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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 report four key findings. Democratic judges, more than Republicans, tended to vote progressively in obscenity cases. Such progressive rulings liberalized sexual attitudes and behaviors, increased asymptomatic STDs, but reduced child abuse. The media played a role in transferring legal precedents onto societal values. These results support a model positing laws not only sanction activities but also shape societal norms, especially when these activities become prevalent.

1. Introduction

From environmentalism, to women's liberation, to abolition of slavery, law is speculated to play a key role in moral revolutions (Acemoglu, 2012). 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). We contribute to this discourse by examining a unique case study: obscenity law in the U.S., and its impact on four societal outcomes. Our analysis provides causal evidence on whether obscenity law leads to changes in moral standards,[1] sexual violence,[2] child sexual abuse,[3] and the prevalence of disease and drugs[4] - issues that are frequently invoked by judges as rationale for restricting free speech. Our work builds on the theoretical framework developed by Bénabou and Tirole (2012), which uniquely accounts for both expressive and backlash effects in response to legal changes. According to this model, human behavior is shaped by intrinsic motivations (personal beliefs), extrinsic motivations (such as material incentives or deterrents), and social motivations (the desire to gain honor or avoid stigma). Legal decisions act as information signals that

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[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).
[4] 50 a.m. JUR.2d §§I, 2 (1995).

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influence perceptions of social norms, potentially affecting how individuals judge stigmatized activities. For instance, a legal prohibition may lead individuals to believe that such activities are more common, thereby shifting their moral judgments. We define an "expressive effect" as the influence of law in aligning societal views of morality with legal values, while "backlash" refers to a shift in moral views against those legal values. For instance, if a legal precedent deters a previously scarce activity, it can provoke a backlash as citizens perceive its increased visibility as normalizing the stigmatized activity; however, if the activity is already common, the legal ruling creates honor for those who refrain from censured act, leading to

an expressive effect.

We employ a quasi-experimental research design that approximates the ideal scenario of assigning legal rules at random across jurisdictions, leveraging the random assignment of judges in U.S. Circuit Courts to approximate a legal rule "coin flip." Because each three-judge

panel is assembled via an effectively random process, its ideological composition—proxied by whether judges were appointed by a Democratic or Republican president—provides exogenous variation in whether any given case is decided in a progressive or conservative manner (Sunstein et al., 2006). This is especially consequential in a common law setting, where Circuit Court decisions bind large regions of the country (up to nine states) and carry final authority for the overwhelming majority of cases, given that the U.S. Supreme Court hears fewer than 2% of appeals.

A natural concern is that our findings might merely reflect a broader, time-driven shift toward liberalized attitudes, rather than an effect of legal precedents themselves. By exploiting random panel assignment, however, we can disentangle (i) organic societal change from (ii) changes directly attributable to court decisions. In practical terms, our design compares how attitudes and behaviors evolve *within* the same historical window, depending on the "luck of the draw" in panel composition. This allows us to identify which fluctuations in sexual norms align with exogenous variations in precedent—rather than simply tracking an ongoing cultural trend.

Our study compiles all free speech precedents related to obscenity from 1958 onwards, as collated by Sunstein et al. (2006) and Kastellec (2013). Throughout this time, a distinct dichotomy is observable: Democrats championing freedom of speech and expression, contrasted by Republicans prioritizing the mitigation of secondary harm resulting from free speech. This ideological divide has led to the transformation of First Amendment jurisprudence into a tool of policy enforcement, a process critically referred to as "weaponization".[5] This jurisprudence significantly guides U.S. free speech policy.

Utilizing the quasi-random assignment of judges to cases, we discover that judge assignments influence outcomes in ways that mirror their decision-making tendencies. These are the outcomes that motivated the U.S. federal courts for at least a half-century. Breakdown of moral standards (*Fort Wayne Books v. Indiana*, 489 U.S. 46 (1989)) and secondary effects, such as sexual violence (*Amatel v. Reno*, 156 F.3d 192 (D.C. Cir. 1998)), child sexual abuse (*Ginsberg v. New York*, 390 U.S. 629 (1968)), disease and drugs (50 a. m. JUR.2d §§

I, 2 (1995)) are among the harms that have been commonly cited by judges to justify the exercise of police powers in restricting expressions of obscenity.

Progressive free speech rulings fostered progressive attitudes and actions, while conservative precedents led to reduced sex crimes (except child abuse) and certain STDs, especially asymptomatic STDs. We found an uptick in the acceptability of various sexual behaviors and an increase in sexual activity, alongside a rise in divorce or separation among those over 40, following progressive decisions. Crimes such as prostitution and drug violations also saw an increase, while child abuse cases decreased. Chlamydia incidence, too, was found to rise. The point estimates indicate that progressive free speech rulings liberalized attitudes and behaviors, with an increase of 0.8, 1.4, and 0.3 percentage points in permissibility of extramarital, premarital, and homosexual sex, respectively. Furthermore, these decisions led to a 0.4 percentage point increase in the likelihood of paid sex, increased annual sexual partners for men by 0.28, and lifted the odds of extramarital sex for men by 0.7 percentage points. The rulings also increased the chances of divorce or separation for individuals over 40 by 1.1 percentage points. On the criminal front, progressive obscenity jurisprudence led to a rise in prostitution and drug violations by 3 and 36 arrests per 100,000 population, respectively, but decreased child abuse arrests by 56 per 100,000 population. The incidence of chlamydia increased by 50 cases per state per year. Taken together, the results suggest that progressive free speech rulings have had a broad and substantial impact on both societal attitudes and behaviors.

Our final analysis explores heterogenous effects in line with the theory of law and norms (Bénabou and Tirole, 2012), which we document in the early time period of our data, with backlash effects in the early period but expressive effects in the latter period.[6] We observe both backlash and expressive mechanisms across the domains of attitudes, behaviors, and crime. In earlier years (1973–1993), conservative rulings appear to increase permissive sexual

views and activities, suggesting a backlash dynamic. By later years (1980–2000), progressive precedents instead reinforce liberalizing trends, producing an expressive effect. Crucially, these shifts are evident in GSS-based indicators (such as paid sex) and corresponding arrest data (e.g., prostitution), indicating that the two-phase pattern of backlash and expressive effects arises consistently under different historical norms.

