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

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Abstract Does law shape values? We test a model of law and norms that predicts when law has backlash or expressive effects. For over fifty years, Democratic judges were 10% more likely to favor progressive free speech standards than Republicans. Using the random assignment of U.S. federal judges setting geographically-local precedent, we estimate that progressive free speech standards, on average, liberalized sexual attitudes and behaviors and had secondary effects on sex-related crimes and diseases. We then conduct experiments by assigning workers to transcribe news reports about free speech decisions. Exposure to progressive decisions led to liberalized sexual attitudes but not self-reported behavior. Exposure to conservative decisions led to beliefs that extramarital sex was more prevalent. The shift in norm perception verifies a key assumption in the model and explains a reversal of free speech law's effects from backlash to expressive before and after the sexual revolution.

Keywords: Law and Norms, Sexual Revolution, Pluralistic Ignorance, Experiment

JEL codes: J12, J16, Z1, N32, N42, K42

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"There ought to exist the fullest liberty of professing and discussing, as a matter of ethical conviction, any doctrine, however immoral it may be considered. ... If all mankind minus one were of one opinion, and only one person were of the contrary opinion, mankind would be no more justified in silencing that one person than he, if he had the power, would be justified in silencing mankind. ... The only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent harm to others" (Mill 1859).

1. INTRODUCTION

How do moral revolutions occur? The origin of rights has long interested legal and humanistic scholars (Tushnet 2009; Appiah 2011). From the abolition of slavery, to women's rights, to environmentalism, law is speculated to play a key role in shaping values (Bénabou and Tirole 2012; Acemoglu and Jackson 2014), yet little causal evidence exists to date. While law and economics has traditionally focused on deterrence to explain behavioral responses to the law (Becker 1968), a large body of work in psychology suggests that people respond to the law simply because it tells them what is the right thing to do (Tyler 2006). We investigate whether law shapes values in an area of law–in particular, free speech laws related to obscenity–that is likely to have effects other than through economic sanctions alone because of its emotional salience and ability to provoke controversy. Previous papers on law and norms use experimental economics with exogenous variation in the rules of the games to mimic the law (Dal Bó et al. 2010; Galbiati and Vertova 2008; Bowles and Polania-Reyes 2012; Croson 2009). This paper presents causal identification in the field.

Throughout history, much debate has arisen over free speech regulation of obscenity; as social norms change and technology facilitates broader dissemination of media, obscene content continues to push previously-held boundaries. In response to worries about the impact of obscene media, governments in developed and developing countries have enacted a variety of regulations while courts have wrestled with the interpretation and legality of these regulations. In India, kissing in public has led to charges of obscenity[1] and the government has authorized the prosecution of Facebook, Yahoo!, and Google over obscene material. In Norway, the introduction of internet broadband increased sex-related crimes, which has been attributed to the increased consumption of obscene content (Bhuller et al. 2013). 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 AM. JUR.2d §§ I, 2 (1995)) are among the harms that have been commonly cited in the U.S. to justify the exercise of police powers in restricting expressions of obscenity. However, little is known regarding what free speech regulations actually do and whether the rationales put forth by policy-makers and by scholars of female empowerment concerned about the commodification of women and its deleterious secondary effects (Dworkin and MacKinnon 1988; Radin 1996)[2] are empirically justified.[3]

[1] BBC 2009

[2] 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. The link between commodification of women and gender violence can be formalized in a model of incomplete contracts (Chen 2004). Though stressing that morality is not the focus (MacKinnon 1987), MacKinnon and Dworkin's (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.

[3] Some studies try to look directly at the link between pornography consumption and sex-related crimes (Diamond 2009; Winick and Evans 1996), but identification of causal effects is challenging.

Since 1973, the legal standard defining obscenity in the U.S. has been the Supreme Court's three-part *Miller* test (*Miller v. California*, 413 U.S. 15 (1973)), which 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." Because community standards define obscenity according to the *Miller* test (and the *Roth* test before it[4]), a positive feedback may arise. If free speech precedent gives people more room for sexually progressive expression and greater social acceptance of alternative behaviors, then more progressive community standards would make it easier to subsequently challenge restrictive free speech obscenity regulations, leading to multiple steady states through which abrupt shifts in normative commitments can occur (Akerlof et al. 1996; Cooter et al. 2008).

Bailey (2010) documents that following progressive Supreme Court obscenity precedent, state statutes quickly liberalized obscenity regulations. Several studies have linked major court rulings with subsequent changes in public opinion in the locality where the case originates and suggested that the media (Hoekstra 2000)[5] and community leaders (Weinrib 2012) play a prominent role; booksellers and distributors were aware of how free speech decisions defined obscenity and were careful to self-censor before the materials reached the public (Barth 1968). Previous field studies of expressive law (Funk 2007), expressive externalities (spillover effects) of law (Fox and Griffin 2009)[6], and of free speech regulations in particular (Paul et al. 2001), have only been cross-sectional or time-series and have lacked a clear control group. Part of the econometric challenge is that courts may make progressive decisions if social mores are progressive, creating upward bias in OLS estimates. On the other hand, if harms from secondary effects are perceived to be high, courts may be more likely to rule conservatively, creating downward bias in OLS estimates.

To address these econometric challenges, we use three aspects of the U.S. common law tradition: the random assignment of judges, judges interpreting the facts and the law differently and in manners correlated with their demographic characteristics, and a system of Circuit Courts with regional jurisdiction setting legal precedent for millions of people. Between 1958 and 2008, 175 U.S. Circuit cases addressed free speech regulations of obscenity (Sunstein et al., 2006; Kastellec, 2011). Cases that reach the Circuit Courts are the more challenging and controversial cases with the greatest likelihood to set new precedent.[7] Because the composition of judicial panels on free speech cases is unlikely to be correlated with subsequent social outcomes other than through the decisions themselves, the random assignment of judges–in particular, whether they are Republicans

[4]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.

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

[6]Laws that affirm moral standards or sexual norms can lead to broader consequences. See, for example, Dennis's (2007) historical discussion of the cultural and literary consequences following the enactment of the Comstock Act of 1873 and Stroebel et al.'s (2012) study that a community leader's interpretation of law led to subsequent increase in contraceptive use (Stroebel and van Benthem 2012). Contraceptive use has been linked to the sexual revolution (Akerlof et al. 1996) through peer effects (Card and Giuliano 2011); it has also been linked to STDs (Klick and Stratmann 2003).

[7]Only 2% of Circuit cases successfully appeal to the U.S. Supreme Court, so Circuit Courts determine the vast majority of decisions each year that set legal precedent.

or Democrats[8]–creates exogenous variation in the progressivity of free speech precedent, which we can use to estimate the causal impact on moral values and behavior.

On a theoretical level, whether the law can affect moral values and behavior simply through its expressive power is widely presumed (Cooter 1988; McAdams 2000; Posner 1998a, 2000; Kahan 1997; Sunstein 1996), but the precise conditions under which expressive or backlash effects occur have not been modeled (i.e., when law causes what is viewed as moral to shift towards or against what the law intended). We describe a model of law that affects the morality of particular actions (Bénabou and Tirole 2012; Kaplow and Shavell 2007) in Section 2. The model examines the implications of three motivations for human behavior: intrinsic motivations (i.e., values), extrinsic motivations (i.e., material incentives), and social motivations (i.e., norms). Social motivations arise from individuals receiving honor or stigma for doing something that is outside the norm. People would like to signal their type (i.e., values) and appear moral to gain honor or avoid stigma. Legal decisions inform people about the social norms (i.e., information multiplier). Prohibitions cause people to think that the government sees a problem and that obscene activity is more prevalent, a key assumption. It is then easier for those who are motivated by intrinsic incentives to signal their honor to others. We call this an "expressive effect," since law causes what is viewed as moral to shift towards what the law values. This expressive effect, however, only arises when a sufficient number of people perform the stigmatized activity. When only a few people conduct the stigmatized activity, the social perception of stigmatized activities can increase substantially if the shift in beliefs cause stigmatized activities to become normalized (i.e., social multiplier). When the normalizing effect exceeds the signaling effect, we call this a "backlash effect,"[9] since the law causes what is viewed as moral to shift against what the law values.

We test this model using random variation in jurisprudence in Section 3. We first verify that judges are effectively randomly assigned and we confirm the divisiveness of social issues along political lines; Democratic judges were more likely than Republican judges to vote progressively in free speech obscenity cases (Sunstein et al., 2006; Songer and Haire, 1992). To buttress existing evidence on information transmission of Circuit Court decisions, we document that newspaper articles about Circuit obscenity decisions increase in the Circuits and years with Circuit decisions. Thus, if subsequent judges follow precedent, and progressive free speech decisions on the margin increase the likelihood that people can successfully challenge restrictive obscenity regulations, then we might plausibly expect individuals to respond to Circuit decisions, whether through law enforcement (Barth 1968) or community organizations raising awareness (Weinrib 2012) or both.

We next estimate the subsequent impact of free speech precedent on sexual attitudes, behaviors, crime, and disease in Section 4. Progressive free speech jurisprudence on average led to more progressive attitudes on premarital, extramarital, and homosexual sex; and more progressive self-reported sexual behavior: more sexual partners, non-marital sex, and paid sex, especially by men. Individuals older than 40 were more likely to report being divorced or separated while those under 40 were less likely to report being divorced or separated.

