Premarital Sex is OK					Homosexual Sex is OK							
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE	(11)	(12)	(13)	(14)	(15)	Wild BS %LE
Proportion Progressive Free Speech	-0.000817 (0.00995)	0.00247 (0.0606)	-0.272 (0.486)	-0.000555 (0.0142)	0.0188 (0.0187)	0.69	-0.0421* (0.0182)	-0.387 (0.284)	-0.0186 (0.292)	-0.0294 (0.0256)	-0.0284 (0.0278)	0.73	-0.00374 (0.0152)	0.0854 (0.0708)	-0.304 (0.887)	-0.0243 (0.0329)	-0.0224 (0.0341)	0.90
Appellate Decisions _{t+1}	-0.0192 (0.0147)	-0.0136 (0.0812)	-0.0501 (0.410)	-0.0179 (0.0161)	-0.0310+ (0.0159)	0.09	0.0611 (0.0358)	0.0856 (0.413)	0.00340 (0.926)	0.0644+ (0.0347)	0.0614+ (0.0365)	0.45	-0.0113 (0.0358)	-0.0314 (0.140)	-0.232 (0.510)	0.0125 (0.0411)	0.0137 (0.0447)	0.80
Proportion Progressive Free Speech	0.00770 (0.0111)	-0.0547 (0.0741)	0.259 (0.670)	0.0183 (0.0193)	0.0389+ (0.0233)	0.18	-0.0613+ (0.0286)	-0.0947 (0.515)	-0.224 (0.785)	-0.0644+ (0.0351)	-0.0627+ (0.0356)	0.39	-0.0133 (0.0242)	-0.0624 (0.144)	-0.165 (0.958)	-0.0410 (0.0461)	-0.0369 (0.0603)	0.64
Appellate Decisions _{t-1}	-0.00296 (0.0120)	0.0484 (0.138)	0.0430 (0.570)	0.0209 (0.0198)	0.0197 (0.0232)	0.60	0.00118 (0.0281)	-0.243 (0.335)	0.119 (0.515)	0.0190 (0.0309)	0.0299 (0.0328)	0.69	0.0219 (0.0241)	0.126 (0.238)	-0.214 (0.909)	0.0772** (0.0213)	0.0904** (0.0190)	0.05
Proportion Progressive Free Speech	0.0256+ (0.0137)	-0.0303 (0.0393)	-0.287 (1.447)	0.0175 (0.0150)	0.00465 (0.0289)	0.81	-0.00424 (0.0198)	-0.0823 (0.497)	0.259 (3.728)	0.0260 (0.0282)	0.0278 (0.0302)	0.85	-0.0105 (0.0306)	-0.114+ (0.0660)	0.454 (2.168)	-0.0361 (0.0317)	-0.0364 (0.0426)	0.48
Appellate Decisions _{t-3}	0.0142 (0.0109)	0.0534* (0.0254)	-0.102 (0.161)	0.00224 (0.0179)	0.00661 (0.0194)	0.94	0.00468 (0.0180)	0.0491 (0.263)	0.0792 (2.421)	0.0284+ (0.0158)	0.0153 (0.0164)	0.94	0.0182 (0.0147)	0.165+ (0.0859)	-0.0601 (0.462)	-0.00737 (0.0304)	-0.0151 (0.0252)	0.97
Proportion Progressive Free Speech																		
Appellate Decisions _{t-4}																		
N	18874	18874	18874	18874	18874		18801	18801	18801	18801	18801		18073	18073	18073	18073	18073	
R-sq	0.014	0.012	.	0.013	0.013		0.028	0.014	0.015	0.028	0.028		0.057	0.052	.	0.057	0.056	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV		N	Y	Y	Lasso IV	Lasso IV		N	Y	Y	Lasso IV	Lasso IV	
District IV	N	N	N	N	N		N	N	N	N	N		N	N	N	N	N	
Aggregation Level																		
Mean dependent variable	0.097	0.097	0.097	0.097	0.097		0.633	0.633	0.633	0.633	0.633		0.267	0.267	0.267	0.267	0.267	
Average Law _t effect	0.005	0.001	-0.027	0.008	0.008		0.000	-0.057	0.047	0.015	0.014		0.001	0.017	-0.043	0.001	0.003	
P-value of Law _t lags	0.002	0.001	0.639	0.135	0.001		0.126	0.666	0.815	0.001	0.000		0.805	0.000	0.574	0.000	0.000	
P-value of Law _t leads	0.936	0.968	0.576	0.967	0.315		0.041	0.174	0.949	0.251	0.307		0.810	0.228	0.732	0.460	0.510	
Average 1[M _t >0] lag	0.001	0.002	-0.003	0.000	-0.001		0.005	0.036	0.007	0.002	0.001		0.006	-0.002	0.060	0.006	0.006	
P-value of 1[M _t >0] lags	0.379	0.270	0.738	0.346	0.814		0.001	0.091	0.983	0.000	0.000		0.053	0.585	0.760	0.221	0.000	
P of Law _t +1[M _t >0] lags	0.001	0.000	0.866	0.000	0.000		0.894	0.871	0.914	0.000	0.012		0.000	0.000	0.539	0.000	0.000	
Typical Law _t effect	0.000	0.000	-0.001	0.000	0.000		0.000	-0.003	0.002	0.001	0.001		0.000	0.001	-0.002	0.000	0.000	
Unconditional effect - progressive	0.000	0.000	-0.002	0.000	0.000		0.000	-0.001	0.003	0.001	0.001		0.000	0.001	0.001	0.000	0.001	
Unconditional effect - conser	0.000	0.000	-0.001	0.000	-0.000		0.001	0.007	0.001	0.000	0.000		0.001	-0.000	0.011	0.001	0.001	
Unconditional effect - all	0.001	0.000	-0.002	0.000	0.000		0.001	0.006	0.004	0.001	0.001		0.001	0.000	0.011	0.001	0.002	
P of 1[M _t >0] leads	0.063	0.466	0.514	0.018	0.041		0.371	0.383	0.999	0.631	0.581		0.122	0.971	0.592	0.203	0.154	
P of Law _t +1[M _t >0] leads	0.178	0.623	0.650	0.329	0.075		0.106	0.176	0.990	0.292	0.371		0.376	0.108	0.831	0.721	0.850	

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

APPENDIX TABLE III
IMPACT OF FREE SPEECH PRECEDENT ON SEXUAL ATTITUDES
ROBUSTNESS OF 2SLS ESTIMATES

The Effect of Appellate Free Speech Precedent on Extramarital Sex is OK			
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
No Circuit-Specific Trends	-0.001	0.394	0.840
No Fixed Effects	0.001	0.001	0.942
State Cluster	0.008	0.057	0.974
No Individual-Level Controls	0.005	0.128	0.905
No Survey Weights	-0.002	0.905	0.901
No Community Standards	0.010	0.002	0.335
No Controls except 1[$M_{ct}>0$]	0.012	0.032	0.769
Drop Circuit 1	0.007	0.107	0.857
Drop Circuit 2	0.013	0.114	0.715
Drop Circuit 3	0.002	0.000	0.947
Drop Circuit 4	0.006	0.442	0.942
Drop Circuit 5	0.006	0.071	0.726
Drop Circuit 6	0.011	0.355	0.961
Drop Circuit 7	0.010	0.019	0.610
Drop Circuit 8	0.004	0.377	0.658
Drop Circuit 9	0.008	0.000	0.063
Drop Circuit 10	0.011	0.000	0.769
Drop Circuit 11	0.004	0.094	0.988
Drop Circuit 12	0.007	0.321	0.832
1 Current 1 Lag	-0.007	0.449	
1 Current 2 Lags	0.006	0.219	
2 Leads 4 Lags	0.006	0.000	0.725
1 Lead 5 Lags	0.006	0.000	0.614
4 Leads 1 Lag	0.004	0.105	0.952

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE IV
IMPACT OF FREE SPEECH PRECEDENT ON SEXUAL ATTITUDES
ROBUSTNESS OF 2SLS DISTRIBUTED LAG ESTIMATES