We have employed several robustness checks to ensure the reliability of our findings. Firstly, we conducted a balance test, the purpose of which was to validate the assumption of random case allocation. This test checks for potential confounding variables that could influence both the assignment of judges and the outcome of the case, thereby providing reassurance that any detected effects are indeed attributable to the judge assignment. Secondly, we implemented a placebo check, which asserts that the future assignment of

[5] "How Conservatives Weaponized the First Amendment", *New York Times*, 06/30/2018.

[6] 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.

judges has no impact on past decisions or outcomes. This serves to confirm the temporality of our observed effects, reassuring that it's the judge assignment influencing outcomes and not vice versa. Both of these tests further underscore the validity of our results, strengthening our overall conclusion.

In additional robustness checks, we alternated the collection of control variables to further substantiate the random allocation of cases. We changed the distributed lag structure, which also confirms the temporality of our observed effects. As another layer of validation, we modified the biographical features used to identify exogenous changes in free speech precedent, ensuring our results aren't exclusively dictated by partisan decision-making differences between Democrats and Republicans. Furthermore, we excluded one Circuit at a time to confirm that outliers don't disproportionately sway our results.

Next, we adopted wild bootstrap and simulations assigning legal variations randomly to different Circuits to confirm that our findings aren't coincidentally significant. As per Barrios, Diamond, Imbens, and Kolesar's (2012) suggestion, our leveraging of repeatedly assigning judges to trios with accessible Circuit-year level data leads to valid standard errors and confidence intervals, even when clustering at the Circuit or Circuit-year level. Lastly, we gauged robustness by taking into account potential reactions of litigants to previous year verdicts. We did this through random judge assignments in lower courts (District Courts).

where appeals originate, providing an extra source of external variation for the presence of an appeal. Our results hold firm under these extensive tests.

In our setting, the exclusion restriction assumes that judge assignment affects the outcomes of interest (attitudes, behaviors, and crime rates) only through its impact on the rulings (progressive or conservative) and not through other channels. The exclusion restriction is likely satisfied because the biographical characteristics of judges are unlikely to directly influence societal outcomes, impacting them only through the rulings themselves. This is supported by several factors: (1) News reports typically focus on the Circuit Court rulings rather than individual judges, making the identity of the judge less salient to the public;

(2) Circuit Courts handle thousands of cases each year, and the biographical details of judges assigned to any specific case are unrelated to judges in other cases; and (3) the most salient aspect of the ruling is whether the decision is progressive or conservative, which shapes societal norms. Empirical evidence from Badawi and Chen (2017) further supports this by showing no market reaction to judge identities in the Delaware Court of Chancery, a court closely watched for corporate decisions. Therefore, potential violations of the exclusion restriction are minimal.

Monotonicity in our context means that assigning a more progressive judge to a case would never decrease the likelihood of a progressive ruling (and similarly for conservative judges). This assumption is supported by historical voting patterns, where Democrats, more than Republicans, consistently voted for progressive outcomes in obscenity cases. In the empirical results, we find consistent directional effects of judge assignment on rulings, which is in line with the monotonicity assumption. To further validate this, we find the predictable influence of judge political affiliation on case outcomes (i.e., progressive or conservative precedents) to hold in the two time periods corresponding to our analysis of heterogenous effects, where we find a first stage coefficients are not statistically significantly different from each other.

We further rely on AR (Anderson Rubin) tests as a robustness check to ensure that the identified effects are not sensitive to weak instruments. The AR test is jointly testing two things: (1) The relevance of the instrument: It checks whether the instrument is sufficiently correlated with the endogenous variable. This ensures that the instrument is not weak. (2).

The significance of the endogenous variable in the structural equation: It tests whether the endogenous variable has a meaningful effect on the outcome variable, i.e., whether the coefficient of the endogenous variable is significantly different from zero in the structural model. Because our primary interest lies in comparative statics from the theory, which involves testing hypotheses about how variables affect each other, our focus is on whether the point estimates of the endogenous variables are significantly different from zero. This aligns with what the Anderson-Rubin (AR) test does: it checks whether the coefficient of the endogenous variable in the structural equation is significantly different from zero, effectively testing whether the endogenous variable has an impact in line with the theoretical predictions.

A wide body of literature has explored how laws influence economic outcomes (e.g., labor markets, investment decisions) and has typically relied on observational or descriptive methods. Far fewer studies provide causal evidence on the expressive dimension of law—that is, the extent to which a legal rule shifts social norms or moral beliefs beyond its direct deterrent or economic consequences. Although earlier analyses (e.g., Tyler, 2006) suggest that laws may shape citizens' behavior through moral messaging, much of that literature remains correlational. In this paper, we use quasi-experimental variation from random judicial assignment in U.S. Circuit Courts to more definitively disentangle changes in attitudes and behaviors caused by legal precedents from broader social or economic trends. This design bridges an important gap, advancing the law-and-norms scholarship by providing a causal test of the expressive and backlash effects within a near-randomized framework. In doing so, we demonstrate for the first time how free speech precedents can push moral attitudes and standards in opposite directions, depending on factors such as the prevalence of the activity in question.

In addition to our causal focus, we build on a diverse body of research exploring how court rulings can catalyze shifts in public attitudes—often via media or community channels. Hoekstra (2000), for instance, finds that citizens become more aware of cases originating in their region, spurring changes in local sentiment. Similar observations appear in news coverage of "community standards" rulings on obscenity (e.g., Mead, 2005; Price, 1996). A broader literature underscores how media and civic organizations disseminate court decisions (e.g.,

Clark et al., 2014; Pastor 2007; Eagle 2007; Sandefur 2005; Weinrib 2012). Recent work on the expressive function of law—especially Feldman (2009) and Mulder et al. (2024)—further shows that legal prohibitions do more than merely penalize: they act as societal signals that can realign norms, heighten social enforcement, and reshape intrinsic motivations. Our analysis extends these findings by examining how U.S. obscenity precedents operate as expressive mechanisms, shaping moral behavior both through formal legal rulings and the subsequent efforts of communities and media outlets to publicize them.