In terms of secondary effects, progressive free speech jurisprudence increased prostitution (i.e.,

[8]As shorthand, we will refer to judges appointed by Democratic presidents as "Democrats."

[9]The term "backlash" is widely used across many fields. A seminal social psychology study documented backlash against women who promoted themselves (Rudman 1998); this is not how we use the term here.

community vices), rape, and drug violations, as measured by arrest data. Arrests for child abuse (i.e., offenses against family and children) declined. Progressive free speech decisions also led to an increase in the incidence of invisible sexually transmitted diseases (STDs), such as chlamydia, but no significant effects on visible STDs, such as gonorrhea and syphilis. This difference is consistent with behavioral epidemiological models of disease transmission with sorting among sexual partners based on their disease status (Kremer 1996).

We subject our estimates to a battery of robustness checks and placebo tests, including whether social mores move in advance of free speech precedent and whether non-sexual crimes, such as property crimes, respond to free speech precedent. We vary the set of controls, vary the distributed lag structure, and vary the biographical characteristics used to isolate exogenous shifts in free speech precedent. Our results yield evidence that law shapes norms.

Sexual norms have changed dramatically during the time period of our study. In 1957, 57% of Americans believed that adults who preferred to be single were "immoral"; 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). Sexual norms also vary dramatically across space. 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).

The theoretical model suggests that whether law has expressive or backlash effects depends on the underlying social norms. When previously-stigmatized activities are relatively scarce and conservative free speech obscenity decisions cause people to update their beliefs that the stigmatized activities are more common than previously thought, these activities become normalized, causing more people to do the previously-stigmatized activity. In our data, a large number of free speech decisions occured during the sexual revolution and a large number were decided conservatively, greatly increasing the information multiplier. In the aftermath of the sexual revolution, progressive free speech decisions weaken the ability for individuals with high intrinsic motivations to signal their value by abstaining from normalized activities. Progressive free speech decisions then have expressive effects. This prediction is verified experimentally in Chen and Yeh (2014a), which finds that religious individuals backlashed to progressive free speech precedent and report lower subjective well-being whereas non-religious individuals experience expressive effects and increased subjective-well being.

This paper complements the experimental evidence with historical data. In the early years of our data, conservative decisions led to backlash: an increase in the perceived morality of homosexual sex and the incidence of non-marital sexual behaviors. The opposite occured in the aftermath of the sexual revolution. Self-reports of paid sex and police-reports of arrests for prostitution notably move in tandem, which suggests that the effects found in arrest data may not be completely due to changes in law enforcement, and that arrest data may reflect actual changes in underlying behavior.

Through experiments reported in Section 5, we explore whether self-reported behavior reflects actual changes in underlying behavior and verify that exposure to free speech precedent could affect attitudes. Data entry workers were assigned to transcribe newspaper summaries of free speech obscenity decisions that we randomized to be progressive or conservative. We then asked the same

set of attitude and behavior questions as in the population-based analysis. Among 197 workers from around the world, those transcribing newspaper summaries of progressive free speech decisions were more likely to say homosexual sex was moral, but were no more likely to report liberalized sexual behaviors. This difference suggests that self-reported behavioral shifts in response to free speech decisions are not simply due to people's openness to discussing sexual behaviors.

A second experiment restricted workers to being from the U.S. and surveyed attitudes as well as beliefs about sexual norms. Among 548 workers, those transcribing newsreports of progressive free speech decisions were, again, more likely to say that homosexual sex was moral and were also more likely to favor sex education in public schools. Verifying a key assumption (the information multiplier) in the theoretical model, workers reported believing a higher percentage of people have extramarital sex after being exposed to conservative free speech decisions. This shift in norm perception explains the reversal of free speech decisions' effects from backlash to expressive during and after the sexual revolution.

Of the 175 free speech obscenity cases in our database, 45% mention "gay" or "lesbian;" including the historical designation, "pervert," increases the proportion of cases related to homosexuality to 65%. As such, our findings may shed light on contemporary debates over same-sex marriage and discrimination, though we emphasize the decisions are about obscenity (as defined in its historical context) and not gay rights per se. We also emphasize that our experimental analyses do not imply that the only channel through which free speech precedent impacts the public is news reports (Hoekstra 2000; Kritzer and Drechsel 2011)[10], but the short time frame of our experiments reveal that information alone can shift attitudes; it also enables us to differentiate expressive from deterrent effects, as the role of material penalties is likely to be small. We invite future studies to further investigate the consequences of free speech decisions and, more generally, to apply our framework to understand social movements and the origins of rights.

2. BACKGROUND

We begin by defining what is being modeled. We define expressive effects as occurring when the law causes preferences to shift towards what the law values, and backlash effects as occurring when the law causes preferences to shift away from what the law values. The theoretical framework is intended to assist in understanding when laws have expressive effects as opposed to backlash effects. Scholars in a wide range of areas have made normative arguments for or against certain policies on the basis of their expressive or backlash effects, but a clear framework for assessing the likelihood of their occurrence is needed (Lessig 1998; Ellickson 1998).[11] To the best of our knowledge, one formal model exists (Bénabou and Tirole 2012), a simplified version of which we present below and apply to the context of court-made law.

We build on three assumptions for human motivations: intrinsic motivations, where people perform an action simply because they believe it is the right thing to do; extrinsic motivations, where material incentives and deterrence influence actions; and social motivations, where values, norms,

[10] News reports may, nevertheless, make community leaders aware of cases, and these leaders may pass the decision onwards.

[11] For a sample of backlash claims, see: voter mobilization (Mann 2010), multiculturalism (Mitchell 2004), environmentalism (Wolf 1995), private infrastructure investments (Lopez et al. 2009), health care (Mechanic 2001), abortion (Pridemore and Freilich 2007), Americans with Disabilities Act (Krieger 2000), globalization (Eckes 2000), Warren Court (Feld 2003).

social sanctions provided by society affect actions. We focus on two sets of multipliers: a social multiplier, where people accrue honor or stigma for actions outside the norm (if very few people use drugs, then drug users receive stigma; if very few people donate millions, then generous donors receive honor); and an information multiplier, where information is conveyed by legal decisions on the norms, which is the distribution of actions in the population. An extensive review of the behavioral assumptions is available elsewhere (Bénabou and Tirole 2012; Kaplow and Shavell 2007).[12]

2.1. *Social Multiplier*

People maximize the following utility function:

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

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

The principal – the social planner – maximizes over the contract and y:

$$W(y) = \overline{U}(y) + \ (1+\lambda)\, y a(y)$$

In the model, the courts are the social planner, as they set the optimal payoffs and the costs. For now, ignore the objective function of the planner, but focus on the effect of change of incentives on people's actions.

In the simple example of two actions ($a = 0$, 1 or, for expositional purposes, not abstaining from extramarital sex/abstaining from extramarital sex, respectively):

$$a = 1 : U(1) = v_a + y - C(1) + e\overline{a} + \mu E(x \mid 1)_s$$

$$a = 0 : U(0) = -C(0) + e\overline{a} + \mu E(x \mid 0)_s$$

Because there are two actions, the social perception of the actor's intrinsic motivations now follows a cutoff rule, which will be elaborated upon below. Normalize $c = C(1) - C(0) - y$, which is the cost difference between the two actions net of the extrinsic benefit (i.e., extrinsic motivation); then the net utilities are:

$$a = 1 : U(1) = v_a - c + \mu E(x \mid 1)_s$$

$$a = 0 : U(0) = \mu E(x \mid 0)_s$$

This expression provides a cutoff rule, since if a person chooses to take action $a = 1$ at some v_a,

[12] For psychological interventions changing the social meaning of actions, see, e.g., Cialdini (1984); experimental evidence on the expressive effects of incentives, see, e.g., Tyran and Feld (2006); Kantian-type (or deontological) reasoning motivating behavior, see Brekke et al. (2003); Andreoni (1989); reputational payoffs as the moral sentiments, see Bem (1972); Smith (1761); desire to signal conformity, see Bernheim (1994), desire to signal distinction, see Pesendorfer (1995); and moral emotions in regulating behavior, see Haidt (2001).

then the person would also choose $a = 1$ at any $v > v_a$, holding others' actions fixed in equilibrium. This is because the social motivation and the extrinsic motivation are fixed, while the intrinsic motivation increases. Thus the cutoff rule will satisfy:

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

We can rearrange the cutoff rule to define

$$\Delta\left(v\right) = \ E\left(v_a \mid v_a > v\right) - E\left(v_a \mid v_a < v\right)$$

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

$$\Delta\left(v\right) = E\left(v_a \mid 1\right) - E\left(v_a \mid 0\right)$$

This expression maps onto our use of the General Social Survey (GSS), when people respond to questions about the morality of particular actions. The expression also motivates a sufficient condition for a fixed point.

The fixed point solves the equation:

$$v^* + \mu\Delta\left(v^*\right) = c$$

A sufficient condition for a fixed point is if $1 + \mu\Delta^{'}\left(v\right) > 0$, and $[\underline{v}, v^*]$ share of the population do not abstain from extramarital sex. Note that the action need not be observable by others for the model to apply. $E\left(x \mid a\right)_s$ could also capture one's own perception of intrinsic motivations in a self-signalling framework.