The Effect of Appellate Free Speech Precedent on Extramarital Sex is OK						
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
No Trends	-0.004 (0.020)	-0.020 (0.021)	-0.010 (0.014)	0.020 (0.016)	0.009 (0.019)	
No FE	-0.002 (0.033)	-0.011 (0.024)	-0.015 (0.029)	0.032 (0.019)	0.001 (0.021)	
State Cluster	-0.001 (0.018)	-0.018 (0.019)	0.018 (0.022)	0.021 (0.028)	0.018 (0.016)	
No Individual-Level Controls	0.002 (0.020)	-0.017 (0.023)	0.003 (0.019)	0.019 (0.022)	0.018 (0.015)	
No Survey Weights	-0.002 (0.018)	-0.017 (0.019)	0.002 (0.019)	0.010 (0.021)	-0.005 (0.017)	
No Community Standards	0.019 (0.019)	-0.031 (0.015)	* 0.038 (0.024)	0.020 (0.025)	0.005 (0.028)	
No Controls except $1[M_{ct}>0]$	0.012 (0.042)	-0.020 (0.014)	0.036 (0.047)	0.034 (0.026)	-0.004 (0.043)	
Drop Circuit 1	-0.002 (0.014)	-0.019 (0.015)	0.022 (0.019)	0.019 (0.020)	0.017 (0.015)	
Drop Circuit 2	-0.005 (0.015)	-0.002 (0.014)	0.031 (0.024)	0.018 (0.021)	0.022 (0.014)	
Drop Circuit 3	0.001 (0.014)	-0.033 (0.015)	* 0.022 (0.016)	0.005 (0.021)	0.015 (0.012)	
Drop Circuit 4	0.001 (0.015)	-0.008 (0.015)	-0.006 (0.028)	0.023 (0.017)	0.019 (0.017)	
Drop Circuit 5	-0.005 (0.015)	-0.011 (0.020)	0.001 (0.024)	0.030 (0.020)	0.015 (0.015)	
Drop Circuit 6	-0.001 (0.020)	0.004 (0.016)	0.037 (0.023)	-0.012 (0.021)	0.029 (0.015)	*
Drop Circuit 7	-0.006 (0.012)	-0.019 (0.023)	0.026 (0.025)	0.022 (0.020)	0.026 (0.023)	
Drop Circuit 8	-0.005 (0.011)	-0.017 (0.013)	0.013 (0.021)	0.015 (0.021)	0.015 (0.022)	
Drop Circuit 9	0.025 (0.013)	+ -0.035 (0.012)	** -0.004 (0.019)	0.031 (0.025)	+ 0.022 (0.019)	
Drop Circuit 10	-0.004 (0.015)	-0.015 (0.014)	0.034 (0.014)	* 0.011 (0.023)	0.027 (0.016)	+
Drop Circuit 11	-0.000 (0.016)	-0.020 (0.019)	0.021 (0.016)	0.005 (0.013)	0.014 (0.016)	
Drop Circuit 12	0.003 (0.012)	-0.018 (0.017)	0.019 (0.020)	0.022 (0.019)	0.011 (0.013)	
1 current 1 lag	-0.021 (0.019)	0.007 (0.039)				
1 current 2 lag	-0.022 (0.018)	0.028 (0.035)	0.013 (0.023)			
2 leads 4 lags	-0.004 (0.017)	-0.015 (0.018)	0.037 (0.020)	+ 0.005 (0.019)	0.009 (0.016)	
1 lead 5 lags	-0.008 (0.015)	-0.012 (0.016)	0.023 (0.018)	0.009 (0.019)	0.005 (0.013)	0.016 (0.011)
4 leads 1 lag	-0.006 (0.027)	0.013 (0.020)	-0.006 (0.016)	0.001 (0.020)	-0.034 (0.021)	0.040 (0.023)

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE V.— The Effects of Free Speech Precedents on Sexual Behaviors

Dependent Variable	Paid Sex					Wild BS					Number of Partners per Year					Wild BS					Number of Female Partners					Wild BS %LE
						%LE										%LE										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)											
Proportion Progressive Free Speech	-0.00176 (0.00216)	0.00381 (0.0142)	-0.00422 (0.00364)	-0.00521* (0.00254)	-0.00279 (0.00249)	0.32 (0.126)	-0.537 (1.020)	0.439* (0.179)	0.0509 (0.201)	0.105 (0.148)	-0.281 (1.827)	-1.281 (4.829)	-1.319 (3.749)	-3.370 (2.265)	-2.207 (2.347)											
Appellate Decisions _{t+1}	0.00600 (0.00360)	-0.00621 (0.0360)	0.0123* (0.00525)	0.00767+ (0.00414)	0.00627+ (0.00341)	0.14 (0.241)	-0.300 (2.608)	-0.159 (0.280)	-0.130 (0.245)	-0.240 (0.220)	3.111 (1.805)	1.702 (24.70)	-0.657 (7.551)	5.432* (2.461)	5.176* (2.130)											
Proportion Progressive Free Speech	-0.000137 (0.00344)	-0.00135 (0.0104)	0.00212 (0.00511)	0.00266 (0.00493)	0.00115 (0.00355)	0.72 (0.405)	1.291* (0.615)	0.994* (0.423)	1.177** (0.434)	0.861* (0.400)	3.829* (1.280)	-0.0335 (8.566)	8.222** (2.253)	6.648** (2.178)	7.772** (1.668)											
Appellate Decisions _{t-1}	0.00632** (0.00201)	0.0218 (0.0155)	0.00731+ (0.00431)	0.00631* (0.00272)	0.00713** (0.00203)	0.15 (0.192)	-0.558 (1.068)	0.421+ (0.220)	0.0901 (0.187)	0.0954 (0.171)	3.262+ (1.526)	2.834 (3.765)	9.065+ (5.381)	4.172+ (2.402)	4.958* (1.985)											
Appellate Decisions _{t-2}	0.00499* (0.00223)	0.0205 (0.0207)	0.00525 (0.00362)	0.00531+ (0.00319)	0.00584** (0.00222)	0.08 (0.196)	-0.198 (1.223)	-0.767 (0.375)	0.0709 (0.145)	-0.131 (0.166)	0.780 (0.927)	3.416 (6.657)	8.824* (4.291)	2.078 (1.743)	4.019+ (2.132)											
Proportion Progressive Free Speech	-0.000925 (0.00208)	-0.00612 (0.00676)	0.00109 (0.00206)	0.000490 (0.00280)	0.000132 (0.00261)	0.88 (0.111)	0.593 (0.800)	0.104 (0.159)	0.108 (0.123)	0.0724 (0.107)	1.268 (0.938)	-1.661 (9.933)	1.004 (2.663)	2.284+ (1.262)	3.217** (1.133)											
N	16659	16659	16659	16659	16659	15346	15346	15346	15346	15346	13833	13833	13833	13833	13833											
R-sq	0.002	0.002	0.002	0.002	0.002	0.010	0.010	0.009	0.009	0.010	0.005	0.004	0.003	0.004	0.004											
Appellate IV	N	Y	Y	Y	Y	N	Y	Y	Y	N	N	Y	Y	Y	Y											
District IV	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N											
Aggregation Level	Individual					Individual					Individual					Individual					Individual					
Mean dependent variable	0.003	0.003	0.003	0.003	0.003	1.129	1.129	1.129	1.129	1.129	6.296	6.296	6.296	6.296	6.296											
Average Law _{id} effect	0.003	0.006	0.006	0.004	0.004	0.066	0.517	0.193	0.263	0.132	2.450	1.252	5.292	4.123	5.028											
P-value of Law _{id} lags	0.022	0.075	0.100	0.101	0.001	0.348	0.001	0.000	0.061	0.181	0.095	0.961	0.000	0.003	0.000											
P-value of Law _{id} leads	0.434	0.789	0.247	0.040	0.263	0.306	0.598	0.014	0.800	0.477	0.881	0.791	0.725	0.137	0.347											
Average 1[M _{id} >0] lag	-0.001	-0.002	-0.002	-0.002	-0.001	0.088	-0.012	0.077	0.019	0.069	-0.705	-0.317	-2.419	-1.319	-1.645											
P-value of 1[M _{id} >0] lags	0.129	0.043	0.232	0.062	0.072	0.562	0.110	0.085	0.005	0.351	0.028	0.279	0.001	0.000	0.000											
P of Law _{id} +1[M _{id} >0] lags	0.067	0.478	0.074	0.159	0.008	0.285	0.000	0.000	0.025	0.152	0.142	0.894	0.002	0.037	0.000											
Typical Law _{id} effect	0.000	0.000	0.000	0.000	0.000	0.002	0.013	0.005	0.007	0.003	0.058	0.030	0.126	0.098	0.120											
Unconditional effect - progressive	0.000	0.000	0.000	0.000	0.000	0.004	0.013	0.007	0.007	0.005	0.042	0.022	0.068	0.067	0.080											
Unconditional effect - conser	-0.000	-0.000	-0.000	-0.000	-0.000	0.010	-0.001	0.009	0.002	0.008	-0.079	-0.035	-0.269	-0.147	-0.183											
Unconditional effect - all	-0.000	-0.000	-0.000	-0.000	-0.000	0.014	0.011	0.015	0.009	0.013	-0.037	-0.013	-0.201	-0.080	-0.103											
P of 1[M _{id} >0] leads	0.270	0.409	0.252	0.603	0.238	0.239	0.675	0.293	0.349	0.267	0.235	0.901	0.049	0.496	0.299											
P of Law _{id} +1[M _{id} >0] leads	0.085	0.914	0.126	0.011	0.058	0.171	0.639	0.094	0.443	0.209	0.364	0.789	0.293	0.065	0.094											