Finally, drawing on Bénabou and Tirole (2012), our framework highlights two contrasting ways a new court ruling can influence social norms. When extramarital sex is relatively common, intensified legal or social constraints (e.g., labeling certain sexual behavior as "obscene") reinforce existing condemnation: people who refrain from extramarital sex gain social honor, while those who violate the standard face stigma—a pattern we call the expressive effect. Conversely, if extramarital sex is rare (or thought to be so), a strong prohibition can inadvertently signal that the behavior occurs more frequently than assumed. This "forbidden fruit" phenomenon can lead some individuals to see it as less taboo, or even as newly relevant, thereby pushing norms in the opposite direction, which we term a backlash effect. The remainder of the paper is organized as follows. Section 2 provides historical and legal 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. Section 7 interprets the results through the lens of theory. Section 8 concludes.

2. Background

2.1. Historical and legal context

Throughout history, much controversy has arisen over obscenity. Many countries worried about the possible impact of obscenity have issued a number of regulations, while courts have wrestled with the interpretation and legality of these regulations. As social norms change and technology facilitates broader dissemination of media, obscene content continues to push previously-held boundaries. In India, couples who elope can be stoned and kissing in public has led to charges of obscenity (both constitute a form of speech and expression in its cultural time and space), and the government has authorized the prosecution of Facebook, Yahoo!, and Google over obscene material. In Russia, newly enacted laws have banned obscenities in public performances. As constitutional theorists such as Balkin (2004) point out, technological change produces new forms of social conflict; while earlier free speech theorists were concerned with democratic deliberation (Habermas, 1991), the contemporary goal of free speech is to promote each individual's ability to participate in the growth and development of culture.

More broadly, an open question in international law is whether custom can be shifted in the direction intended by formal institutions (Aldashev et al., 2012). 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 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. Anti-pornography advocates assert that regulation is necessary to communicate social values and protect human welfare. For example, Radin (1996) argues that the failure to regulate pornography would lead to the commodification of the body and endanger women, and the link between commodification and gender violence can be formalized in a model of incomplete contracts (Chen, 2004). Though stressing that morality is not the focus (MacKinnon, 1987), Dworkin and MacKinnon (1988) assertion that pornography should be banned because it undermines women's status and leads to violence against women is consistent with the view that the law is linked to societal attitudes as well as tangible harms.

Observers of legal change recognize the possibility that laws can have effects through the

moral messages that they convey. Segregationists feared that *Brown v. Board of Education* would reduce the indoctrination of racial prejudice among white youth (Walker, 2011). Previous field studies of expressive law (Funk, 2007), expressive externalities (i.e., spillover effects) of law (Fox and Griffin, Jr, 2009), and free speech regulations in particular (Paul et al., 2001), have only been cross-sectional or time-series and have lacked a clear control group. Consistent with court decisions being able to precipitate rapid change, Bailey (2010) documents that following progressive Supreme Court obscenity precedent, state statutes quickly liberalized obscenity regulations.

Based on the theory of expressive law, scholars in a wide range of legal areas have made normative arguments for or against various policies on the basis of their expressive or backlash effects. However, there lacks a clear framework for assessing the likelihood of their occurrence (Lessig, 1998; Ellickson, 1998; Paul et al., 2001). Claims of backlash also exist in almost every area of law or policy: abortion (Pridemore and Freilich, 2007), desegregation (Klarman, 2005), multiculturalism (Mitchell, 2004), globalization (Eckes and Alfred, 2000), environmentalism (Wolf, 1995), voter mobilization (Mann, 2010), private infrastructure investments (Lopez et al., 2009), health care (Mechanic, 2001), Americans with Disabilities Act (Krieger, 2000), and Warren Court (Feld, 2003). The precise conditions under which expressive or backlash effects occur have not been modeled nor empirically tested.

Sexual norms have changed dramatically during the time period of our study. As noted by Fernandez-Villaverde et al. (2014), in 1958, 35% of U.S. women engaged in premarital sex by the age of 19 compared to 75% today. They estimate however, if individuals' moral views had not changed, a little over 50% of U.S. women would have had premarital sex by the age of 19 today. Changes in moral views include: In 1968, only 15% of women had a permissive attitude towards premarital sex, but this increased to 45% by 1983. In 1957, 57% of Americans believed that adults who preferred to be single were "immoral", but today, it 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). The American Association of Retired Persons (AARP), founded in 1958, provided retirees with advice on sexting in 2009.

(Leshnoff, 2011). Fernandez-Villaverde et al. (2014) report that between 1710 and 1750, 69% of all criminal cases in New Haven were for premarital sex, which was punished by fines, jail, and public flogging. Five times in the last 25 years, the South Korean Constitutional Court has decided on the legality of a law that makes adultery a crime, and in the past six years alone, 5500 people have been arrested and arraigned. In 2008, a legal opinion in India held that rape by a father-in-law was simply adultery with coercion, and the woman involved not only brought shame upon the family, but was ordered to leave her husband and live with the rapist (Vatuk, 2008).

These dramatic differences raise the question: can formal institutions shift custom? What causes a rights revolution and what role does the law play? Historical studies of the advent of the sexual revolution 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 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 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[7] that 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. Yet, the 1970 Commission's findings 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 conclusion that "legislation prohibiting the sale, exhibition, or distribution of

sexual materials to consenting adults should be repealed" 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 Thurmond organized the "Fortas Obscene Film Festival," (it featured transvestites) which not only led to the resignation of Justice Fortas but also the nomination of Justice Burger instead, who by 1973 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). Group conflict arises over social preferences and sacred values (Bowles and Polania-Reyes, 2012; Chen, 2006, 2010, 2013; Chen and Martin Schonger, 2013; Chen and Schonger, 2015). The 2016 Republican Party platform has declared, "Current laws on all forms of pornography and obscenity need to be vigorously enforced" and that "Pornography, with its harmful effects, especially on children, has become a public health crisis that is destroying the life of millions".