2.1.1. *Strategic Substitutes*

The infra-marginal group is changing when the cutoff rule changes. At the steady state, the incentives of the infra-marginal group must be such so that the marginal benefit of the action is equal to its marginal cost. The expression $v^* + \ \mu\Delta\left(v^*\right)$ captures the marginal benefit for the people at the cutoff. This marginal benefit is the sum of intrinsic motivation and social motivation, due in part to the observability of the action. If the derivative is positive, then the marginal benefit will eventually equal the marginal cost c and there will be a fixed point. The intuition is that $1+\mu\Delta^{'}\left(v\right)$ captures the increase in marginal benefit of increasing intrinsic motivation for the people at the cutoff when the cutoff changes. The increase in marginal benefit increases directly as well as indirectly through changes in social perceptions that result from society recalculating expectations when this group switches. To put it another way, as the cutoff rule goes up, only high intrinsic types reap a positive benefit from performing the action through social perceptions because fewer low intrinsic types try to pretend to be high intrinsic types. In other words, actions are strategic substitutes: the fewer the number of people who do an action, the more likely others will do it because it is easier to signal their intrinsic type.

2.1.2. *Strategic Complements*

While $1+\mu\Delta^{'}(v)>0$ is a sufficient condition for a fixed point, it is not a necessary condition and $1+\mu\Delta^{'}(v)$ can be negative for some cutoff values. Indeed, the cutoff rule and $\Delta^{'}(v)$ are closely linked. A rise in v^{*}, raises both $E(v_a \mid 1) = E(v_a \mid v_a > v)$ and $E(v_a \mid 0) = E(v_a \mid v_a < v)$. So, the difference: $\Delta(v) = E(v_a \mid 1) - E(v_a \mid 0)$ may either increase or decrease. Δ initially decreases, then increases, so it is U-shaped, though not necessarily uni-modal. To see why, when v^{*} is small (most people are abstaining from extramarital sex), then raising v^{*} will increase $E(v_a \mid 0)$ more than $E(v_a \mid 1)$, as $E(v_a \mid 0)$ will include very few points on the left tail of the v-distribution, and so by slightly increasing the right margin, we include a lot bigger v's, and many more proportionately than what we had before in $E(v_a \mid 0)$ (See Figure 1 and recall that everyone to the right of v^{*} will do the action–abstain from extramarital sex–whereas everyone to the left of v^{*} will not abstain from extramarital sex). But for the $E(v_a \mid 1)$, when we have most of the v distribution, cutting off a bit from the left-hand side will not have that much effect on the mean, as we're cutting off a small fraction proportionately. The social perception (or morality) that one gains for having extramarital sex increases more than does the social perception (or morality) for abstaining from extramarital sex. In other words, actions are strategic complements: the more people do some misdeed, the more other people will do it.

So, for v^{*} small, $E(v_a \mid 1) - E(v_a \mid 0)$ will most likely be decreasing. In this region, there is a social stigma from not abstaining from extramarital sex because it is relatively scarce. By the same argument when v^{*} is close to the $\overline{v}$, then $E(v_a \mid 1) - E(v_a \mid 0)$ will be increasing. This gives a U-shape to Δ. In this region, there is a lot of honor attached to abstaining from extramarital sex when everyone else is not abstaining from extramarital sexual activity. Whether v^{*} is on the left-side or the right-side of the distribution determines whether the social multiplier makes actions strategic substitutes or strategic complements.

Multiple equilibria can arise if complementarity is strong enough or μ is large enough. Indeed, when $1+\mu\Delta^{'}(v)$ is negative, there may be unstable equilibria. Rapid social changes are possible when society moves from one steady state to another.

2.2. *Information Multiplier*

Now suppose individuals misperceive the distribution–pluralistic ignornance in the psychological literature–and v^{*} is on the left side of the distribution of actions, meaning there are few extramarital sexual activities. First, consider the case of excessive optimism. People think v^{*} is even lower (i.e., people think there are even fewer extramarital sexual activities than is actually true). In this case, social stigma is a sufficient motivator. Releasing statistical information about the true distribution backfires, since it reduces the stigma effect. Explicit sanctions, however, indicate that the policymaker sees a problem within the Circuit. The judge gathers information about v^{*} and issues a sanction when she believes v^{*} is too high. For example, the *Miller* community standard test incentivizes litigants to bring information about v^{*} to the judge. Upon hearing the decision, individuals update their beliefs about the underlying distribution. Therefore, explicit sanctions substitute for norm-based stigma. That is, law undermines the intrinsic, social norm-based motivation for abstaining from an action and we obtain a backlash effect. The previously stigmatized activity becomes normalized and its morality increases faster than does the morality of abstaining from extramarital

sex. Now, consider the case of excessive pessimism. People think v^* is not that low (i.e., people think a larger percentage of people have extramarital sexual activities than is actually true). In this case, statistical information strengthens the stigma effect and complements the norm-based stigma. However, explicit sanctions indicate that the policymaker sees a problem and shifts v^* further to the right, which reduces the stigma effect. Thus, when v^* is on the left side of the distribution, conservative free speech obscenity decisions lead to have backlash effects. The backlash effect can even exceed the sanctions effect if judges do not optimally account for these non-deterrence-based effects or miscalculate μ.

When there are many extramarital sexual activities and v^* is on the right side of the distribution, free speech decisions have expressive effects. First, consider the case of excessive optimism: people think v^* is not that high, that there are fewer extramarital activities than is actually true. Statistical information strengthens the honor effect. Explicit sanctions in the form of conservative free speech decisions lead people to update their beliefs that extramarital sex is more prevalent. This complements the norm-based honor effect that comes from individuals with high intrinsic values signalling their type. Consider the case of excessive pessimism when people think v^* is even higher. In this case, people think a larger percentage of people have extramarital sexual activities than is actually true and social honor is a sufficient motivator. Sanctions move beliefs about v^* even higher, which reinforces the honor effect. Progressive free speech decisions lower beliefs about v^* and reduces the norm-based motivation for an action. When v^* is on the right side of the distribution, free speech law has expressive effects.[13]

3. DESIGN OF STUDY

3.1. *Identification Strategy*

Free speech obscenity law in the U.S. is represented at several levels. Locally, city ordinances disallow the showing of explicit films at theaters; on the federal level, FCC regulations prohibiting television stations from broadcasting obscene content while federal statutes proscribe interstate transport of obscene matter. Laws that regulate obscene expression rely on definitions of what is obscene and can be subject to Constitutional scrutiny. Under First Amendment jurisprudence, obscenity is unprotected speech, meaning that a government is allowed to regulate one's expression if that expression is defined to be obscene.[14] As it happens, there is no umbrella federal statute in the U.S., so whether or not something is obscene depends on legal precedent. In other words, the scope of allowable obscenity regulation depends on federal court precedent.

Our identification strategy exploits both the law-making function of U.S. common law courts and its geographic scope. At the heart of the U.S. legal system is its common law tradition in which judges not only apply the law but also make the law, since a judge's decisions in current cases become precedent for use in decisions in future cases in the same court and in lower courts of the

[13]Government policy can also provide information about externalities. If, for example, conservative free speech decisions indicate that negative externalities from obscenity exposure are greater than previously thought due to the secondary effects of sexual violence and disease, then free speech decisions would always have expressive effects. In our experiments and in our field data, we do not find that exposure to conservative free speech jurisprudence increase beliefs about the negative externalities of obscenity, such as whether sexual materials lead to the breakdown of morals or whether sexual materials lead to rape. In fact, after progressive free speech precedent, people were more likely to believe that sexual materials lead to the breakdown of morals and that sexual materials lead to rape.

[14]The regulation must also satisfy the necessary Constitutional criteria such as not being overbroad or vague.

same jurisdiction.

Jurisdictional boundaries in the United States are geographical (see Figure 2), and the smallest geographical subdivision is the "District." A District Court sits in each locality (boundaries in dotted lines) and serves as the general trial court where a jury is drawn to decide *issues of facts*. A "Circuit" is the larger geographic subdivision (boundaries in solid lines) and comprises a number of Districts from 5 to 13. There are a total of 12 Circuits, which decide *issues of law*; they take facts as given from District Courts and have no juries. They are also known as Courts of Appeals or federal appellate courts, and only hear cases presenting new legal issues (only 10-20% of District Court opinions are appealed).

In deciding issues of law, Circuit Courts provide new interpretations or distinctions of pre-existing precedents or statutes. These new distinctions expand or contract the space under which an actor is found liable (Gennaioli and Shleifer 2007). For example, *Young v. American Mini Theatres, Inc.*, 427 U.S. 50 (U.S. 1976) declared constitutional a city ordinance that prohibited adult movie theaters from being located within 1000 feet of any two other "regulated uses" (10 different kinds of establishments in addition to adult theaters). Later, *Renton v. Playtime Theatres*, 475 U.S. 41 (U.S. 1986) introduced a distinction that provided further restrictions: these kinds of city ordinances applied to theater owners who intended to exhibit adult motion pictures in their theaters, even if there may be some uncertainty about their secondary effects on other persons. See the Appendix for additional examples of the changing legal doctrine on obscenity.

Each Circuit Court decides many thousands of cases per year (less than one case per Circuit per year is related to obscenity) that are *binding precedent* within that Circuit. 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. Circuit Court decisions are *persuasive precedent* on state courts within the Circuit.[15]

Each Circuit Court case receives *three randomly assigned judges* out of a pool of judges, numbering roughly 8 to 40 depending on the size of the Circuit. These judges are appointed for life by the U.S. President and their positions and decisions are highly esteemed.[16] Some judges take a reduced caseload, but all are randomly assigned by a computer algorithm and their names are typically not revealed to the litigating parties until after they file their briefs.