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

APPENDIX TABLE VI.— The Effects of Free Speech Precedents on Sexual Behaviors

Dependent Variable	Number of Partners per Year (reported by Men)					Number of Female Partners (reported by Men)					Extramarital Sex (reported by Men)					Wild BS %LE		
	Wild BS %LE					Wild BS %LE												
Proportion Progressive Free Speech	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)			
Appellate Decisions ₊₁	0.160	-2.660	0.749*	-0.0470	0.0501	0.61	1.466	-7.887	-5.880	-5.195	-2.703	0.32	-0.0142	-0.0747	0.00240	-0.0553	-0.0298	0.65
Proportion Progressive Free Speech	(0.356)	(1.862)	(0.368)	(0.469)	(0.376)		(3.835)	(8.287)	(9.012)	(4.170)	(4.139)		(0.0290)	(0.296)	(0.0563)	(0.0423)	(0.0383)	
Appellate Decisions ₁	-0.810	3.451	-0.787+	-0.423	-0.673	0.44	5.722	16.09	3.321	11.27*	10.49*	0.03	0.0705	0.500	0.0251	0.102+	0.0927	0.41
Proportion Progressive Free Speech	(0.561)	(3.125)	(0.442)	(0.589)	(0.535)		(3.374)	(13.45)	(15.71)	(4.980)	(4.136)		(0.0584)	(1.262)	(0.0770)	(0.0589)	(0.0584)	
Appellate Decisions ₋₁	1.858+	2.653	2.266**	2.767**	2.080*	0.33	8.739**	6.962	19.05**	15.42**	16.89**	0.03	0.107*	0.279	0.0872	0.133**	0.122*	0.03
Proportion Progressive Free Speech	(0.904)	(2.246)	(0.934)	(0.991)	(0.909)		(2.669)	(7.593)	(4.855)	(3.767)	(3.390)		(0.0448)	(0.519)	(0.0710)	(0.0517)	(0.0493)	
Appellate Decisions ₋₂	0.0709	0.0437	0.205	0.103	0.185	0.49	10.04**	9.426	18.69+	12.65**	13.62**	0.05	0.0583+	-0.0482	0.110**	0.0892*	0.0774*	0.03
Proportion Progressive Free Speech	(0.349)	(1.627)	(0.467)	(0.315)	(0.321)		(2.280)	(8.386)	(10.42)	(4.910)	(3.846)		(0.0308)	(0.368)	(0.0424)	(0.0370)	(0.0341)	
Appellate Decisions ₋₃	-0.647	-0.307	-1.054	-0.00362	-0.510	0.72	1.633	4.608	17.85*	5.162+	8.658+	0.24	0.0572	-0.100	0.0600	0.0691	0.0667	0.12
Proportion Progressive Free Speech	(0.491)	(1.872)	(0.773)	(0.363)	(0.441)		(1.944)	(5.878)	(8.611)	(2.958)	(4.676)		(0.0434)	(0.354)	(0.0534)	(0.0478)	(0.0501)	
Appellate Decisions ₋₄	0.188	1.425	0.336	0.468	0.306	0.71	2.519	-0.257	4.862	5.619*	7.055**	0.03	-0.0131	-0.0632	-0.0434	-0.00149	-0.0132	0.69
Proportion Progressive Free Speech	(0.298)	(2.206)	(0.304)	(0.328)	(0.275)		(1.886)	(9.863)	(7.326)	(2.416)	(2.031)		(0.0267)	(0.788)	(0.0265)	(0.0328)	(0.0285)	
N	6626	6626	6626	6626	6626		6077	6077	6077	6077	6077		7170	7170	7170	7170	7170	
R-sq	0.023	0.006	0.022	0.022	0.023		0.010	0.008	0.006	0.009	0.009		0.010	.	0.010	0.010	0.010	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV		N	Y	Y	Lasso IV	Lasso IV		N	Y	Y	Lasso IV	Lasso IV	
District IV	N	N	N	Lasso IV	Lasso IV		N	N	N	Lasso IV	Lasso IV		N	N	N	Lasso IV	Lasso IV	
Aggregation Level	Individual					Individual					Individual							
Mean dependent variable	1.421	1.421	1.421	1.421	1.421		14.041	14.041	14.041	14.041	14.041		0.161	0.161	0.161	0.161	0.161	
Average Law _a effect	0.134	1.453	0.193	0.582	0.278		5.730	7.366	12.756	10.025	11.342		0.056	0.113	0.048	0.077	0.069	
P-value of Law _a lags	0.095	0.581	0.000	0.016	0.017		0.001	0.049	0.000	0.000	0.000		0.014	0.968	0.000	0.003	0.003	
P-value of Law _a leads	0.662	0.153	0.042	0.920	0.894		0.709	0.341	0.514	0.213	0.514		0.635	0.801	0.966	0.192	0.437	
Average 1[M _a >0] lags	0.237	-0.154	0.231	0.073	0.185		-1.190	-1.596	-5.435	-2.643	-3.167		-0.023	-0.027	-0.021	-0.030	-0.027	
P-value of 1[M _a >0] lags	0.241	0.465	0.090	0.004	0.055		0.008	0.053	0.000	0.000	0.000		0.029	0.919	0.009	0.000	0.000	
P of Law _a +1[M _a >0] lags	0.008	0.003	0.000	0.001	0.000		0.005	0.149	0.000	0.010	0.000		0.000	0.989	0.000	0.000	0.000	
Typical Law _a effect	0.003	0.036	0.005	0.015	0.007		0.140	0.179	0.311	0.244	0.276		0.001	0.003	0.001	0.002	0.002	
Unconditional effect - progressive	0.009	0.033	0.011	0.016	0.012		0.111	0.141	0.178	0.180	0.199		0.001	0.002	0.001	0.001	0.001	
Unconditional effect - conser	0.027	-0.018	0.027	0.008	0.021		-0.134	-0.179	-0.611	-0.297	-0.356		-0.003	-0.003	-0.002	-0.003	-0.003	
Unconditional effect - all	0.037	0.015	0.037	0.025	0.033		-0.023	-0.039	-0.433	-0.117	-0.157		-0.002	-0.001	-0.002	-0.002	-0.002	
P of 1[M _a >0] leads	0.337	0.259	0.816	0.349	0.336		0.145	0.726	0.147	0.340	0.180		0.008	0.892	0.003	0.013	0.001	
P of Law _a +1[M _a >0] leads	0.357	0.207	0.135	0.716	0.490		0.604	0.281	0.269	0.109	0.161		0.077	0.809	0.225	0.005	0.034	