Little is known about when law causes what is viewed as moral to shift towards or against what the law values. Little is known about what regulations on obscene speech actually do and whether the rationale put forward by policy makers and scholars of female empowerment concerned about the commodification of women and the potential deleterious secondary effects (i.e., sexual violence, child sexual abuse, disease and drugs) are empirically justified. Our study also relates to contemporary debates over same-sex marriage and discrimination, an area of significant social change in recent years. Though we emphasize that our legal cases are about obscenity as defined in its historical context and not gay rights per se, of the 175 free speech cases in our database, 45% mention "gay" or "lesbian; " including the historical euphemism, "pervert," increases the proportion of cases related to homosexuality to 65%. As communities continue to evolve along with conceptions of rights, this model may help explain why, for example, harsh sentencing in gay hate crimes have been feared to lead to backlash.[8] Though this paper answers a question different from that addressed in the

usual research on law and norms, it has the advantage of a relatively clear source variation that allows identification of any effects in a paired lab and field setting.

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.[9] 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

[7] *Stanley v. Georgia* (394 U.S. 557).

[8] http://www.nytimes.com/2012/05/21/nyregion/Some-Gay-Rights-Advocates-Question-Rutgers-Sentencing.html?_r=1&hp.

[9] When 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.

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 of life-tenured judges, numbering roughly 8 to 40 depending on the size of the Circuit.[10] 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 Jasmin (2011) formally tests for randomization with a balance test and shows that case characteristics as determined by District Courts are not correlated with the characteristics of the Courts of Appeals judges assigned to the case. This paper presents a balance test using the lagged assignment of judges to cases. Further details on this randomization check and the randomization mechanism in the federal courts can be found in the Appendix.

2.3. Model

2.3.1. Example

In Bénabou and Tirole (2012)–style models, we can visualize the population's moral attitudes toward a behavior (e.g., extramarital sex) as a bell-shaped distribution. We split this distribution into "doers" (those who engage in the behavior) and "non-doers" (those who abstain). The difference in their average moral types—essentially, how deviant or principled each group appears—defines the social-image gap or stigma/honor gradient.

When a new law (or legal ruling) reveals that more people engage in the activity than previously assumed, it reshapes the "trapezoids" of doers and non-doers:

Left side (Rare Activity → Backlash): If the behavior was seen as a small, extreme tail, learning that "moderate" individuals also participate raises the doers' average moral standing and reduces the gap between doers and non-doers. This shrinking of the gap can reduce stigma or even spark curiosity, leading to backlash, where the behavior becomes less taboo. Right side (Common Activity → Expressive): If the behavior was already widespread, the discovery that it's even more pervasive can make non-doers stand out as an especially virtuous (or principled) minority, increasing their moral distinction from the now-larger doer group. This widening gap creates an expressive effect, where abiding by the law confers

greater honor, and violating it carries more stigma.

In sum, the same legal signal—namely, "there are more doers than you realized"—can narrow the moral gap on the left side (producing backlash) or widen it on the right side (producing an expressive effect), depending on whether the activity was perceived as rare or

common to begin with.

2.3.2. Formal model

We provide a simplified version of the Bénabou and Tirole (2012) model, which identifies three key motivations for human behavior: (1) intrinsic motivations, where individuals act based on their belief that the action is morally right; (2) extrinsic motivations, where actions are driven by material incentives or deterrents; and (3) social motivations, where societal values, norms, and sanctions influence behavior. Individuals gain honor or face stigma depending on how their actions align with societal norms. For instance, drug users may be stigmatized if drug use is uncommon, while large donors to public goods may be honored if such donations are rare. Legal decisions convey information about societal norms and influence the distribution of behaviors within a community. Two perspectives on the impact of law on social motivations arise: (1) the law can align social motivations with its values, reinforcing its deterrent effects (expressive effect), or (2) the law can shift social motivations away from its values, undermining its intended effects (backlash).

Individuals maximize their utility by taking into account several components, as described by the following utility function:

$$U(a) = (v_a + y)\, a - C(a) + e\overline{a} + \mu E(x \mid a)_s$$

v_a represents intrinsic motivation, which ranges from $[\underline{v}, v]$, reflecting the moral or personal satisfaction from taking action a, y denotes the extrinsic payoff, such as material benefits or incentives, $C(a)$ is the cost associated with the action, including the punishment imposed by the legal sanction, $e\overline{a}$ accounts for the public good aspect of the action, representing the broader societal benefit from the behavior, and μ reflects the individuals place on social perceptions, specifically how others perceive their intrinsic motivations, $E(x \mid a)_s$. Society uses a rule s to form expectations about an individual's motivations based on their observed actions. In a rational expectations equilibrium, society's expectations are accurate, so the

term $\mu E(v_a \mid a)$ captures the correct perception of the actor's intrinsic motivations.

The principal–in this case, the judge–maximizes utility by selecting the contract and y, represented by the following function:

[10] Their 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.

$$W(y) = f\left(\overline{U}(y) + (1+\lambda)\, ya(y) + \sigma_j \overline{a}\right) \quad (1)$$

In this setup, the judge determines the costs, where $\sigma_j\ \overline{a}$ reflects the systematic component of judge j's decision-making, which influences how much they value the public good $\overline{a}$ compared to other judges. The parameter λ represents the shadow cost of resources used for incentives, such as the costs associated with enforcement. Because judges are randomly assigned with varying values of σ_j, we have exogenous variation in y in our empirical application. This allows us to focus primarily on the behavior of the agent in response to these variations.

In the simple case of two possible actions ($a = 0, 1$), the actor's utility depends on which action they choose:

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

In this framework, choosing $a = 0$ represents exercising free speech rights, which $a = 1$ represents abstaining from free speech. The term $e > 0$ captures judicial concerns that exercising free speech may cause some harm, which influences the overall utility for the actor.