Judges may rely on personal values, heavily influenced by larger historical forces (Klarman 2004). Group identity, experience, or legal philosophy may cause judges to view issues in different ways (Chen and Spamann, 2014; Berdejó and Chen 2013). Our causal inference therefore comes from the random assignment of judges who interpret the facts and the law differently.[17] Democratic judges vote differently from Republican judges on free speech cases pertaining to obscenity (Sunstein et al. 2006; Songer and Haire 1992). We use exogenous variation in establishment of precedent across

[15] Persuasive precedent must be adopted by the state courts to become binding precedent. State courts have a similar heirarchy: district courts, appellate courts, and supreme courts; and an appeal from the state supreme court goes to the U.S. Supreme Court.

[16] Except for retirement, Circuit judges typically leave the bench only for a position in the U.S. Supreme Court.

[17] It has been documented that judges' personal attributes, such as gender, race, religion, and political persuasion (judges are appointed for life by the president, and the party of the president is often a good proxy for a judge's ideology), can predict how judges may vote on a certain type of case (Peresie 2005; Chang and Schoar 2013; Sunstein, Schkade, and Ellman 2004).

different regions to identify the effects of obscenity law on sexual attitudes, behavior, crime, and disease.

Governments may act more aggressively if they feel empowered by new, favorable precedents (Berliner 2003; Nader and Hirsch 2004; Chen and Yeh 2014b). Municipalities could increase or decrease enactments of obscenity regulations or modify existing ordinances in response to court decisions. For example, following a progressive Supreme Court obscenity precedent, state statutes quickly liberalized obscenity regulations (Bailey 2010). The mayor of Jacksonville, Florida, in 1980 also publicly acknowledged that the city rewrote its anti-pornography ordinances to meet court challenges.[18] Changes in a locality's enforcement of existing regulations relating to obscene or licentious conduct may alter public behavior. Community organizations in addition to the ACLU, such as religious organizations or other interest groups (Kobylka 1991), may respond to free speech decisions by making statements directly to audiences or through the media.

An extensive literature review suggests that court decisions only have direct influences on preferences among people in the community where the case originates (Hoekstra 2003). To buttress the plausibility that Circuit decisions could eventually reach individuals in a locality, we use a sample of newspapers and their mentions of Circuit decisions. We collated articles from the major newspaper for the city in which each Circuit Court resides.[19] We find a positive correlation that is statistically significant at the 10% level. Figure 3 displays a plot comparing the number of free speech obscenity decisions and the number of newspaper articles about obscenity decisions from 1979 to 2008).[20] While we lack data for newspaper articles before 1979, the salience of free speech obscenity law was potentially greater at that time. A large number of important law review articles responded to obscenity decisions during the 1960s (Kalven 1960, Magrath 1966, Lockhart 1960).

We also investigate a second channel of transmission: pornography media providers, who were often parties in free speech litigation. We obtain state-level data on sales of the pornographic magazines, *Playboy* and *Penthouse,* from the Audit Bureau of Circulations. These data were collected annually for a single month's issue, 1955-2010 for *Playboy* and 1970-2010 for *Penthouse*. *Playboy* circulated widely in the 1960s and '70s among men and its total circulation peaked in the 1970s. There is weak to no evidence of any impact of free speech decisions on magazine circulation.

Finally, we note that information need not transmit directly from the Circuit Court decision itself. In other work, we document that Circuit precedent is followed by states and District Courts within the Circuit but not outside. Circuit decisions are cited more frequently by state statutes and treatises and District Courts inside the Circuit Court rather than outside. State citations to cases where the state lost are statutory amendments complying with the Circuit Court precedent or, in some cases, distinguishing from the Circuit decision many years later. Reading District cases indicates that District Court cases follow Circuit precedent, which is an assumption buttressed in

[18] Matt Bokor, "Jacksonville Porn-Free, Officials Say," Associated Press, Domestic News, Dec. 16, 1980.

[19] 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".

[20] Not every newspaper is available for every year. In our model, we include Circuit- and year-fixed effects. In the figure, we divide the number of newspaper articles by the proportion of newspapers available. For example, if in 1980, only half of the typical newspaper coverage is available because of data limitations, we divide by 0.5. This allows us to compare graphically the number of Circuit decisions and newspaper articles about obscenity over time.

other quantitative work (Chen et al. 2014a).[21] This suggests that we can feel reasonably confident that states and District Courts are predominantly following the precedent of Circuit Courts that contain them.

3.2. *Data*

3.2.1. *Legal Cases*

Our empirical analysis draws on several sources of data on free speech obscenity cases—established datasets as well as our own data collection. Sunstein et al. (2006) and Kastellec (2013) collect data on all Circuit free speech obscenity decisions published from 1958 to 2004. We extend the data to 2008. The cases were identified by shepardizing (tracking the citations of) the following landmark Supreme Court decisions, as it is reasonable that most obscenity cases would cite one or more of these cases: *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). Sunstein et al. (2006) and Kastellec (2011) then narrow to cases decided on substantive grounds regarding obscenity. Many cases involve challenges to charges of the distribution, production, or possession of obscene materials. Some examples are *United States v. Keller* (mailing postcards containing indecent language),[22] *Eckstein v. Melson* (selling magazines and books with explicit sexual imagery),[23] and *Penthouse v. McAuliffe* (showing a *Penthouse* movie).[24] More recently, cases in the 1990s and 2000s involve downloading images from the Internet,[25] and making lewd phone calls.[26] In addition to Circuit Court obscenity cases, our second dataset comprise all District Court obscenity cases, yielding 2960 cases from 1957 to 2008.[27]

Decisions supporting a finding that the activity was not obscene within the meaning of the law are coded as progressive. A progressive free speech decision could mean that the court held that material itself was not obscene according to obscenity-defining criteria, such as the *Miller* test. A progressive free speech decision could also mean that the court considered that individual interest in free expression outweighed the state's interest in protecting individuals from the effects of obscenity, as the *Ginsberg v. New York* (390 U.S. 629 (1968)) rationale provides. Appendix Table I lists the cases and their coding.

Figure 4 plots the quantity of free speech obscenity cases that were decided progressively or conservatively over time. Table I indicates that, on average, there are 0.30 free speech obscenity panels per Circuit-year for a total of 175 cases. Roughly two-thirds of these are conservative decisions. The ratio of progressive to conservative free speech decisions is lower after 1973, the year of the *Miller* decision, compared to 1958–1972, when *Roth* was the standard. This pattern is consistent

[21] A key challenge to examining whether law has precedent is to address the endogenous selection of cases into courts based on legal standard. Chen et al. (2014a) examines cases filed before the Circuit Court decision but resolved after the Circuit decision. Such a methodology requires relatively high frequency judicial decision-making (e.g., on piercing corporate veil cases) for all relevant cases filed in District Courts, along with random assignment of judges setting precedent.

[22] 259 F.2d 54 (3d Cir. 1958).

[23] 18 F.3d 1181 (4th Cir. 1994).

[24] 702 F.2d 925 (11th Cir. 1983).

[25] United States v. Thomas, 74 F.3d 701 (6th Cir. 1996).

[26] United States v. Landham, 251 F.3d 1072 (6th Cir. 2001).

[27] In robustness checks, we collect another smaller dataset on District Court cases that are, to some extent, mergeable with courthouses.

TABLE I

Summary Statistics

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

with Songer and Haire's (1992) results. They interpreted these findings as suggesting that *Miller* was a doctrinal shift to a more conservative stance. Indeed, we observe a dramatic spike in both the number of free speech obscenity Circuit cases and the number of conservative free speech decisions immediately after *Miller* was decided. The salience and timing of this spike is consistent with people paying attention to these precedents.

3.2.2. *Judicial Biographies*

We compiled information on judges' characteristics from the Appeals Court Attribute Data, District Court Attribute Data,[28] Federal Judicial Center, and our own data collection. The final dataset includes information on vital statistics.[29] We added missing data by searching transcripts of Congressional confirmation hearings and other official or news publications on Lexis.

In our data, the average Circuit-year has 17.81 judges available for assignment to panels.[30] In expectation, there are 0.43 Democrats per seat (or 1.3 Democrats on a 3-judge panel) (Table I).[31]

3.2.3. *Attitudes and Behaviors*

We use the General Social Survey (GSS) to measure moral attitudes and sexual behaviors.[32] 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 survey provides information on the demographic characteristics of the respondents and their attitudes towards various situations and societal phenomena. The GSS provides responses from

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

[29] 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). Sisk's data are available at http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm. Judges whose religions remained missing or unknown were coded as having no publicly known religious affiliation.

[30] Some judges assigned to cases come from District Courts or specialized courts. In robustness checks, we omit these judges.

[31] The expected number of judges per seat is a proportion varying from 0 to 1. Senior judges sit less frequently and we weigh their characteristics based on the frequency a typical senior judge sits on cases in calculating expectations. In robustness checks, we omit senior judges and use the exact months in which judges are appointed or retire when calculating their availability.

[32] http://publicdata.norc.org:41000/gssbeta/index.html

approximately 1500 respondents for each survey year between 1973 and 1992, and approximately 2900 respondents per survey year from 1994 to 2004, for a total of 44,897 sample individuals between 1973 and 2004.