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

APPENDIX TABLE VII.— The Effects of Free Speech Precedents on Sexual Behaviors

Dependent Variable	Divorced or Separated (older than 40)					Divorced or Separated (40 or younger)					Premarital Sex is OK (reported by Women)					Wild BS		
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE	(11)	(12)	(13)	(14)	(15)	Wild BS %LE
Proportion Progressive Free Speech	0.0291 (0.0192)	0.142 (0.159)	0.0320 (0.0309)	-0.00201 (0.0462)	0.0206 (0.0303)	0.34	-0.0462+ (0.0213)	0.0873 (0.140)	-0.0582 (0.0730)	-0.0873* (0.0418)	-0.0442 (0.0357)	0.17	-0.00836 (0.0304)	-0.356 (1.429)	0.571 (0.906)	0.0100 (0.0486)	0.00147 (0.0478)	0.70
Appellate Decisions _{t+1}	0.0426 (0.0329)	0.124 (0.0841)	0.100* (0.0482)	0.0653 (0.0445)	0.0669+ (0.0391)	0.21	0.00387 (0.0298)	-0.139 (0.109)	-0.0853 (0.0798)	0.0177 (0.0457)	0.0134 (0.0429)	0.76	0.0682 (0.0434)	-0.0316 (0.309)	-0.444 (4.003)	0.0710+ (0.0417)	0.0685 (0.0443)	0.48
Proportion Progressive Free Speech	-0.0253 (0.0222)	-0.0246 (0.0993)	-0.0456 (0.0414)	-0.00278 (0.0664)	-0.0216 (0.0378)	0.87	-0.0986* (0.0382)	0.161 (0.267)	-0.0820 (0.0503)	-0.128+ (0.0723)	-0.109* (0.0519)	0.22	-0.0513 (0.0336)	0.365 (0.713)	-0.290 (2.947)	-0.0457 (0.0341)	-0.0537 (0.0329)	0.21
Appellate Decisions _{t-1}	0.0386 (0.0358)	0.0655 (0.135)	-0.00602 (0.0376)	0.0594 (0.0385)	0.0632+ (0.0384)	0.22	0.0280 (0.0235)	-0.180 (0.315)	-0.0920 (0.0569)	0.0126 (0.0177)	0.0428+ (0.0236)	0.33	0.0121 (0.0286)	-0.472 (1.492)	0.426 (1.876)	0.0190 (0.0358)	0.0499 (0.0402)	0.52
Proportion Progressive Free Speech	-0.0247 (0.0257)	0.0593 (0.152)	0.0641 (0.0563)	-0.0970+ (0.0562)	-0.0674* (0.0330)	0.29	-0.0185 (0.0359)	0.126 (0.208)	-0.104 (0.0750)	-0.0845 (0.108)	-0.0820 (0.0685)	0.28	0.000166 (0.0274)	0.131 (0.414)	0.453 (5.765)	0.0431 (0.0325)	0.0515 (0.0345)	0.49
Appellate Decisions _{t-3}	0.0122 (0.0210)	-0.00984 (0.116)	0.0292 (0.0441)	0.00618 (0.0303)	0.0162 (0.0336)	0.76	-0.0137 (0.0287)	0.168 (0.204)	-0.0568 (0.0389)	-0.0668 (0.0500)	-0.0608 (0.0410)	0.25	-0.00355 (0.0201)	0.338 (0.478)	-0.0105 (3.779)	0.0276 (0.0192)	-0.00211 (0.0186)	0.99
Appellate Decisions _{t-4}																		
N	10778	10778	10778	10778	10778		6368	6368	6368	6368	6368		10693	10693	10693	10693	10693	
R-sq	0.035	0.031	0.032	0.033	0.034		0.021		0.015	0.019	0.019		0.036			0.036	0.036	
Appellate IV																		
District IV	N	Y	N	Y	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
Aggregation Level	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Mean dependent variable	0.237	0.237	0.237	0.237	0.237		0.174	0.174	0.174	0.174	0.174		0.585	0.585	0.585	0.585	0.585	
Average Law _{id} effect	0.009	0.043	0.028	0.006	0.011		-0.020	0.027	-0.084	-0.050	-0.039		0.005	0.066	0.027	0.023	0.023	
P-value of Law _{id} lags	0.460	0.674	0.000	0.012	0.008		0.060	0.123	0.000	0.015	0.003		0.324	0.638	0.994	0.001	0.000	
P-value of Law _{id} leads	0.157	0.370	0.301	0.965	0.496		0.053	0.534	0.425	0.037	0.216		0.788	0.803	0.528	0.836	0.975	
Average 1[M _{id} >0] lag	-0.018	-0.030	-0.020	-0.023	-0.021		0.016	0.014	0.040	0.020	0.016		0.002	0.020	0.029	-0.002	-0.004	
P-value of 1[M _{id} >0] lags	0.006	0.072	0.006	0.003	0.000		0.100	0.009	0.000	0.022	0.026		0.002	0.675	1.000	0.000	0.000	
P of Law _{id} +1[M _{id} >0] lags	0.003	0.592	0.000	0.049	0.001		0.045	0.377	0.010	0.003	0.000		0.105	0.499	0.934	0.301	0.430	
Typical Law _{id} effect	0.000	0.002	0.002	0.000	0.001		-0.001	0.002	-0.005	-0.003	-0.002		0.000	0.003	0.001	0.001	0.001	
Unconditional effect - progressive	-0.001	0.001	-0.001	-0.001	-0.001		-0.000	0.003	-0.003	-0.002	-0.002		0.000	0.004	0.003	0.001	0.001	
Unconditional effect - conser	-0.003	-0.006	-0.004	-0.004	-0.004		0.003	0.003	0.008	0.004	0.003		0.000	0.004	0.006	-0.000	-0.001	
Unconditional effect - all	-0.004	-0.005	-0.003	-0.005	-0.004		0.003	0.005	0.005	0.002	0.002		0.001	0.008	0.008	0.001	0.000	
P of 1[M _{id} >0] leads	0.255	0.606	0.218	0.145	0.194		0.403	0.888	0.640	0.277	0.728		0.964	0.808	0.952	0.590	0.828	
P of Law _{id} +1[M _{id} >0] leads	0.014	0.302	0.024	0.409	0.108		0.044	0.441	0.361	0.011	0.079		0.762	0.802	0.911	0.939	0.978	

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

APPENDIX TABLE VIII
IMPACT OF FREE SPEECH PRECEDENT ON SEXUAL BEHAVIORS
ROBUSTNESS OF 2SLS ESTIMATES

The Effect of Appellate Free Speech Precedent on Paid Sex			
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
No Circuit-Specific Trends	0.001	0.218	0.530
No Fixed Effects	0.000	0.007	0.816
State Cluster	0.003	0.121	0.186
No Individual-Level Controls	0.003	0.000	0.136
No Survey Weights	0.006	0.001	0.018
No Community Standards	0.004	0.002	0.274
No Controls except 1[$M_{ct}>0$]	0.000	0.029	0.834
Drop Circuit 1	0.004	0.074	0.044
Drop Circuit 2	0.003	0.247	0.004
Drop Circuit 3	0.006	0.000	0.157
Drop Circuit 4	0.002	0.001	0.625
Drop Circuit 5	0.002	0.005	0.352
Drop Circuit 6	0.005	0.000	0.264
Drop Circuit 7	0.002	0.000	0.063
Drop Circuit 8	0.005	0.007	0.039
Drop Circuit 9	0.003	0.000	0.303
Drop Circuit 10	0.004	0.072	0.246
Drop Circuit 11	0.001	0.008	0.421
Drop Circuit 12	0.004	0.082	0.062
1 Current 1 Lag	0.002	0.386	
1 Current 2 Lags	-0.000	0.203	
2 Leads 4 Lags	0.004	0.036	0.289
1 Lead 5 Lags	0.001	0.000	0.236
4 Leads 1 Lag	0.004	0.163	0.367

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE IX
IMPACT OF FREE SPEECH PRECEDENT ON SEXUAL BEHAVIORS
ROBUSTNESS OF 2SLS DISTRIBUTED LAG ESTIMATES

	The Effect of Appellate Free Speech Precedent on Paid Sex					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
No Trends	-0.002 (0.003)	0.002 (0.002)	0.001 (0.004)	0.001 (0.003)	0.003 (0.002)	
No FE	-0.000 (0.002)	-0.001 (0.001)	-0.002 (0.004)	0.001 (0.002)	0.003 (0.003)	
State Cluster	-0.005 (0.004)	0.008 (0.004)	+	0.003 (0.005)	0.006 (0.003)	0.005 (0.005)
No Individual-Level Controls	-0.006 (0.004)	0.007 (0.004)	0.004 (0.005)	0.005 (0.003)	0.004 (0.002)	+
No Survey Weights	-0.006 (0.003)	* 0.008 (0.004)	* 0.007 (0.005)	0.007 (0.003)	* 0.012 (0.003)	**
No Community Standards	-0.003 (0.003)	0.007 (0.004)	+	0.002 (0.004)	0.007 (0.002)	**
No Controls except 1[$M_{ct}>0$]	-0.000 (0.002)	0.003 (0.001)	* -0.003 (0.004)	0.002 (0.002)	0.001 (0.002)	
Drop Circuit 1	-0.005 (0.003)	* 0.008 (0.004)	+	0.003 (0.005)	* 0.007 (0.003)	+
Drop Circuit 2	-0.006 (0.002)	** 0.008 (0.005)	+	0.004 (0.005)	** 0.006 (0.003)	0.005 (0.003)
Drop Circuit 3	-0.004 (0.002)	0.013 (0.005)	**	0.006 (0.003)	+	0.007 (0.002)
Drop Circuit 4	-0.001 (0.002)	0.003 (0.002)	+	-0.001 (0.003)	0.005 (0.002)	0.004 (0.002)
Drop Circuit 5	-0.004 (0.004)	0.007 (0.003)	* -0.004 (0.004)	0.007 (0.004)	0.001 (0.004)	
Drop Circuit 6	-0.006 (0.006)	0.010 (0.006)	+	0.004 (0.004)	0.007 (0.003)	0.010 (0.002)
Drop Circuit 7	-0.005 (0.003)	+	0.003 (0.004)	0.002 (0.004)	0.005 (0.002)	+
Drop Circuit 8	-0.007 (0.003)	* 0.011 (0.005)	* 0.008 (0.004)	* 0.006 (0.003)	* 0.006 (0.004)	
Drop Circuit 9	-0.002 (0.002)	0.003 (0.003)	0.001 (0.004)	0.008 (0.003)	0.006 (0.002)	**
Drop Circuit 10	-0.003 (0.002)	0.007 (0.004)	0.003 (0.005)	0.007 (0.003)	0.007 (0.003)	*
Drop Circuit 11	-0.002 (0.003)	0.004 (0.004)	-0.003 (0.005)	0.005 (0.002)	0.002 (0.003)	
Drop Circuit 12	-0.005 (0.002)	+	0.008 (0.004)	+	0.003 (0.005)	+
1 current 1 lag	0.004 (0.004)	0.000 (0.004)				
1 current 2 lag	0.003 (0.003)	-0.001 (0.004)	-0.003 (0.003)			
2 leads 4 lags	-0.003 (0.003)	0.009 (0.004)	* 0.002 (0.005)	0.007 (0.003)	0.005 (0.003)	+
1 lead 5 lags	-0.003 (0.002)	0.005 (0.003)	-0.001 (0.004)	0.003 (0.003)	0.003 (0.003)	-0.002 (0.003)
4 leads 1 lag	0.004 (0.004)	0.003 (0.003)	0.002 (0.003)	0.003 (0.006)	0.004 (0.003)	-0.001 (0.004)

Notes: Significant at +10%, *5%, **1%. Data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE X.— The Effects of Free Speech Precedents on Crimes