With two possible actions, the social perception of the actor's intrinsic motivations follows a cutoff rule. To simplify the analysis, we normalize $c = C(1) - C(0) - y$, which represents the extrinsic cost difference between the two actions. Using ordinal utilities, we can rewrite the net utilities as follows:

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

This structure establishes a cutoff rule: if an individual chooses $a = 1$ for a certain intrinsic motivation level v_a, they will also choose $a = 1$ for any higher intrinsic motivation $v > v_a$, assuming that others' actions remain fixed in equilibrium. This is because social and extrinsic motivations are constant, while intrinsic motivation increases. Therefore, the cutoff rule is satisfied when:

$$v^* - c + \mu E(v_a \mid 1) = \mu E(v_a \mid 0) \quad (2)$$

This expression provides a sufficient condition for a fixed point, where the cutoff value v^*
solves the equation:

$$\mathrm{v}^* + \mu\Delta(v^*) = c \quad (3)$$

Here, we define:

$$\Delta(v) = E(v_a \mid v_a > v) - E(v_a \mid v_a < v) \quad (4)$$

At the cutoff value v, individuals choose action 1 if their intrinsic motivation v_a exceeds v, and they will choose $a = 0$ if their intrinsic motivation v_a is less than v. Therefore $\Delta(v)$ represents the difference in the expected intrinsic motivation for those who choose action 1 and those who choose action 0:

$$\Delta(v) = E(v_a \mid 1) - E(v_a \mid 0) \quad (5)$$

A sufficient condition for a fixed point is if $1 + \mu\Delta'(v) > 0$, meaning that the share of the population exercising free speech corresponds to the interval $[\underline{v}, v^*]$.

To understand this sufficient condition, note that $v^* + \mu\Delta(v^*)$ represents the marginal benefit of exercising free speech for individuals at the cutoff. The marginal benefit is the sum of their intrinsic motivation and social motivation, while c is the marginal cost. The intuition behind the sufficient condition is as follows: if $1 + \mu\Delta'(v) > 0$, then as the cutoff v^* increases, the marginal benefit will eventually equal the constant marginal cost c, establishing a fixed point for the cutoff. As more individuals exercise free speech, the social honor associated with abstaining increases, which in turn discourages others from exercising free speech. While $1 + \mu\Delta'(v) > 0$ is a sufficient condition for a fixed point, it is not a necessary one. In some cases, $\Delta'(v) < 0$ can occur, leading to rapid shifts in social behavior, as small changes result in a transition between steady states in society.

Appendix Fig. 1 provides the distribution of intrinsic motivations, and under Jewitt's lemma, the shape of Δ reflects the density of v. Initially, Δ decreases and then increases. Intuitively, this occurs because adding a small mass around the cutoff shifts one truncated mean more than the other. When v^* is small (most people choose $a = 1$), increasing v^* raises.

$E(v_a \mid 0)$ more than $E(v_a \mid 1)$, since $E(v_a \mid 0)$ contains few points from the left tail of the

v-distribution. Slightly extending the truncated distribution to the right adds a substantial number of high v individuals. In contrast, $E(v_a \mid 1)$ is less impacted.

In simple terms, the more people who exercise free speech, the more normalized it becomes,

leading others to follow suit, which implies $\Delta'(v) < 0$. This dynamic can lead to multiple equilibria if the complementarity between actions is strong or μ is large enough. When $1 + \mu\Delta'(v)$ becomes negative, unstable equilibria can emerge.

Explicit sanctions signal that the judge perceives a problem, as they have information about v^* through the *Miller* community standard test. This test incentivizes litigants to present information about v^* to the judge in an adversarial system. The judge imposes sanctions when they believe v^* is too high. Observing these decisions prompts community

leaders and individuals to update their beliefs about the distribution of intrinsic motivations. When free speech is commonly exercised, v^* lies on the right side of the distribution, leading to expressive effects in free speech rulings. The model implies two key hypotheses that we test in the data: (1) laws have expressive effects when v^* is high (where the density of v is

decreasing), and (2) laws lead to backlash when v^* is low (the density of v is increasing).[11] We map $\Delta(v)$ to the General Social Survey (GSS), where respondents answer questions

about the perceived morality of specific actions. By reporting their perception of the morality of an action, individuals indicate the difference in social perception between those who choose $a = 1$ versus those who choose $a = 0$, which influences their behavior.

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 and Jonathan (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.[12] 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 2960 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.[13] The administrative data facilitates additional randomization checks. Our data on judge biographical characteristics come from the Appeals Court Attribute Data, District Court Attribute Data,[14] 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).[15] Judges whose religions remained missing or unknown were coded as hav-

ing 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. Table 1 displays summary statistics. Roughly two-thirds of these are conservative decisions. The share of progressive decisions declines after 1973.[16] A dramatic spike is observed, which Songer and Susan (1992) attribute to the causal impact of a 1973 Supreme Court decision.[17]

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).[18] For attitudes, we constructed a binary indicator for the response "not wrong at all".[19] 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.[20] 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".

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

[11] It is worth noting although some laws address fringe offenses, many prohibitions target activities that a substantial share of the public performs regularly, such as speeding, jaywalking, texting while driving, etc.

[12] *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).

[13] Sixteen 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.

[14] http://www.cas.sc.edu/poli/juri/attributes.html.

[15] Additional religion data are available at http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm.

[16] Appendix Fig. 4 plots the quantity of free speech cases that were decided progressively or conservatively over time.

[17] Our results are robust to removing this spike.

[18] The 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 1500 respondents per survey year for 1973–1992 and roughly 2900 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.

[19] The other three response choices are "always wrong", "almost always wrong", "wrong only sometimes".

[20] Appendix Fig. 2 is a map of the 12 Circuits.

Table 1
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

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.[21] County population is used as weights.