Our variables of interest are in two categories: (1) attitudes towards more progressive sexual behaviors such as premarital sex, extramarital sex, and same-sex sex; and (2) self-reports of one's actual sexual behaviors (e.g., number of partners last year, extramarital sex, or paid sex). 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. For attitudes on the morality of progressive sexual behaviors, we construct a binary indicator dividing the four possible responses: always wrong, almost always wrong, wrong only sometimes, or not wrong at all. Wrong only sometimes and not wrong at all are coded as "okay." This captures the difference in social perception, $\Delta(v) = E(v_a \mid v_a > v) - E(v_a \mid v_a < v)$. We also construct a measure for community standards using the response to whether sexual materials lead to breakdown of morals as one of the control variables since the *Miller* standard instructs judges to make decisions according to the community's standards. We use GSS survey weights in our regressions.

3.2.4. *Crime and Disease*

Statistics on sex and violent crime incidents come from the FBI's Uniform Crime Reports (UCR). These data are collected through voluntary reporting by local law enforcement agencies each year since 1960. Arrest data at the county level are available for prostitution, rape, and drug-related incidents and are constructed to be arrests per 100,000 population.[33] For the crime regressions, covariates for the 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 are obtained from official U.S. government publications.[34] County population numbers are used as weights.

Sexual behaviors may be observable beyond self-reports. The spread of venereal diseases, which have been mentioned as a secondary effect justifying obscenity regulation, may indicate riskier sexual practices. We obtain the incidence (new cases) of sexually transmitted diseases (STD) chlamydia, syphilis, and gonorrhea for each state from 1984 to 2008 from the Centers for Disease Control and Prevention[35] and extend syphilis and gonorrhea back to 1960 from 2003.[36] The STD incidence rates are weighted by annual state population numbers from the U.S. Census.[37]

[33] These UCR data are from the Inter-university Consortium for Political and Social Research. The UCR series have been criticized for underreporting criminal incidents because of the voluntary participation of law enforcement agencies. With sex crimes, stigma adds another level of underreporting from the victim's end. We assess whether changes in law enforcement or in self-reporting explain our findings.

[34] 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.

[35] 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.

[36] 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.

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

3.3. *Specification*

Even though we cannot ask any particular Circuit to randomize their decisions, a randomized control trial is effectively created through the random assignment of judges who interpret the facts and the law differently. The following conceptual illustration helps motivate the specification. Consider a Circuit with a high proportion of judges who are Democrats and another Circuit with a low proportion of judges who are Democrats. The empirical strategy does not rely on cases getting more Democrats in the first Circuit as opposed to the second Circuit, which could be different for unobserved reasons.[38] Rather, the strategy relies on the fact that, from year to year, the proportion of free speech cases in the first Circuit that are assigned to Democrats varies in a random manner. Since judicial assignments are not revealed to parties until after each litigant's briefs are filed, idiosyncratic variation cannot be predetermined. In the years when an unexpectedly high number of Democrats are assigned to free speech panels, the proportion of cases that will yield progressive free speech precedent is also high. Random variation in the assignment of Circuit judges is attractive as it varies in both the cross-section and the time series, so we do not rely on strong assumptions about the comparability of different regions (Circuits) and years. In this paper and related papers (Chen and Sethi 2011; Chen, Levonyan, and Yeh 2011; Chen and Yeh 2014b; Chen and Lind 2014), we use the random assignment of judges to estimate the effects of legal precedent on economy-wide outcomes, which we detail below again for clarity.

Our structural specification is a distributed lag:

$$Y_{ict} = \beta_0 + \sum_n \beta_{1n} Law_{c(t-n)} + \sum_n \beta_{2n} \mathbf{1}[\mathrm{M_{c(t-n)}} > 0] + \beta_3 C_c + \beta_4 T_t + \beta_5 C_c * Time$$

$$+ \sum_n \beta_6 W_{c(t-n)} + \beta_7 X_{ict} + \varepsilon_{ict}$$

The dependent variable, Y_{ict}, is a measure of sexual attitudes, behavior, crime, and disease of an individual, county, or state i in Circuit c and time t. We use population weights or survey weights provided by the dataset.

In principle, we have 612 experiments (51 years x 12 Circuits). With random treatment assignment, adding controls can add precision to the estimates if the controls are strong predictors of the outcomes. We show that our main estimates are fairly robust to the inclusion or exclusion of:

- Circuit-fixed effects, C_c, and time-fixed effects, T_t;
- Circuit-specific time trends, $C_c * Time$, to allow different Circuits to be on different trajectories with respect to outcomes;
- state-fixed effects to address the possible influence of state-specific obscenity regulations or state interpretation of federal laws;

[38] One might claim that the Fourth Circuit traditionally had more church-goers who think and act more conservatively than people in the Ninth Circuit, or that people in 2000 will admit to more progressive sexual practices compared to people in 1972. As a result, any observed differences in social mores would be due to the regional traditions or the spirit of the time, but are not due to the precedents themselves.

- a vector of observable unit characteristics, X_{ict}, depending on the unit being observed (for example, (a) at the individual level: age; and at the individual level entered as dummies: gender, educational attainment, and race; (b) at the state level: 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);
- and time-varying Circuit-level controls, W_{ct}, such as the characteristics of the pool of judges available to be assigned.
- $\mathrm{X_{ict}}$ also includes lagged community standards since the *Miller* standard requires judges to follow community standards in making their decisions.[39]

Our main specification clusters standard errors at the Circuit level. Our results are robust to using wild bootstrap (Cameron, Gelbach, and Miller 2008), simulations that randomly assign legal variation to another Circuit, and clustering standard errors at the Circuit-year level. This robustness is to be expected since random assignment of treatment addresses serial and spatial correlation of ε_{ict} (Barrios et al. 2012).

Our main specification uses distributed lags of $Law_{c(t-n)}$ that include four years of lags and one lead ($n = -1$ to 4), but we vary lag structure and include up to four leads. Distributed lags explicitly model the impulse response function and address violation of SUTVA when treatment and control occur repeatedly over time within a Circuit. Moreover, individuals may need time to adjust to judicial decisions.[40] $Law_{c(t-n)}$ is the proportion of Circuit Court cases with a progressive outcome. Typically $Law_{c(t-n)}$ is 1 (100% progressive) or 0 (100% conservative) and there is less than one obscenity decision per Circuit per year. We define $Law_{c(t-n)}$ to be 0 when there is no case and add a dummy $\mathbf{1}[M_{ct} > 0]$. In robustness checks, estimates become more precise when we weigh by the number of cases (weights are the geometric mean of $M_{c(t-n)} + 1$ over the previous four years, where $M_{c(t-n)}$ is the number of cases). Our main analyses report $\frac{\sum_n \beta_{1n}}{n}$ and joint significance of the lag coefficients.

Alternatively, we can omit $\mathbf{1}[M_{ct} > 0]$ if we define $Law_{c(t-n)}$ as the average of -1/0/+1 (progressive/no precedent/conservative decisions). This robustness check imposes the additional identification assumption—progressive and conservative decisions have opposite effects of equal size in absolute value. Our main specifications do not impose this restriction.

If free speech obscenity decisions and population outcomes are systematically correlated for unobserved reasons, then β_1 is biased. For example, social trends may drive judicial decisions, so ascertaining a causal effect from judicial decisions to social trends is difficult. A particular form of endogeneity arises because the *Miller* and *Roth* test for determining whether an expression is obscene relies on community standards of sexual conduct. If social mores are progressive, courts may be more likely to make progressive decisions, creating upward bias in OLS estimates. On the other hand, if harms from secondary effects are perceived to be high, courts may be more likely to rule conservatively, creating downward bias in OLS estimates.

[39] We define community standards using an index of views on the effects of pornography. We construct a two-year bin summarizing the five- to six-year lag of community standards because our main specification includes four lags of the law.

[40] The effects of a law may also fade as expectations or statutory regimes adjust or Circuit splits are resolved.

3.3.1. *Instrumental Variable*

The rise of the religious right movement in the U.S. for a large part of the twentieth century (Chen and Lind 2014) means that Republicans would be associated with socially conservative views on matters of free speech. We develop an instrumental variable using judges' biographical characteristics and we will refer to the number of judges assigned to free speech panels with these characteristics as N_{ct} (e.g., Democrat).

If a Circuit-year has a higher fraction of progressive judges (N_{ct}/M_{ct}) assigned, the precedent for that year will be that much more progressive. Were we only interested in the contemporaneous effect of Law_{ct}, the moment condition for causal inference is $\mathbf{E}[(N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct}))\varepsilon_{ict}] = 0$. $\mathbf{E}(N_{ct}/M_{ct})$ is the expected proportion of judges who tend to be progressive in free speech cases. Figure 5 illustrates the identification strategy. The jagged line displays N_{ct}/M_{ct} and the smooth line displays $\mathbf{E}(N_{ct}/M_{ct})$ in the U.S. (Figure 5A) and in each of the 12 Circuits (Figure 5B). The smooth lines indicate the underlying variation in judge-specific characteristics within Circuits over time. The jagged line indicates the random year-to-year variation in Democrats per seat. We estimate how outcomes Y_{ict} respond to idiosyncratic variation in excess proportion, $N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct})$.