<i>Dependent Variable</i>	Offenses Against Family and Children per 100,000										Community Vices per 100,000									
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE		(6)	(7)	(8)	(9)	(10)	Wild BS %LE							
Proportion Progressive Free Speech	-19.09	-75.89	-35.61	-56.89+	0.744	0.39		-4.471	33.69+	7.843	18.78+	36.06	0.39							
Appellate Decisions _{t+1}	(12.91)	(59.36)	(43.93)	(32.38)	(39.84)			(3.492)	(20.24)	(23.39)	(9.633)	(41.16)								
Proportion Progressive Free Speech	-5.989	-54.85**	-19.10	-51.84**	-63.15	0.50		1.028	12.31	18.49	14.74	-5.061	0.74							
Appellate Decisions _t	(6.722)	(4.151)	(58.25)	(15.68)	(55.22)			(5.325)	(13.07)	(14.92)	(10.90)	(36.76)								
Proportion Progressive Free Speech	-18.87	-61.20**	-121.6+	-69.98**	-48.80	0.14		0.408	0.995	15.57	5.398	53.61	0.18							
Appellate Decisions _{t-1}	(12.41)	(8.438)	(66.10)	(6.784)	(61.30)			(2.160)	(5.901)	(21.12)	(3.501)	(40.67)								
Proportion Progressive Free Speech	-13.48	-46.39**	4.754	-55.26**	-46.01	0.85		1.254	11.29	-10.05	3.989	-15.48	0.37							
Appellate Decisions _{t-2}	(7.642)	(10.28)	(54.46)	(10.74)	(38.04)			(4.656)	(11.88)	(27.92)	(8.726)	(29.16)								
Proportion Progressive Free Speech	-12.75	-35.52+	-66.43*	-33.32+	-47.07	0.03		-2.548	0.164	2.311	2.260	18.83	0.82							
Appellate Decisions _{t-3}	(7.441)	(18.39)	(28.86)	(18.04)	(35.18)			(3.581)	(11.23)	(12.32)	(10.81)	(26.28)								
Proportion Progressive Free Speech	-3.920	-24.98	-35.53	-18.01	-77.34	0.84		6.403	23.44*	16.78	24.79*	-36.91	0.85							
Appellate Decisions _{t-4}	(6.687)	(16.04)	(35.03)	(22.51)	(74.70)			(5.063)	(9.460)	(20.89)	(10.81)	(69.17)								
N	43992	43992	43992	43992	43992			43992	43992	43992	43992	43992								
R-sq	0.206	0.189	0.175	0.192	0.182			0.146	0.135	0.140	0.140	0.105								
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV		N	Y	Y	Lasso IV	Lasso IV								
District IV	N	N	N	Lasso IV	Lasso IV	Lasso IV		N	N	Lasso IV	Lasso IV	Lasso IV								
Aggregation Level				ORI Agency - Year	Lasso IV	Lasso IV				ORI Agency - Year	Lasso IV	Lasso IV								
Mean dependent variable	46.063	46.063	46.063	46.063	46.063			5.104	5.104	5.104	5.104	5.104								
Average Law_a effect	-11.002	-44.588	-47.575	-45.683	-56.475			1.309	9.641	8.620	10.235	2.998								
P-value of Law_a lags	0.422	0.000	0.000	0.000	0.001			0.094	0.000	0.000	0.000	0.081								
P-value of Law _a leads	0.170	0.201	0.418	0.079	0.985			0.229	0.096	0.737	0.051	0.381								
Average 1 <i>M_a</i> >0 lags	8.466	21.077	21.449	21.549	18.459			-0.876	-4.138	-5.715	-4.176	-5.316								
P-value of 1 <i>M_a</i> >0 lags	0.078	0.000	0.004	0.000	0.000			0.156	0.016	0.000	0.019	0.256								
P of Law _a +1 <i>M_a</i> >0 lags	0.905	0.000	0.036	0.000	0.115			0.001	0.000	0.103	0.002	0.346								
Typical Law _a effect	-0.515	-2.089	-2.229	-2.140	-2.646			0.061	0.452	0.404	0.480	0.140								
Unconditional effect - progressive	-0.127	-1.177	-1.308	-1.209	-1.904			0.022	0.276	0.145	0.303	-0.116								
Unconditional effect - conser	0.977	2.432	2.475	2.486	2.130			-0.101	-0.477	-0.659	-0.482	-0.613								
Unconditional effect - all	0.831	1.262	1.182	1.286	0.289			-0.078	-0.206	-0.505	-0.184	-0.705								
P of 1 <i>M_a</i> >0 leads	0.426	0.244	0.703	0.092	0.754			0.386	0.188	0.737	0.115	0.585								
P of Law _a +1 <i>M_a</i> >0 leads	0.036	0.189	0.446	0.108	0.833			0.263	0.057	0.813	0.075	0.491								

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (circuit average response to whether sexual materials lead to a breakdown of morals), and state controls: percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Population weights are population reporting to ORI agency.

APPENDIX TABLE XI.— The Effects of Free Speech Precedents on Crimes

<i>Dependent Variable</i>	Drug Violations per 100,000					Forcible Rapes per 100,000					Wild BS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Proportion Progressive Free Speech	12.59	254.6	-74.52	144.0	105.7	2.231+	6.604	-0.923	14.60	0.838	0.99	
Appellate Decisions _{t-1}	(22.86)	(176.1)	(156.3)	(98.99)	(332.2)	(1.220)	(4.384)	(13.03)	(5.805)			
Proportion Progressive Free Speech	58.97	126.0+	272.4+	141.7**	62.82	-0.648	4.394	8.918	11.18	9.335	0.02	
Appellate Decisions _t	(41.18)	(68.06)	(144.5)	(48.43)	(221.5)	(0.867)	(3.218)	(8.373)	(15.11)	(7.986)		
Proportion Progressive Free Speech	10.92	37.83	-19.57	56.69	294.1	-0.105	4.935	3.665	11.92	2.979	0.58	
Appellate Decisions _{t-1}	(39.35)	(31.15)	(212.1)	(36.78)	(397.7)	(2.245)	(5.333)	(10.14)	(8.537)	(11.75)		
Proportion Progressive Free Speech	3.219	10.45	-10.53	-4.894	-69.43	-0.273	4.122	2.749	12.37	2.301	0.53	
Appellate Decisions _{t-2}	(22.50)	(50.28)	(197.1)	(43.31)	(201.2)	(0.948)	(4.242)	(5.573)	(10.11)	(6.752)		
Proportion Progressive Free Speech	30.58	67.50	36.36	65.38	127.1	0.56	8.496	-4.052	7.324	-4.044	0.36	
Appellate Decisions _{t-3}	(24.21)	(49.49)	(86.60)	(41.53)	(183.3)	(1.084)	(5.570)	(6.101)	(9.786)	(5.153)		
Proportion Progressive Free Speech	51.09	105.2*	174.4*	115.8*	-236.9	-1.510	1.123	1.764	4.129	0.380	0.91	
Appellate Decisions _{t-4}	(36.39)	(47.47)	(81.75)	(52.21)	(376.2)	(1.577)	(4.068)	(4.745)	(8.157)	(3.639)		
N	43992	43992	43992	43992	43992	67017	67017	67017	67017	67017		
R-sq	0.335	0.323	0.322	0.329	0.302	0.077	0.051	0.039		0.043		
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	
District IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	
Aggregation Level	ORI Agency - Year											
Mean dependent variable	286.987	286.987	286.987	286.987	286.987	10.044	10.044	10.044	10.044	10.044		
Average Law_{ct} effect	30.956	69.391	90.613	74.925	35.542	-0.413	4.614	2.609	9.385	2.190		
P-value of Law_{ct} lags	0.038	0.002	0.000	0.000	0.002	0.367	0.268	0.103	0.000	0.268		
P-value of Law _{ct} leads	0.594	0.148	0.633	0.146	0.750	0.097	0.154	0.833	0.262	0.885		
Average 1[$M_{ct} > 0$] lag	-20.745	-42.342	-61.412	-42.898	-44.445	0.035	-1.643	-0.985	-3.534	-1.001		
P-value of 1[$M_{ct} > 0$] lags	0.001	0.000	0.000	0.003	0.038	0.200	0.044	0.252	0.515	0.425		
P of Law _{ct} +1[$M_{ct} > 0$] lags	0.016	0.256	0.005	0.001	0.269	0.536	0.309	0.004	0.000	0.008		
Typical Law _{ct} effect	1.450	3.251	4.245	3.510	1.665	-0.027	0.301	0.170	0.612	0.143		
Unconditional effect - progressive	0.511	1.355	1.462	1.604	-0.446	-0.027	0.216	0.118	0.425	0.086		
Unconditional effect - conser	-2.394	-4.886	-7.086	-4.950	-5.128	0.006	-0.290	-0.174	-0.625	-0.177		
Unconditional effect - all	-1.848	-3.482	-5.520	-3.311	-5.402	-0.019	-0.085	-0.061	-0.217	-0.092		
P of 1[$M_{ct} > 0$] leads	0.240	0.154	0.898	0.107	0.626	0.241	0.264	0.444	0.350	0.769		
P of Law _{ct} +1[$M_{ct} > 0$] leads	0.042	0.198	0.352	0.376	0.870	0.294	0.128	0.850	0.239	0.749		

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and state controls: percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Population weights are population reporting to ORI agency.