Finally, we collected data on diseases from the Centers for Disease Control and Prevention[22] for 1984 to 2008 and extend it back to 1960 using Klick and Thomas (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.[23]

3. Specification

We use regressions of the form:

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

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.[24]

The instrumental variable is constructed by exploiting the random assignment of judges. This method assumes that the random assignment mechanism ensures that judges with different political affiliations (e.g., Democrat, Republican) are distributed independently of the specific case characteristics. Thus, the judge's party affiliation serves as a valid instrument to predict the likelihood of a progressive or conservative ruling on obscenity cases. 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 characteristics. Let N_{ct} be a biographical characteristic, e.g., the number of Democrats assigned to free speech panels. Let $p_{ct} = \frac{N_{ct}}{\mathrm{M}_{ct}} * \mathbf{1}\ [M_{ct-n} > 0]$, i.e., defined to be 0 when $\mathbf{1}\ [M_{\mathrm{ct}-n} > 0] = 0$. Since Circuit Courts handle thousands of cases per year (a small fraction of which are free speech cases) and the

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

[22] U.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.

[23] http://www.census.gov/popest/states/.

[24] Barrios 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.

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: $\mathbf{E}[\frac{N_{ct}}{M_{ct}}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), 1\ [M_{ct} > 0]] =$

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.

Our first stage is thus.

$$(1)\ Law_{ct} = \theta_c + \theta_t + \gamma p_{ct} + \varepsilon_{ct}$$

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.[25] 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.[26] Moreover, newspaper headlines of Circuit Court opinions typically refer to the court and not the identity of the judges on the panel.[27] Violations of the exclusion restriction are also likely to be minimal.

To address the possibility that $1\ [M_{ct-n} > 0]$ responds to previous years' legal decisions, we

instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. Appendix D presents additional details. The demographic characteristics of District judges predict with whether the judge is reversed by Circuit Courts (Haire et al., 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.[28]

4. The effect of judge identity on court outcomes

Table II shows that Republicans were less likely to vote for a progressive verdict.[29] 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

$M_{c(t-n)} + 1$

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

[25] We 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.

[26] An 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.

[27] Badawi 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.

[28] The results of a 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$_{\text{direct}}$) + (TOT_{indirect} of individuals) *P(exposure$_{\text{indirect}}$). 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.

[29] Table 2 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; percentage 15–19; percentage 20–24; percent nonwhite; police employment; unemployment rate; and real per capita income.

Table 2
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	Outcome: % Progressive Free Speech Decisions					
(Merged with Individual-Level GSS Data)	(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	Outcome: % Progressive Free Speech Decisions					
(Merged with State-Level CDC/UCR Data)	(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 *5%; **1%. Additional table notes in text.

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 3 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.[30] 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 Fig. 5A and B 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 Fig. 5A and for each of the 12 Circuits in Fig. 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 Fig. 7A shows the first

[30] These specifications are analogous to the ones in Table 2, 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 3
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*	0.326*	0.362**	0.361**
	(0.125)	(0.129)	(0.110)	(0.108)
Democratic Appointees per Seat$_{t-1}$	−0.129	−0.137		
	(0.0977)	(0.100)		
Democratic Appointees per Seat$_{t-2}$		−0.0526		
		(0.0886)		
Democratic Appointees per Seat$_{t+1}$			−0.0917	−0.0753
			(0.0865)	(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 *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.

stage relationship, that is, the relationship between the assigned N_{ct}/M_{ct} and the proportion of progressive decisions. Appendix Fig. 7B shows the placebo relationship between $\mathbf{E}(N_{ct}/M_{ct})$ and the proportion of progressive decisions.[31]

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.[32] We find that non-religious judges, Democrats, and attendance at elite schools were important 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.

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 permissible, 1.4 percentage points more likely to say premarital sex is permissible, and 0.3 percentage points more likely to say homosexual sex is permissible (see Table 5).

The lags are jointly significant. These are small relative to the mean dependent variable displayed in Column 6. To put this estimate in perspective, Aksoy et al. (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

[31] Appendix Fig. 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 Fig. 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.

[32] The 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.

Table 4
The effect of free speech jurisprudence on attitudes.

Average Lag effect	OLS	Appellate IV	Appellate and District IV	LASSO IV	Obs	Mean Dependent Variable
	(1)	(2)	(3)	(4)	(5)	(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.

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 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. Countries that have not passed any legislation have a frequency of justification of 8.36, which increases to 8.64 with recognition of civil unions, and again to 8.92 with same-sex marriage.

Table V 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, by 3.9 percentage points, to be

Table 5
The effect of free speech jurisprudence on behavior.

Average Lag effect	OLS	Appellate IV	Appellate and District IV	LASSO IV	Obs	Mean Dependent Variable
	(1)	(2)	(3)	(4)	(5)	(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.

Table 6
The effect of free speech jurisprudence on crime.

Average Lag effect	OLS	Appellate IV	Appellate and District IV	LASSO IV	Obs	Mean Dependent Variable
	(1)	(2)	(3)	(4)	(5)	(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.

divorced or separated. 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

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. Fig. 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.

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).[33] Bhuller et al. (2013) and Barrios et al.

[33] Most 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)).