Next, we construct an instrumental variable whose moment conditions are implied by the original moment condition. Consider an instrument, $p_{ct} - \mathbf{E}(p_{ct})$, where p_{ct} is the proportion of judges who tend to be progressive in free speech cases and p_{ct} is defined as 0 when there are no cases:

$$p_{ct} = \begin{cases} N_{ct}/M_{ct} & \text{if } \mathbf{1}[\mathrm{M_{ct}} > 0] = 1 \\ 0 & \text{if } \mathbf{1}[\mathrm{M_{ct}} > 0] = 0 \end{cases}$$

Without loss of generality, N_{ct} is the number of Democrats per seat in free speech cases in Circuit c and year t. These two conditional moment conditions imply $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$ unconditionally: $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{Pr}[M_{ct} > 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} > 0] + \mathbf{Pr}[M_{ct} = 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} = 0] = 0$. Furthermore, $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}[\mathbf{E}(p_{ct})\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}(p_{ct})\mathbf{E}(\varepsilon_{ict}) = \mathbf{E}[p_{ct}\varepsilon_{ict}]$. We show that $\mathbf{E}(p_{ct})$ is uncorrelated with Law_{ct} in Figure 7B.

3.3.2. *Interpretation*

In common law, hard cases (compliers) precede easy cases (always-/never-takers). Cases that reach the Circuit Courts are the more challenging and legally innovative cases. According to one Circuit judge's estimate, about 15% of cases are hard and have no strong legal precedent. In these cases, judges seek guidance, often from policy arguments (Posner 1998b; Breyer 2006; Abramowicz et al. 2011), and their biographical characteristics may influence decisions when there is no obvious precedent. Ambiguity has also been theoretically shown to cause polarization along partisan lines (Abdellaoui et al. 2014) and even when judges intend to be fair, misperceptions of what is fair can significantly affect case outcomes (Chen et al. 2014b). Our 2SLS estimates capture the effects of hard cases, but these are also the very cases in which judges seek guidance.

This perspective yields $\sum_{n=0}^{\infty}\beta_{1n} = \sum_{n=0}^{\infty} TOT_{ct}^{n}$, where TOT_{ct}^{n} denotes treatment-on-treated of cases n years ago. In principal, we only know $LATE$ and not TOT.[41] 2SLS estimates of β_{10} measure

[41]Recall, $TOT \equiv E[Y_{1i} - Y_{0i}|R_i = 1] = E[Y_{1i} - Y_{0i}|R_{1i} > R_{0i}]Pr(R_{1i} > R_{0i}|R_i = 1) + E[Y_{1i} - Y_{0i}|R_i = 1]Pr(R_{1i} =$

the effect of hard cases at $t = 0$ (the compliers whose decisions are affected by judicial biography). β_{1n} captures hard cases n years ago; their effects at $t = 0$ can be decomposed into delayed economic effects and to subsequent easy cases (the always-takers and never-takers for any $t > -n$) that cite the hard cases at $t = -n$. Thus, $\sum_{n=0}^{\infty} \beta_{1n} = \sum_{n=0}^{\infty} TOT_{ct}^{n} = \sum_{n=0}^{\infty} LATE_{ct}^{n}$.

Indirect inference reveals that the effect of hard free speech cases is largely through cases subsequently not litigated or not published in Circuit Courts: Few cases occur per Circuit-year and contemporaneous judicial composition is not correlated with subsequent free speech decisions in the Circuit. The absence of subsequent easy cases following prior hard cases in the published record is not surprising since Circuit cases should bring issues of new law. Litigants should settle the easy cases; even if they do not, judges should leave easy cases unpublished. Since published cases are predominantly hard cases, they are responding to biographical characteristics. In addition, the strong first stage suggests that any bias that results from the presence of non-compliers is likely to be small.[42]

3.3.3. *Dynamic Effects*

Many Circuit-years do not have any decisions. Dummying for missing values allows estimating additional counterfactuals. $\beta_{1(t-n)}$ captures the effect of progressive precedent where the counterfactual is a conservative precedent, $\beta_{1(t-n)} + \beta_{2(t-n)}$ captures the effect of progressive precedent where the counterfactual is no precedent, and $\beta_{2(t-n)}$ captures the effect of conservative precedent where the counterfactual is no precedent.

However, litigants' decisions to appeal may respond to previous years' legal decisions, so controlling for $\mathbf{1}[M_{ct} > 0]$ may bias the coefficient for Law_{ct}; the bias is more severe for more distant lags and non-existent for the most advanced lead. We assess whether this potential endogeneity is a significant concern by comparing $\beta_{1(t-n)}$ when we instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. District judge demographic characteristics are correlated with reversal rates in Circuit Courts (Haire, Songer, and Lindquist 2003; Sen 2011; Barondes 2010; Steinbuch 2009); and expected reversal rates could encourage litigants to pursue an appeal. If $\mathbf{1}[M_{ct} > 0]$ and Law_{ct} are both identified, estimates should be roughly invariant to the inclusion or exclusion of additional lags and leads[43] and lead coefficients being 0 provide an omnibus check of our instrumental variable being endogenous to pre-existing trends. In our tables, we show average lag and lead effects in OLS, 2SLS with Circuit IV, and 2SLS specifications with Circuit and District IV to assess the degree to which the endogeneity concerns we describe for Law_{ct} and $\mathbf{1}[M_{ct} > 0]$ are important for estimating $\beta_{1(t-n)}$.

To instrument for $\mathbf{1}[M_{ct} > 0]$, we define our District IV in two ways. In the first definition, $w_{ct} = \frac{K_{1t} * \left(\frac{L_{1t}}{K_{1t}}\right) + \ldots + K_{6t} * \left(\frac{L_{6t}}{K_{6t}}\right)}{K_{1t} + \ldots + K_{6t}}$, where K_{it} denotes the number of cases filed and L_{it} denotes the number of judges with a particular characteristic for cases in District *Court* i and time t.[44] In

$R_{0i} = 1 | R_i = 1$), where R_i indicates whether i received treatment, $R_{1i} > R_{0i}$ indicates whether individual i is a complier and $R_{1i} = R_{0i} = 1$ denotes an always-taker, under the assumption of no defiers.

[42] The bias from non-monotonicity is given by $\frac{Pr[Defier]}{Pr[Complier] - Pr[Defier]}(\beta^{Complier} - \beta^{Defier})$, which is likely to be small even if there are non-compliers, since the magnitude of the first stage is large (the denominator grows and the numerator shrinks) using biographical characteristics as the instrument.

[43] Including lags that are important predictors of the outcome improves statistical precision, but losing data at the beginning and end periods reduces precision.

[44] Without loss of generality, let i go to a maximum of 6.

the second definition, $\tilde{w}_{ct} = K_{1t} * \left(\frac{L_{1t}}{K_{1t}} - E\left(\frac{L_{1t}}{K_{1t}}\right)\right) + ... + K_{6t} * \left(\frac{L_{6t}}{K_{6t}} - E\left(\frac{L_{6t}}{K_{6t}}\right)\right)$ where i denotes District *courthouse* or *Court*. A District Court has several courthouses (also referred to as Divisions); in some Districts, random assignment is at the courthouse level. The second definition addresses two issues[45]: the location of takings controversies, K_{it}, may be endogenous[46] and the presence of a case in a district courthouse is not guaranteed. The weighted sum, rather than the collection, of $\frac{L_{it}}{K_{it}} - E\left(\frac{L_{it}}{K_{it}}\right)$ as instruments lessens the $K_{it} = 0$ problem because it increases the chances of observing at least one district case in every Circuit-year. Unfortunately, merging in courthouse information, which we tried to link in from the Administrative Office of the U.S. Courts (AOC) database on all cases filed via docket number, is not possible for many cases, which results in $\tilde{w}_{ct}$ being undefined in over 50% of Circuit-years[47], so our main tables use w_{ct}.

3.3.4. *Many Instruments*

A large number of biographical characteristics serve as valid instruments, which would result in a weak instruments problem if we used them all.[48] There are two ways to reduce dimensionality: a priori theory and model selection. LASSO (least absolute shrinkage and selection operator[49]) (Belloni et al. 2012) is commonly used for model selection. We find that characteristics related to religion, political party, and having attended non-elite schools are important in predicting free speech obscenity decisions, perhaps because social issues divide starkly along religious and political lines in the U.S. In our main specifications, we display results using LASSO alongside those using only Democrat as the source of variation.

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[45]

Fact The Law of Iterated Expectations (LIE) implies $E\left(K_i * \left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\right) = 0$.

Proof Using LIE, $E\left(K_i * \left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\right) = E\left(E\left[K_i * \left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\middle| K_i\right]\right)$. Rearranging results in: $E\left(K_i E\left[\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\middle| K_i\right]\right)$. Again by LIE: $E\left[\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\middle| K_i\right] = E\left[E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\middle| \epsilon_{ct}, K_i\right)\middle| K_i\right]$. Rearranging once again: $E\left[\epsilon_{ct} E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right)\middle| \epsilon_{ct}, K_i\right)\middle| K_i\right]$. The expression $\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)$ is the deviation of the ratio of judge assignment characteristics from the mean, so it should be independent of ϵ_{ct} and $K_1, ..., K_6$. Therefore, $E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right)\middle| \epsilon_{ct}, K_i\right) = 0$.

[46] Shifts in K_{it} may occur due to endogenous economic or government activity at the District courthouse level or due to special interests funding cases in certain locations. This formulation also permits endogenous shifts in $E\left(\frac{L_{it}}{K_{it}}\right)$, for example, due to District judges' appointment, retirement, or movement between courthouses.