APPENDIX TABLE XII.— The Effects of Free Speech Precedents on Property Crimes

<i>Dependent Variable</i>	Property Crimes per 100,000					Wild BS %LE
	(1)	(2)	(3)	(4)	(5)	
Proportion Progressive Free Speech	27.89 (16.29)	-51.91 (73.69)	-91.47 (200.5)	136.3 (161.4)	-102.8 (195.3)	0.51
Appellate Decisions _{t+1}						
Proportion Progressive Free Speech	1.663 (18.65)	-54.87 (42.31)	-43.15 (181.7)	143.2 (207.1)	-60.04 (188.9)	0.50
Appellate Decisions _t						
Proportion Progressive Free Speech	-16.41 (20.13)	-82.48+ (49.50)	-129.8 (183.0)	119.3 (133.9)	-117.4 (187.2)	0.39
Appellate Decisions _{t-1}						
Proportion Progressive Free Speech	-25.82+ (13.66)	-83.96 (59.70)	18.26 (183.2)	121.7 (132.5)	42.38 (199.9)	0.64
Appellate Decisions _{t-2}						
Proportion Progressive Free Speech	-14.01 (15.64)	-54.52 (55.03)	-215.0 (163.7)	94.86 (147.2)	-231.1 (182.8)	0.10
Appellate Decisions _{t-3}						
Proportion Progressive Free Speech	-34.48* (14.05)	-22.32 (59.65)	-122.5 (139.2)	3.649 (122.3)	-115.0 (163.8)	0.47
Appellate Decisions _{t-4}						
N	67017	67017	67017	67017	67017	
R-sq	0.228	0.224	0.210	0.213	0.206	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level			ORI Agency - Year			
Mean dependent variable	559.876	559.876	559.876	559.876	559.876	
Average Law_{ct} effect	-17.811	-59.631	-98.440	96.546	-96.232	
P-value of Law_{ct} lags	0.205	0.438	0.241	0.733	0.769	
P-value of Law _{ct} leads	0.118	0.481	0.648	0.399	0.598	
Average 1[<i>M_{ct}</i> >0] lag	-3.557	13.374	28.689	-44.527	29.720	
P-value of 1[<i>M_{ct}</i> >0] lags	0.161	0.337	0.557	0.490	0.758	
P of Law _{ct} +1[<i>M_{ct}</i> >0] lags	0.173	0.009	0.032	0.780	0.835	
Typical Law _{ct} effect	-1.161	-3.887	-6.416	6.293	-6.272	
Unconditional effect - progressive	-1.551	-3.358	-5.063	3.776	-4.828	
Unconditional effect - conser	-0.629	2.364	5.070	-7.869	5.253	
Unconditional effect - all	-1.995	-0.750	0.311	-4.149	0.697	
P of 1[<i>M_{ct}</i> >0] leads	0.375	0.691	0.750	0.543	0.671	
P of Law _{ct} +1[<i>M_{ct}</i> >0] leads	0.241	0.400	0.571	0.358	0.556	

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and state controls: percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Population weights are population reporting to ORI agency.

APPENDIX TABLE XIII
IMPACT OF FREE SPEECH PRECEDENT ON CRIMES
ROBUSTNESS OF 2SLS ESTIMATES

The Effect of Appellate Free Speech Precedent on Offenses Against Family and Children per 100,000			
	Average of yearly lags (1)	P-value of lags (2)	P-value of leads (3)
No Circuit-Specific Trends	-81.698	0.140	0.156
No Fixed Effects	-63.238	0.714	0.176
State Cluster	-53.458	0.008	0.119
No State-Level Controls	-91.126	0.089	0.404
No Population Weights	-24.107	0.000	0.304
No Community Standards	-53.846	0.000	0.077
No Controls except $1[M_{ct}>0]$	-165.204	0.749	0.382
Drop Circuit 1	-65.941	0.000	0.158
Drop Circuit 2	-54.088	0.000	0.072
Drop Circuit 3	-52.431	0.000	0.033
Drop Circuit 4	-53.162	0.000	0.127
Drop Circuit 5	-52.673	0.000	0.106
Drop Circuit 6	-22.058	0.056	0.816
Drop Circuit 7	-58.951	0.000	0.172
Drop Circuit 8	-9.430	0.026	0.805
Drop Circuit 9	-82.132	0.000	0.173
Drop Circuit 10	-54.119	0.000	0.106
Drop Circuit 11	-50.734	0.000	0.062
Drop Circuit 12	-53.458	0.000	0.079
1 Current 1 Lag	-9.132	0.248	
1 Current 2 Lags	-21.557	0.062	
2 Leads 4 Lags	-65.505	0.000	0.364
1 Lead 5 Lags	-45.856	0.000	0.090
4 Leads 1 Lag	7.297	0.001	0.891

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are population reporting to ORI agency.

APPENDIX TABLE XIV
IMPACT OF FREE SPEECH PRECEDENT ON CRIMES
ROBUSTNESS OF 2SLS DISTRIBUTED LAG ESTIMATES

The Effect of Appellate Free Speech Precedent on Offenses Against Family and Children per 100,000										
	(t0)		(t1)		(t2)		(t3)		(t4)	(t5)
No Trends	-91.353 (64.462)		-81.141 (45.029)	+	-94.558 (38.112)	*	-75.751 (44.801)		-65.686 (54.096)	
No FE	-82.056 (60.700)		-78.434 (62.034)		-75.302 (48.448)		-46.958 (36.288)		-33.439 (27.757)	
State Cluster	-56.888 (36.520)		-51.841 (38.504)		-69.982 (37.600)	+	-55.258 (37.435)		-33.322 (41.573)	
No Ind Control	-101.894 (121.993)		-80.435 (83.931)		-117.014 (117.420)		-90.922 (123.947)		-65.367 (122.816)	
No Weights	-13.422 (13.066)		-16.093 (12.059)		-36.758 (6.881)	**	-38.544 (10.626)		-15.718 (11.695)	
No Community Standards	-58.394 (32.994)	+	-51.890 (15.079)	**	-70.319 (7.617)	**	-55.459 (10.225)	+	-33.165 (18.893)	+
No Controls except 1[$M_{ct}>0$]	-226.714 (259.576)		-191.154 (243.387)		-201.168 (224.136)		-109.214 (155.064)		-97.769 (126.684)	
Drop Circuit 1	-79.711 (56.486)		-63.593 (32.739)	+	-83.160 (17.712)	**	-64.068 (20.529)		-39.174 (21.009)	+
Drop Circuit 2	-59.057 (32.773)	+	-53.648 (15.847)	**	-69.657 (8.054)	**	-57.449 (15.537)	+	-30.632 (18.628)	
Drop Circuit 3	-51.053 (23.966)	*	-42.069 (9.930)	**	-68.778 (5.019)	**	-48.348 (7.475)	*	-51.910 (10.390)	**
Drop Circuit 4	-53.679 (35.170)		-50.913 (18.408)	**	-68.941 (7.055)	**	-52.930 (10.221)		-39.347 (16.099)	*
Drop Circuit 5	-62.407 (38.628)		-52.638 (18.477)	**	-66.414 (8.788)	**	-56.349 (16.076)		-25.557 (20.075)	
Drop Circuit 6	-4.340 (18.612)		-3.666 (15.229)		-31.343 (24.071)		-46.655 (33.380)		-24.286 (36.556)	
Drop Circuit 7	-60.410 (44.221)		-60.801 (24.821)	*	-77.127 (10.951)	**	-58.833 (20.536)		-37.586 (36.401)	
Drop Circuit 8	-8.701 (35.268)		-6.972 (20.811)		-16.677 (17.162)		-21.846 (13.570)		7.046 (15.235)	
Drop Circuit 9	-87.683 (64.317)		-102.192 (115.462)		-96.512 (16.615)	**	-75.410 (68.031)		-48.865 (56.414)	
Drop Circuit 10	-56.827 (35.172)		-52.147 (17.691)	**	-70.156 (7.426)	**	-56.426 (12.664)		-35.038 (17.195)	*
Drop Circuit 11	-49.149 (26.377)	+	-52.186 (15.151)	**	-70.039 (8.674)	**	-50.317 (9.769)	+	-31.980 (17.630)	+
Drop Circuit 12	-56.888 (32.379)	+	-51.841 (15.681)	**	-69.982 (6.784)	**	-55.258 (10.742)	+	-33.322 (18.044)	+
1 current 1 lag	3.662 (9.083)		-21.926 (13.151)	+						
1 current 2 lag	-3.711 (13.626)		-28.316 (10.936)	**	-32.645 (17.248)	+				
2 leads 4 lags	-56.447 (43.201)		-63.901 (27.651)	*	-84.808 (58.359)		-69.766 (44.716)		-52.605 (72.366)	
1 lead 5 lags	-51.692 (30.496)	+	-53.219 (14.185)	**	-70.399 (4.493)	**	-53.089 (12.023)	+	-27.914 (18.456)	-18.82 (22.167)
4 leads 1 lag	20.923 (20.030)		-6.330 (21.678)		-13.216 (25.401)		-24.437 (53.931)		30.848 (27.848)	3.625 (32.504)

Notes: Significant at +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are population reporting to ORI agency.