Table 7
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	−81.141	−94.558 *	−75.751	−65.686	
	(64.462)	(45.029)	(38.112)	(44.801)	(54.096)	
No FE	−82.056	−78.434	−75.302	−46.958	−33.439	
	(60.700)	(62.034)	(48.448)	(36.288)	(27.757)	
State Cluster	−56.888	−51.841	−69.982	−55.258	−33.322	
	(36.520)	(38.504)	(37.600)	(37.435)	(41.573)	
No Ind Control	−101.894	−80.435	−117.014	−90.922	−65.367	
	(121.993)	(83.931)	(117.420)	(123.947)	(122.816)	
No Weights	−13.422	−16.093	−36.758 **	−38.544	−15.718	
	(13.066)	(12.059)	(6.881)	(10.626)	(11.695)	
No Community Standards	−58.394	−51.890 **	−70.319 **	−55.459	−33.165	
	(32.994)	(15.079)	(7.617)	(10.225)	(18.893)	
No Controls except $1[M_{ct}>0]$	−226.714	−191.154	−201.168	−109.214	−97.769	
	(259.576)	(243.387)	(224.136)	(155.064)	(126.684)	
Drop Circuit 1	−79.711	−63.593	−83.160 **	−64.068	−39.174	
	(56.486)	(32.739)	(17.712)	(20.529)	(21.009)	
Drop Circuit 2	−59.057	−53.648 **	−69.657 **	−57.449	−30.632	
	(32.773)	(15.847)	(8.054)	(15.537)	(18.628)	
Drop Circuit 3	−51.053 *	−42.069 **	−68.778 **	−48.348 *	−51.910 **	
	(23.966)	(9.930)	(5.019)	(7.475)	(10.390)	
Drop Circuit 4	−53.679	−50.913 **	−68.941 **	−52.930	−39.347 *	
	(35.170)	(18.408)	(7.055)	(10.221)	(16.099)	
Drop Circuit 5	−62.407	−52.638 **	−66.414 **	−56.349	−25.557	
	(38.628)	(18.477)	(8.788)	(16.076)	(20.075)	
Drop Circuit 6	−4.340	−3.666	−31.343	−46.655	−24.286	
	(18.612)	(15.229)	(24.071)	(33.380)	(36.556)	
Drop Circuit 7	−60.410	−60.801 *	−77.127 **	−58.833	−37.586	
	(44.221)	(24.821)	(10.951)	(20.536)	(36.401)	
Drop Circuit 8	−8.701	−6.972	−16.677	−21.846	7.046	
	(35.268)	(20.811)	(17.162)	(13.570)	(15.235)	
Drop Circuit 9	−87.683	−102.192	−96.512 **	−75.410	−48.865	
	(64.317)	(115.462)	(16.615)	(68.031)	(56.414)	
Drop Circuit 10	−56.827	−52.147 **	−70.156 **	−56.426	−35.038 *	
	(35.172)	(17.691)	(7.426)	(12.664)	(17.195)	
Drop Circuit 11	−49.149	−52.186 **	−70.039 **	−50.317	−31.980	
	(26.377)	(15.151)	(8.674)	(9.769)	(17.630)	
Drop Circuit 12	−56.888	−51.841 **	−69.982 **	−55.258	−33.322	
	(32.379)	(15.681)	(6.784)	(10.742)	(18.044)	
1 current 1 lag	3.662	−21.926				
	(9.083)	(13.151)				
1 current 2 lag	−3.711	−28.316 **	−32.645			
	(13.626)	(10.936)	(17.248)			
2 leads 4 lags	−56.447	−63.901 *	−84.808	−69.766	−52.605	
	(43.201)	(27.651)	(58.359)	(44.716)	(72.366)	
1 lead 5 lags	−51.692	−53.219 **	−70.399 **	−53.089	−27.914	−18.82
	(30.496)	(14.185)	(4.493)	(12.023)	(18.456)	(22.167)
4 leads 1 lag	20.923	−6.330	−13.216	−24.437	30.848	3.625
(t0, t1, f4, f3, f2, f1)	(20.030)	(21.678)	(25.401)	(53.931)	(27.848)	(32.504)

Notes: Significant at *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.

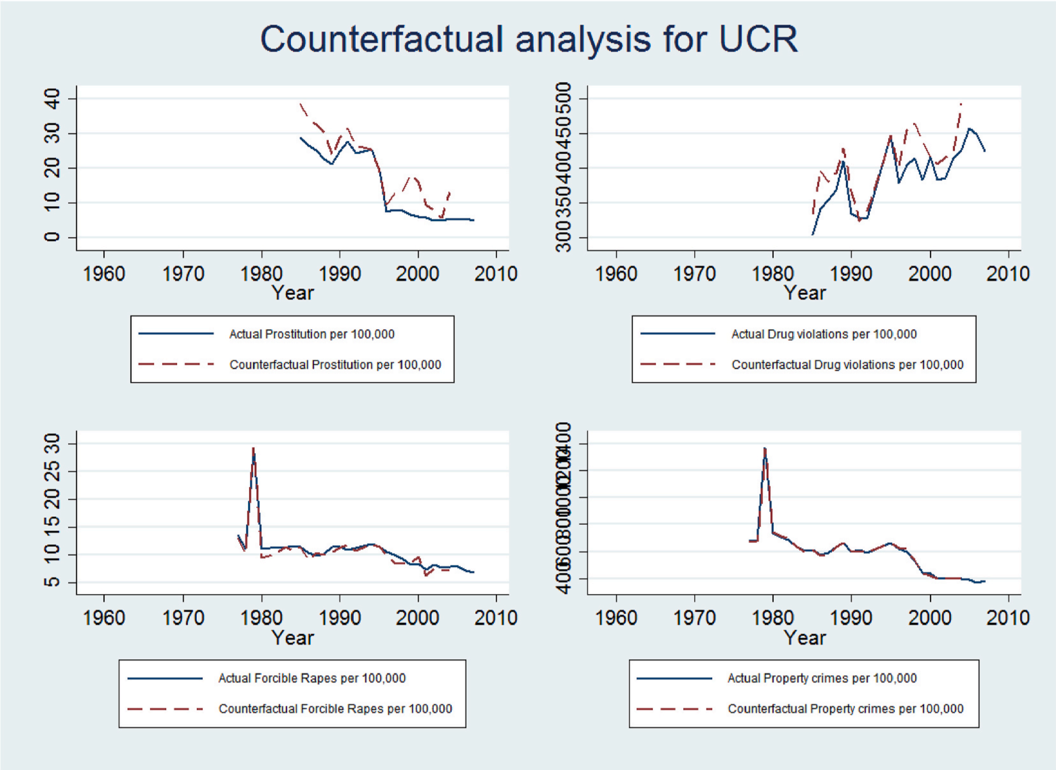

Fig. 1. What if these legal precedents did not exist.?.

Table 8
The effect of free speech jurisprudence on disease.