[47] When we use $\tilde{w}_{ct}$, we include a dummy for missing values in $\tilde{w}_{ct}$ and define $\tilde{w}_{ct}$ to be 0 when it would otherwise be missing.

[48] Roughly 30 characteristics enter in levels (Democrat, male, racial minority, black, Jewish, Catholic, No religion, Mainline Protestant, Evangelical, bachelor's degree (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, decade of birth (1910s, 1920s, 1930s, 1940s, or 1950s), appointed when the 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), judge-level interactions (e.g., racial minority Democrats), and panel-level interactions (e.g., fraction of judge seats assigned to Democrats multiplied by fraction of judge seats assigned to racial minorities) yielding a total of several thousand possible instruments.

[49] LASSO has sparseness and continuity, which OLS lacks. With OLS, large subsets of covariates are deemed important, resulting in too many instruments, which makes 2SLS susceptible to a weak instruments problem. Small changes in the data result in different subsets of covariates deemed important. Formally, LASSO modifies OLS by adding a data penalty for having too many large coefficients. The model minimizes the sum of squares subject to the sum of the absolute value of the coefficients being less than a constant, which tends to set some coefficients to exactly 0 and hence reduces model complexity.

3.3.5. *Separate First Stages*

With many endogenous variables and many instruments, there is a danger of overfitting with instruments from the wrong year. In robustness checks, we use the contemporaneous instruments to predict $Law_{c(t)}$ and $\mathbf{1}[M_{ct} > 0]$ and use the fitted values as instrumental variables in 2SLS.[50]

3.3.6. *Plausible Exogeneity*

Exclusion Restriction The identity of the judge plausibly affects economic outcomes through the legal precedent alone. Even if judges' identities are known, the court decision is taken as precedent by subsequent courts. Badawi and Chen (2014) verifies that the identity of judges only affects economic outcomes through legal precedent in an area of law where markets pay close attention to the courts and judges.[51]

Randomization 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.[52] Chen and Sethi (2011) formally tests for randomization by showing that case characteristics as determined by District Courts are not correlated with the characteristics of the Circuit judges assigned to the case.

Omnibus Tests Even if judges are randomly assigned, because our data comprise published opinions, several additional issues need to be considered: settlement, publication, and strategic use of citation. In Circuit Courts, judges are revealed very late, after litigants file their briefs, sometimes only a few days before the hearing, if there is a hearing, which gives little opportunity and incentive for settlement upon learning the identity of the panel. Most of the litigation costs are sunk by that point, and when the D.C. Circuit began announcing judges earlier, it did not affect settlement rates (Jordan 2007). Unpublished cases are not supposed to have precedential value.[53] We cannot rule out

[50] To see how, suppose:

$$Y_{ict} = \beta_{10} Law_{c(t)} + \beta_{11} Law_{c(t-1)} + ... + \varepsilon_{ict}$$

Let the first stage be: $L_{c(t)} = Z_0\Pi_0 + u_0$, where $Z_0 = \left[\ p_{c(t)}\ \right]$ and $L_{c(t-1)} = Z_1\Pi_1 + u_1$, where $Z_1 = \left[\ p_{c(t-1)}\ \right]$. Set $\hat{X} = \left[\ \hat{L}_{c(t)} \quad \hat{L}_{c(t-1)} \quad ... \quad \hat{L}_{c(t-j)}\ \right]$ for $j = 0, 1, ...$, where $\hat{L}_{c(t-j)} = Z_j\hat{\Pi}_j = Z_j(Z_j'Z_j)^{-1}Z_j'L_{c(t-j)}$.

Observe that $\hat{\beta} = (\frac{\hat{X}'X}{n})^{-1}\frac{\hat{X}'Y}{n} = \beta + (\frac{\hat{X}'X}{n})^{-1}\frac{\hat{X}'\varepsilon}{n}$. Let $\hat{Q} = (\frac{\hat{X}'X}{n})$, then $\sqrt{n}(\hat{\beta} - \beta) = \hat{Q}^{-1}\frac{\hat{X}'\varepsilon}{\sqrt{n}}$. $\frac{1}{\sqrt{n}}\hat{X}_j'\varepsilon = \frac{1}{\sqrt{n}}\frac{X_jz_j}{n}(\frac{z_j'z_j}{n})^{-1}z_j'\varepsilon = \hat{\Gamma}\sqrt{n}\frac{z_j'\varepsilon}{n}$. Since $\sqrt{n}\frac{z_j'\varepsilon}{n} \to N(0, \Phi_j)$, so $\sqrt{n}(\hat{\beta} - \beta) \to N(0, V), V = Q^{-1}\Gamma\Phi\Gamma Q^{-1}$.

[51] Stock prices do not respond to the assignment of judges to corporate cases and judge identity does not predict stock prices at the time of resolution controlling for the manner in which the case was resolved.

[52] Senior judges can opt out of death penalty cases in some Circuits, but they would do so before random assignment. Other variations from random assignment include: remanded cases from the Supreme Court are returned to the original panel; en banc cases are heard by the entire pool of judges (or a significant fraction in Circuit 9); judges with conflict of interests opt out after random assignment, which is extremely rare. We do not use remanded or en banc cases, which are also relatively infrequent.

[53] Unpublished cases are deemed as routine and easy: studies find that judicial ideology predicts neither the decision in unpublished cases (Keele et al. 2009) nor the decision to publish (Merritt and Brudney 2001).

strategic use of keywords or citation of Supreme Court precedent, so we propose a (weak) omnibus[54] test to collectively address deviations from strict exogeneity: We examine how similar the string of actual panel assignments is to a random string (Chen 2013).

Figure 5 suggests visually that panel composition is not serially correlated. Formally, we:

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

We use the following statistics:

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

Mean-Reversion: We test whether there is any form of mean reversion in the sequence, meaning that the assignment in the n$^{\text{th}}$ case is correlated with the assignment in previous $n-1$ cases. This test tells us whether judges or their assignors were attempting to equilibrate their presence, considering whether a judge was "due" for a free speech case.

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

Number of Runs: Instead of simulating 1000 random strings, we compute the exact statistic for number of runs. This test captures violations of randomization at the case level rather than Circuit-year. In power calculations, this test has less Type II error compared to the other tests.[55]

With a truly random process, the collection of all unit p-values should be uniformly distributed. The 1001$^{\text{th}}$ random string should have a summary statistic that is equally likely to be anywhere from 1 to 1000. A visual examination suggests that the empirical distributions for our p-values for physical and regulatory takings approach the CDF of a uniform distribution (Appendix Figure 1), which we formally test using a Kolmogorov-Smirnov test statistic from an empirical null in Table II.[56][57]

[54] To see random strings as an omnibus test: Suppose Democrats publish free speech cases and other judges do not. Suppose this publication tendency is correlated with social trends, then we should expect observed assignment of Democrats in published cases to violate the random strings test.

[55] These random strings tests also have lower Type II error compared to regression.

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

[57] For details about District Courts, we refer the reader to Chen and Yeh (2014b). We assume that remaining deviations from random assignment (like vacation days) are ignorable.

TABLE II

Randomization Check: P-values

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

3.4. *First Stage*

Studies have discussed the relationship between judges' personal attributes and their voting behavior (Chang and Schoar 2013; Ellman, Sunstein, Schkade 2004). In particular, Democrats have been found to favor parties raising a constitutional challenge to accusations of unlawful obscenity (Sunstein, Schkade, Ellman, and Sawicki 2006; Songer and Haire 1992), and this voting pattern holds even when controlling for other factors, such as litigant characteristics, major shifts in Supreme Court obscenity doctrine, types of legal arguments, and other case characteristics since 1957 (Songer and Haire 1992). Moreover, the pattern holds at the panel level: Circuit panels that are randomly assigned two or more Democratic appointees are more likely to deliver a progressive obscenity decision (2006).

Table III shows that Democrats are 10% more likely to vote progressively in free speech obscenity cases (Panel A).[58] At the case level, an additional Democrat per seat on a three-judge panel increases the chances of a progressive decision by 26% (Panel B). At both the judge level and case level, point estimates and statistical significance remain stable or increase with controls for Circuit, year, and the expected proportion of Democrats per seat. At the Circuit-year level, an additional Democrat per seat increases the proportion of progressive decisions by 36% (Panel C). [59] The F-statistic is 6.7 and increases with the inclusion of controls up to 10.4.

At the level of analysis, which merged laws with individual data, the estimates are slighly different because of the differing numbers of individuals per Circuit (Panels D and E). The joint F statistic on the instrument is past the conventional threshold for weak instruments at betwen 12 and 13 (Stock and Yogo 2005), and the F statistics again increase with the inclusion of fixed effects and additional Circuit-year controls. The slope of the first stage also increases in recent years, which can be seen in the merged GSS and UCR analysis (Column 6 of Panels D and E); this finding is consistent with increasing polarization in the U.S. along social issues and political party (Chen and

[58] All analyses in this section cluster standard errors at the Circuit level.

[59] The Circuit-year estimates are slightly different from the case level since cases are not evenly distributed across Circuit-years. Not every Circuit-year has a case and cases can bunch up unevenly across Circuit-years. For an example of how a coefficient can differ between Circuit-year and case level, suppose there are 4 cases, one case each with 0, 1, 2, or 3 judges who are Democratic appointees, and suppose that the panel makes a progressive decision when there are 3 Democratic appointees. If 1 Circuit-year has the case with 0 Democratic appointee and the other Circuit-year has the remaining 3 cases, the coefficient at the Circuit-year level is 0.5 (= difference in percent progressive/difference in Democratic appointees assigned per seat) but when the 1 Circuit-year with the case has the case with 1 Democratic appointee, the coefficient at the Circuit-level is 1.5.