APPENDIX TABLE XV.— The Effects of Free Speech Precedents on Diseases

Dependent Variable	Chlamydia Incidence					Wild BS					Gonorrhea Incidence					Wild BS					Syphilis Incidence					Wild BS	
						%LE					(7) (8) (9) (10)					%LE					(11) (12) (13) (14) (15)					%LE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)										
Proportion Progressive Free Speech	-11.04	142.5	-171.7	80.06	67.48	2.683	47.26	-306.2	70.32	-245.6	0.17	0.327	-3.205	7.592	-5.412	3.787	0.90										
Appellate Decisions _{t+1}	(13.64)	(137.3)	(549.8)	(148.5)	(100.2)	(10.65)	(41.09)	(2436.5)	(46.22)	(1767.9)		(0.725)	(4.190)	(16.24)	(3.950)	(18.11)											
Proportion Progressive Free Speech	-1.047	186.1*	-71.07	-84.61	249.2*	0.07	4.518	-30.98	133.6+	-47.10	0.78	-0.386	-2.318	10.55	-5.495	9.191	0.31										
Appellate Decisions _t	(14.03)	(94.12)	(818.5)	(374.9)	(115.2)	(10.42)	(66.96)	(816.6)	(71.03)	(630.0)		(0.922)	(6.006)	(13.29)	(5.245)	(11.62)											
Proportion Progressive Free Speech	14.21	70.15	446.0	380.1	209.1	0.40	8.016	44.79	457.1	115.3	391.2	0.33	-1.263	-6.492	6.928	9.680	0.93										
Appellate Decisions _{t-1}	(19.56)	(67.48)	(1431.5)	(247.2)	(194.4)	(11.05)	(75.12)	(2026.4)	(81.69)	(1613.1)		(0.857)	(7.131)	(20.97)	(6.121)	(21.27)											
Proportion Progressive Free Speech	34.45+	43.20	76.93	157.3	-124.4	0.39	20.84	56.46	365.9	99.35	311.6	0.80	-0.878	-7.445	3.459	9.131	0.28										
Appellate Decisions _{t-2}	(17.44)	(207.0)	(320.3)	(158.0)	(304.5)	(13.36)	(61.55)	(1097.6)	(87.67)	(762.3)		(0.848)	(6.115)	(15.45)	(7.685)	(13.62)											
Proportion Progressive Free Speech	3.188	89.09	264.8	102.2	-79.83	0.52	18.67	72.87+	-563.6	118.4	-500.6	0.82	-0.643	-2.442	0.368	-1.975	0.15										
Appellate Decisions _{t-3}	(16.69)	(78.89)	(1192.9)	(431.6)	(257.6)	(12.52)	(38.36)	(2679.5)	(72.55)	(2039.9)		(0.894)	(5.010)	(23.47)	(5.384)	(18.57)											
Proportion Progressive Free Speech	14.34	48.46	-346.0	355.2	-5.852	0.98	12.58	20.34	615.8	83.74	521.4	0.24	-0.228	3.261	0.0919	6.604	0.65										
Appellate Decisions _{t-4}	(17.59)	(117.1)	(925.0)	(329.2)	(193.9)	(11.98)	(36.27)	(2585.3)	(89.54)	(1991.3)		(1.238)	(5.082)	(26.07)	(8.815)	(25.56)											
N	1117	1117	1117	1117	1117	2141	2141	2141	2141	2141	2141	2141	2141	2141	2141	2141											
R-sq	0.736	0.648	0.055	0.369	0.491	0.724	0.707		0.642			0.576	0.528	0.451	0.467	0.412											
Appellate IV																											
District IV																											
Aggregation Level																											
Mean dependent variable	207.509	207.509	207.509	207.509	207.509	243.911	243.911	243.911	243.911	243.911	243.911	6.748	6.748	6.748	6.748	6.748											
Average Law _{ct} effect	13.029	87.392	74.130	182.040	49.636	13.367	40.036	221.957	101.040	186.113		-3.601	-0.243	1.853	1.025	0.681											
P-value of Law _{ct} lags	0.014	0.000	0.979	0.211	0.000	0.404	0.263	0.987	0.027	0.980		0.172	0.946	0.598	0.589	0.756											
P-value of Law _{ct} leads	0.435	0.299	0.755	0.590	0.501	0.842	0.368	0.900	0.199	0.888		0.906	0.609	0.599	0.705	0.562											
Average 1[$M_{ct}>0$] lag	0.754	-34.057	-34.856	-56.527	-23.852	7.277	-5.505	-86.507	-32.242	-67.354		1.070	-0.196	-0.890	-0.458	-0.841											
P-value of 1[$M_{ct}>0$] lags	0.147	0.000	0.507	0.483	0.055	0.477	0.003	0.990	0.159	0.990		0.078	0.966	0.886	0.862	0.599											
P of Law _{ct} +1[$M_{ct}>0$] lags	0.005	0.064	0.998	0.269	0.012	0.268	0.174	0.985	0.067	0.965		0.328	0.331	0.828	0.431	0.619											
Typical Law _{ct} effect	0.519	3.482	2.953	7.252	1.977	1.107	3.316	18.381	8.368	15.413		-0.158	-0.011	0.081	0.045	0.030											
Unconditional effect - progressive	0.654	2.531	1.863	5.955	1.223	2.237	3.742	14.677	7.455	12.869		-0.132	-0.023	0.050	0.030	-0.008											
Unconditional effect - conser	0.069	-3.140	-3.214	-5.212	-2.199	1.145	-0.866	-13.616	-5.075	-10.602		0.102	-0.019	-0.084	-0.043	-0.080											
Unconditional effect - all	0.613	-0.757	-1.384	0.218	-0.991	2.708	2.105	-0.649	1.275	0.596		-0.018	-0.036	-0.035	-0.015	-0.080											
P of 1[$M_{ct}>0$] leads	0.482	0.445	0.957	0.408	0.547	0.163	0.758	0.875	0.946	0.859		0.731	0.477	0.473	0.621	0.577											
P of Law _{ct} +1[$M_{ct}>0$] leads	0.709	0.252	0.756	0.432	0.538	0.184	0.230	0.930	0.091	0.930		0.822	0.773	0.815	0.842	0.718											

Notes: Significant at +10%, *5%, **1%. Data consist of STDs reported by CDC (at the state level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, and a dummy for whether there were any cases in that Circuit-year. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Population weights are state population.

APPENDIX TABLE XVI
IMPACT OF FREE SPEECH PRECEDENT ON SEXUALLY TRANSMITTED DISEASE
ROBUSTNESS OF 2SLS ESTIMATES

The Effect of Appellate Free Speech Precedent on Chlamydia Incidence			
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
No Circuit-Specific Trends	11.432	0.003	0.235
No Fixed Effects	529.154	0.107	0.911
State Cluster	127.014	0.038	0.422
No State-Level Controls	127.014	0.211	0.590
No Population Weights	27.185	0.000	0.000
No Community Standards	64.303	0.000	0.501
No Controls except $1[M_{ct}>0]$	-5.5e+03	1.000	0.998
Drop Circuit 1	94.326	0.033	0.516
Drop Circuit 2	196.974	0.737	0.758
Drop Circuit 3	153.973	0.660	0.744
Drop Circuit 4	110.036	0.000	0.442
Drop Circuit 5	122.780	0.000	0.133
Drop Circuit 6	161.737	0.022	0.851
Drop Circuit 7	184.328	0.890	0.652
Drop Circuit 8	183.479	0.000	0.538
Drop Circuit 9	145.875	0.260	0.624
Drop Circuit 10	121.589	0.374	0.634
Drop Circuit 11	123.501	0.117	0.612
Drop Circuit 12	125.999	0.201	0.594
1 Current 1 Lag	64.842	0.010	
1 Current 2 Lags	94.582	0.013	
2 Leads 4 Lags	103.268	0.003	0.869
1 Lead 5 Lags	154.005	0.105	0.581
4 Leads 1 Lag	58.206	0.198	0.800

Notes: Significant at +10%, *5%, **1%. Data consist of STDs reported by CDC (at the state level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE XVII
IMPACT OF FREE SPEECH PRECEDENT ON SEXUALLY TRANSMITTED DISEASE
ROBUSTNESS OF 2SLS DISTRIBUTED LAG ESTIMATES