Average Lag effect	OLS	Appellate IV	Appellate and District IV	LASSO IV	Obs	Mean Dependent Variable
	(1)	(2)	(3)	(4)	(5)	(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.

(2012) report a link to sex crimes.[34] 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 chlamydia by 50 (incidents per state per year), but did not significantly increase gonorrhea or syphilis. Chlamydia, known as the

[34] Barrios et al. (2012) 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.

"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).[35] 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).[36]

5.4. Deterrence

Throughout the 1970s, pornography media providers like *Playboy* and *Penthouse* were frequently involved in free speech litigation, often pushing the boundaries of community standards. However, our analysis found little to no significant impact of free speech decisions on the circulation of these magazines. It is important to note that we

are assessing the effects of obscenity law rather than the consumption of pornography itself. While Bhuller et al. (2013) found that the expansion of internet broadband led to an increase in child abuse, our findings suggest the opposite effect in the context of free speech rulings. This difference indicates that deterrence may not be the only factor driving these outcomes. If deterrence were the primary mechanism, we would expect progressive legal precedents that make obscene content more accessible to increase child abuse, as suggested by Bhuller et al. (2013). Instead, our results suggest that progressive precedents might cause a moral shift toward behaviors like paid or extramarital sex, which could act as a substitute for child abuse (Ciacci and Sviatschi, 2019).

6. Newsreports

Previous studies provide important context for understanding how court rulings influence public preferences. For example, Franklin and Kosaki (1989) found that Supreme Court precedents were linked to shifts in public opinion on abortion. Hoekstra (2000) suggests that local media coverage plays a key role, with residents being more aware of cases occurring in their jurisdiction, which could amplify the impact of local court rulings. The salience of obscenity law was particularly pronounced during the 1960s, as evidenced by the numerous law review articles written in response to obscenity decisions at the time (Kalven, 1960; Magrath, 1966). This ongoing relevance is reflected in our newspaper data search, which shows continued media attention to obscenity rulings from 1979 onward. Appendix Fig. 3 illustrates the correlation between the number of obscenity decisions and related newspaper articles from 1979 to 2008. To account for variations in newspaper availability, we adjust the number of articles by the proportion of newspapers available each year. The correlation remains statistically significant, even after accounting for Circuit and year fixed effects. Although the newspaper database does not extend before 1979, the emotional salience of these rulings suggests that earlier cases were also likely reported.

In a mechanism experiment reported by Chen and Yeh (2023), data entry workers were randomly exposed to news reports of obscenity decisions and then surveyed on their attitudes and behaviors using the same questions as in the General Social Survey (GSS). Across three

replications with 1345 participants, the study observed marginally significant effects on the acceptability of homosexual sex, echoing findings from population-based analyses. However, self-reported behaviors did not change in response to progressive free speech precedents, likely due to the short timeframe of the study, which limited the possibility of actual behavioral changes. This suggests that changes in self-reporting norms are unlikely to explain the results observed in the population-based analyses. Interestingly, exposure to conservative obscenity precedents increased the perceived prevalence of extramarital sex by 2.5 percentage points, supporting the law and norms theory: when legal authorities impose stricter sanctions, people infer that the activity is more widespread. Chen and Yeh (2014) also found that liberal obscenity rulings liberalized both individual and perceived community standards, increasing utility for most participants. However, religious workers became more conservative in their values, identified more strongly as Republican, viewed community standards as more liberal, and reported lower utility. This shows that individuals update their beliefs about the prevalence of sexual activities differently depending on whether they are exposed to liberal or conservative rulings.

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

[35] In 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 10–90 days and rashes within 1–6 months of the primary infection.

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

Table 9
The effects of free speech precedents over time.

Average Lag effect	1973–1993		1980–2000	
	OLS	Appellate IV	OLS	Appellate IV
	(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 *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.

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.[37]

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.[38] We confirm that first stage F-statistics remain high for the two time periods.[39] 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.

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 judicial rulings 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. Chlamydia, often referred to as the “silent” disease due to its lack of immediate symptoms, showed a significant increase, while other STDs like gonorrhea and syphilis did not display similar trends. This suggests that while progressive rulings liberalized sexual behaviors, leading to greater sexual activity, these rulings particularly affected the spread of diseases that are asymptomatic and therefore less likely to be detected early. The findings indicate that changes in legal norms and societal behaviors, especially in sexual activity, had tangible public health consequences, highlighting the broader societal impact of legal decisions. 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).

Our findings also reveal that a legal intervention can steer social norms in two fundamentally different directions depending on a behavior’s initial prevalence. When extramarital sex is already common, labeling it “obscene” or “immoral” strengthens the existing

[37] https://www.cdc.gov/nchs/pressroom/sosmap/unmarried/unmarried.htm.
[38] The results are robust to variation in these cutoffs.
[39] 0.108 (0.039) and 0.141 (0.046), respectively.

stigma, rewarding those who refrain and deterring those who do not—a clear expressive impact.

Conversely, if the behavior is scarce or newly emerging, the same prohibition may reduce perceived taboo, causing a backlash that counteracts the law's original intent. Recognizing this duality underscores why seemingly straightforward regulations can yield unexpectedly divergent moral outcomes.

Our study bears some caveats worth mentioning. While the use of random judge assignment is a robust method for causal inference within the scope of U.S. obscenity law, the generalizability of these findings to other legal contexts or jurisdictions remains an open topic for research. The specific cultural, political, and legal environment of the United States might limit the broader applicability of the results. Moreover, the study measures outcomes such as sexual attitudes, behaviors, and health impacts primarily through secondary data sources like the General Social Survey and crime statistics. These measures, while useful, might not fully capture the nuanced changes in societal norms and values. Additionally, self-reported data on sensitive topics like sexual behavior can be prone to bias and inaccuracies. Finally, some of our analyses are constrained due to data availability.

That said, 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 policymakers 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.

CRediT authorship contribution statement

Daniel L. Chen: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Susan Yeh:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ssresearch.2025.103155.

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