	The Effect of Appellate Free Speech Precedent on Chlamydia Incidence					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
No Trends	76.737 (64.643)	-68.164 (136.131)	132.431 (120.442)	72.087 (58.873)	-155.931 (168.439)	
No FE	-249.387 (2227.792)	357.966 (1413.695)	617.517 (6323.876)	621.122 (5847.995)	1298.554 (1.3e+04)	
State Cluster	80.057 (99.728)	-84.608 (246.736)	380.074 (201.008)	+	157.336 (140.568)	102.211 (401.541)
No Ind Control	80.057 (148.538)	-84.608 (374.942)	380.074 (247.166)		157.336 (158.006)	102.211 (431.597)
No Weights	98.252 (26.409)	** -33.766 (100.401)	64.317 (112.113)		103.784 (137.105)	** -96.659 (152.501)
No Community Standards	67.484 (100.234)	249.163 (115.192)	* 209.141 (194.402)		-124.440 (304.534)	-79.833 (257.581)
No Controls except 1[$M_{ct}>0$]	27.646 (1.1e+04)	-4.8e+03 (3.7e+04)	-1.6e+04 (1.6e+05)		6040.910 (7.1e+04)	-1.3e+04 (1.3e+05)
Drop Circuit 1	91.313 (140.559)	-106.718 (377.240)	343.021 (282.085)		130.943 (122.709)	13.073 (344.483)
Drop Circuit 2	55.710 (180.691)	-51.334 (479.505)	501.879 (307.110)		181.290 (299.527)	297.327 (693.658)
Drop Circuit 3	51.272 (156.887)	-43.808 (360.315)	387.611 (250.365)		166.862 (222.877)	207.927 (562.718)
Drop Circuit 4	98.989 (128.708)	-67.811 (239.594)	288.579 (194.199)		238.056 (106.679)	-7.634 (280.644)
Drop Circuit 5	113.868 (75.881)	6.009 (62.804)	267.189 (93.561)	**	149.722 (119.749)	77.110 (161.955)
Drop Circuit 6	-24.991 (132.674)	101.893 (186.816)	259.522 (126.155)	*	210.112 (199.859)	262.148 (509.617)
Drop Circuit 7	167.472 (371.653)	-227.734 (1010.913)	621.886 (868.762)		245.392 (251.777)	114.625 (821.254)
Drop Circuit 8	65.767 (106.701)	17.197 (155.400)	353.518 (232.213)		233.533 (165.378)	247.378 (490.652)
Drop Circuit 9	123.099 (251.004)	45.866 (348.536)	104.375 (64.404)		502.363 (782.440)	-46.330 (1480.031)
Drop Circuit 10	72.216 (151.535)	-96.478 (424.803)	388.352 (276.139)		127.264 (194.201)	116.591 (438.138)
Drop Circuit 11	75.270 (148.601)	-83.781 (393.360)	370.289 (229.628)		130.057 (196.068)	125.671 (448.045)
Drop Circuit 12	78.506 (147.357)	-83.422 (371.221)	377.248 (245.059)		155.136 (156.879)	102.528 (426.698)
1 current 1 lag	49.805 (25.427)	+	79.879 (32.424)	*		
1 current 2 lag	69.484 (26.583)	**	63.697 (48.782)	150.566 (64.076)	*	
2 leads 4 lags	50.250 (99.373)	35.067 (137.914)	212.262 (51.812)	**	112.350 (95.930)	106.412 (168.899)
1 lead 5 lags	74.933 (135.812)	-89.322 (190.762)	358.497 (262.285)		161.300 (165.616)	78.745 (189.586)
4 leads 1 lag	-16.827 (44.999)	133.239 (183.604)	-147.527 (250.512)		34.825 (130.513)	30.344 (144.360)
(t0, t1, f4, f3, f2, f1)						339.88 (299.235)
						192.564 (229.162)

Notes: Significant at +10%, *5%, **1%. Data consist of STDs reported by CDC (at the state level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are state population.

APPENDIX TABLE XVIII.— The Effects of Free Speech Precedents over Time

Dependent Variable	1973-1993										1980-2000					
	Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK		Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Proportion Progressive Free Speech	-0.00252 (0.00529)	-0.00526 (0.00426)	7.321+ (3.669)	-3.559 (10.07)	-1.277+ (0.601)	-0.597 (0.510)	-0.0249* (0.00892)	-0.0397 (0.0962)	-0.00153 (0.00228)	0.00277 (0.0125)	-4.355 (3.487)	37.10+ (21.78)	0.0851 (0.133)	-0.466 (0.892)	-0.00427 (0.0151)	0.0854 (0.0706)
Appellate Decisions _{t+1}	0.00397 (0.00433)	-0.00174 (0.00300)	10.71* (4.475)	-4.110 (13.67)	-1.475+ (0.674)	-0.360 (0.338)	0.0113 (0.0219)	-0.0594 (0.104)	0.00605 (0.00352)	-0.00560 (0.0320)	1.015 (5.287)	11.29 (17.10)	-0.308 (0.217)	1.796 (2.292)	-0.0119 (0.0361)	-0.0315 (0.123)
Appellate Decisions _t	-0.00301 (0.00490)	-0.00932* (0.00385)	9.339* (19.53)	-13.48 (19.53)	0.0950 (0.505)	0.509 (0.494)	-0.0186 (0.0169)	-0.0897 (0.0908)	-0.0000587 (0.00339)	-0.00204 (0.0100)	0.512 (2.197)	0.0865 (7.168)	0.733 (0.417)	1.208+ (0.682)	-0.0131 (0.0241)	-0.0623 (0.140)
Proportion Progressive Free Speech	0.00841+ (0.00441)	0.00200 (0.00508)	6.538 (4.924)	5.140 (12.91)	-1.065 (0.639)	-0.807+ (0.464)	-0.00664 (0.0177)	-0.0141 (0.0729)	0.00660* (0.00213)	0.0188 (0.0126)	1.373 (4.586)	10.92 (12.28)	-0.0173 (0.186)	-0.516 (0.836)	0.0215 (0.0237)	0.126 (0.219)
Appellate Decisions _{t-2}	0.00357 (0.00526)	-0.00493 (0.00744)	3.438 (2.724)	-16.55 (13.63)	-0.728 (0.556)	-0.551* (0.254)	-0.0128 (0.0148)	-0.0971 (0.126)	0.00520* (0.00229)	0.0183 (0.0174)	-2.531 (3.577)	-0.394 (11.16)	-0.244 (0.206)	-0.694 (0.994)	-0.0101 (0.0300)	-0.114+ (0.0614)
Proportion Progressive Free Speech	0.00609+ (0.00328)	0.00185 (0.00384)	7.293+ (3.373)	18.76** (5.851)	-0.446 (0.411)	0.365 (0.326)	0.0115 (0.0134)	0.00922 (0.117)	-0.00102 (0.00212)	-0.00566 (0.00572)	6.450 (5.016)	24.00* (10.17)	0.0502 (0.112)	0.545 (0.744)	0.0190 (0.0150)	0.165+ (0.0851)
Appellate Decisions _{t-4}																
N	9969	9969	26961	26961	9392	9392	20930	20930	16659	16659	43992	43992	15346	15346	18073	18073
R-sq	0.003	0.002	0.160	0.156	0.014	0.012	0.044	0.041	0.002	.	0.146	0.133	0.009	0.001	0.057	0.052
Appellate IV	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
District IV	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
Mean dependent variable	0.003	0.003	5.060	5.060	1.130	1.130	0.219	0.219	0.003	0.003	5.104	5.104	1.129	1.129	0.267	0.267
Average Law_{it} effect	0.004	-0.002	7.463	-2.050	-0.724	-0.169	-0.003	-0.050	0.003	0.005	1.364	9.181	0.043	0.468	0.001	0.017
P-value of Law_{it} lags	0.083	0.000	0.108	0.000	0.101	0.047	0.394	0.008	0.036	0.123	0.056	0.050	0.348	0.031	0.771	0.000
P-value of Law _{it} leads	0.643	0.217	0.074	0.724	0.057	0.242	0.018	0.680	0.514	0.824	0.240	0.089	0.535	0.601	0.783	0.227
Average 1 <i>M_{it}</i> >0 lag	-0.004	0.001	-3.616	1.365	0.394	0.358	0.003	0.022	-0.001	-0.002	-0.808	-3.985	0.112	0.003	0.006	-0.002
P-value of 1 <i>M_{it}</i> >0 lags	0.087	0.000	0.202	0.004	0.318	0.164	0.193	0.195	0.129	0.017	0.089	0.025	0.537	0.133	0.064	0.600
P of Law _{it} +1 <i>M_{it}</i> >0 lags	0.831	0.000	0.186	0.007	0.094	0.127	0.182	0.001	0.095	0.450	0.002	0.181	0.313	0.000	0.000	0.000
Typical Law _{it} effect	0.000	-0.000	0.545	-0.150	-0.026	-0.006	-0.000	-0.005	0.000	0.000	0.064	0.430	0.001	0.012	0.000	0.001
Unconditional effect - progressive	0.000	-0.000	0.301	-0.054	-0.012	0.007	-0.000	-0.004	0.000	0.000	0.023	0.260	0.004	0.012	0.000	0.001
Unconditional effect - conser	-0.001	0.000	-0.630	0.238	0.060	0.055	0.001	0.006	-0.000	-0.000	-0.104	-0.460	0.013	0.000	0.001	-0.000
Unconditional effect - all	-0.001	0.000	-0.330	0.180	0.048	0.062	0.001	0.003	-0.000	-0.000	-0.079	-0.203	0.017	0.012	0.002	0.000
P of 1 <i>M_{it}</i> >0 leads	0.801	0.582	0.416	0.768	0.245	0.154	0.714	0.847	0.253	-0.403	0.373	0.192	0.246	0.620	0.111	0.971
P of Law _{it} +1 <i>M_{it}</i> >0 leads	0.110	0.290	0.116	0.938	0.015	0.510	0.111	0.390	0.096	0.891	0.316	0.041	0.206	0.677	0.402	0.104

Notes: Significant at +10%, *5%, **1%. Attitudinal and behavioral data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS. Crime data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether there were any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and state controls: percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Population weights are population reporting to ORI agency.