The estimates and statistical significance are robust regardless of whether the Circuit-years with no cases are dropped or are dummied and the proportion of progressive decisions and judge type per seat are set to 0 for those Circuit-years with no cases. The R-square increases significantly.

TABLE III

First Stage: Relationship Between Progressive Free Speech Decisions and Democratic Appointees on Appellate Free Speech Panels, 1958-2008

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

Notes: Significant at +10%; *5%; **1%. 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 defind to be 0. Panel D: GSS (1973-2004) weights are sampling weights. Individual-level controls are age, gender, race, and college education. Panel E weights are population of state or reporting agency. State-level controls are percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income.

TABLE IV

Falsification Test of Instrument: Relationship Between Progressive Free Speech Decisions 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 +10%; *5%; **1%. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the Circuit level. Proportions of progressive free speech decisions and judicial type per seat during Circuit-years with no cases are defind 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.

Lind 2014). A falsification of the instrumental variables shows that this kind of legal variation is not related to the instrument in the one or two years before and after the true instrument (Table IV).

To check whether our (linear) specifications miss important aspects of the data, Figure 7A presents nonparametric local polynomial estimates of the effect of number of Democrats per seat on the proportion of progressive decisions. Estimation proceeds in two steps. In the first step, we regress the proportion progressive on Circuit and year-fixed effects and we regress the number of Democrats per seat on the same. Next, we take the residuals from these two regressions and use the nonparametric local polynomial estimator (Epanechnikov kernel) to characterize the relationship between Democrats and progressive decisions. The relationship is monotonically increasing and not driven by outliers. In other words, having randomly more Democrats assigned corresponds to unexpectedly more progressive free speech obscenity decisions. Figure 7B shows that there is no relationship between expected number of Democrats per seat and the proportion of progressive free speech obscenity decisions.

The instruments chosen by the LASSO procedure we verify do not vary much with the inclusion or exclusion of controls, though when the number of observations per Circuit-year or the number of years of data varies across outcomes, the instruments do vary. The F statistics increase significantly to 37 for attitudes on the morality of extramarital sex and homosexual sex, and to 104 for behaviors like partners per year and paid sex with 4 instruments each.[60]

4. THE IMPACT OF FREE SPEECH LAWS REGULATING OBSCENITY

4.1. *Summary*

On average, from 1958 to 2008, progressive free speech precedent spurred progressive sexual attitudes and behavior as well as secondary effects of crimes and disease. Table V summarizes the

[60]Given the large number of outcomes, we suppressed the reporting of LASSO-selected instruments, but these are available on request.

following parameters for each outcome: $\beta_{1(t-n)}$, $\beta_{1(t-n)} + \beta_{2(t-n)}$, and $\beta_{2(t-n)}$, scaled by the number of cases per year to report the typical effect per year of free speech precedent.[61] One hundred seventy-five decisions occur over the 51-year time period, so the typical Circuit-year is unlikely to experience a free speech obscenity precedent.

The first column indicates that progressive–as opposed to conservative–decisions yield a positive impact on most outcomes. The second column reports that progressive decisions–as opposed to no decision–still yields positive impacts on sexual attitudes and behaviors, but some effects on crime become negative.

This difference for crime may reflect displacement since government actors may defer issuing regulations restricting obscenity until a favorable legal regime and the absence of a case serves as a "supercontrol" (Crépon et al. 2013). The lack of displacement effects for sexual attitudes and behavior is not surprising since, if law is providing some norm-shifting information, there is no reason to expect individuals to defer their norm changes until a favorable legal regime. This interpretation is further supported by our experimental results.

The third column shows impacts of having any free speech appeal and is equivalent to the effect of a conservative decision vs. no decision. Since the majority of decisions are conservative, the third column presents a mixed picture on the overall impact of Circuit free speech obscenity laws during this time period.

4.2. *Attitudes*

Table VI reports the effects of free speech precedent on sexual attitudes. Across columns, we present OLS and 2SLS estimates, using only variation from assignment of Democrats, variation from LASSO-selected judicial characteristics, and/or variation in District judge assignment to instrument for presence of a case. To streamline presentation, only the average lag effect ($\frac{\sum_n \beta_{1n}}{n}$) and the joint significance of lags and leads are reported.[62] Appendix Table II reports the full set of coefficients along with additional statistics of interest, such as p-values from a wild bootstrap of the LASSO specification to assess the validity of clustering standard errors when there are only 12 Circuit clusters.

The average lag effect is jointly significant and estimated to be 0.008 with LASSO (Column 4), meaning that, when there is one decision in that Circuit-year, a progressive decision increased the morality of extramarital sex by 0.8 percentage points on average per year for the following four years relative to a conservative decision. The mean dependent variable indicates that 9.7% of the population believe extramarital sex is never wrong or wrong only sometimes. Progressive free speech decisions increased the morality of premarital sex by 1.4 percentage points out of a mean of 63% of the population believing that premarital sex is okay. It increased the morality of same-sex sex by 0.3 percentage points out of a mean of 27% of the population believing same-sex sex is okay.

[61] To compute the effect of pro-government precedent in a typical Circuit-year, we can multiply the coefficient on Law_{ct} by $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$, the typical proportion of decisions that are pro-government when there are Circuit cases, and by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of Circuit-years with a Circuit case. A similar calculation can be made for the typical effect of pro-government precedent taking into account the presence of an appeal: $\mathbf{1}[M_{ct} > 0]$*$\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]$+$Law_{ct}$*$\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]$. The results of these calculations are presented in Appendix Tables II, V, VI, VII, X, XI, XII, XV, and XVIII.

[62] Because attitudes may be autocorrelated, it is customary to rely on joint tests of significance to determine whether the law's effects are statistically significant.

TABLE V

Summary of Results

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

TABLE VI

The Effects of Free Speech Precedents on Sexual Attitudes

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

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

4.3. *Sexual Behavior*

We next turn to tangible manifestations of the shift in attitudes. Table VII reports that sexual behavior becomes more liberal after progressive free speech decisions. Progressive free speech precedent increased the likelihood of paid sex by 0.4 percentage points (the mean dependent variable is 0.3%), number of partners per year by 0.13 (relative to a mean of 1.13), and total number of female partners by 5 (relative to a mean of 6.3). The increase is driven by men, who reported 0.3 more partners per year and 11 more female partners. Appendix Tables V to VII report the full set of coefficients and more details.

After progressive decisions, men were 7 percentage points more likely to have extramarital sex (relative to a mean of 16%). Individuals older than 40 were 1.1 percentage points more likely to be divorced or separated. Those under 40 were 3.9 percentage points less likely to be divorced or separated. Since these data rely on self-reports, we report an online experiment to test whether the increase in progressive sexual behaviors was simply due to people being more open to discussing their behavior.

4.4. *Crime*

Sexual crimes provide an alternative method to validate self-reports of attitudes and behavior, and in particular, to evaluate the secondary effects of free speech law that has concerned advocates and policy-makers. Table VIII shows that child abuse (offenses against family and children) decreased by 56 arrests per 100,000 individuals in the population while prostitution (community vices) increased by 3 arrests per 100,000 individuals in the population. The secondary effects of drug violations also increased by 35.5 arrests per 100,000 individuals in the population (Table 5B). The increase in forcible rapes was not statistically significant. We report a placebo test of free speech law's impact on property crimes, which shows no effect.[63] These results contrast with a previously documented correlation of a decrease in sex crimes after relaxing government restricts on explicit materials (Ben-Veniste 1971). Appendix Tables X to VII report more details and the full set of coefficients.

Arrest data to measure crime occurrences can be incomplete. They are susceptible to underreporting, particularly by victims in sex-related crimes. In conservative areas, people may be less likely to report rapes. However, in conservative areas, people may be equally likely to stigmatize the reporting of child abuse, yet progressive decisions decreased reports of child abuse.

Resources or decisions of local law enforcement can also determine the number of arrests observed. Law enforcement departments often heed changes in the law, and more progressive laws on pornography may put some departments on alert, and lead them to more aggressively investigate or make arrests. On the other hand, it is perhaps equally likely–if not more likely–that conservative free speech precedent empowers police to arrest more often, but we find an increase in arrests of prostitutes following progressive precedent. Moreover, self-reported paid sex increased with progressive precedent, which is consistent with the increase in prostitution arrests reflecting an actual increase in prostitution.

[63] These results are robust when defining the instrument for variation in free speech precedent using the number of judicial panels with at leastone1 Democratic appointee and with at least a Democratic majority, while controlling for the number of free speech cases.

TABLE VII

The Effects of Free Speech Precedents on Sexual Behaviors

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

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

TABLE VIII

The Effects of Free Speech Precedents on Sexual Crimes

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

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

4.5. *Disease*

Examining sexually transmitted disease (STD) outcomes addresses the police power rationales in free speech regulations to protect health and welfare. They also provide another measure to counteract concerns about the reliability of self-reported sexual behaviors. Table IX reports that free speech effects on sexually transmitted disease incidence vary depending on disease type. The point estimates on gonorrhea and syphilis are positive but not jointly significantly so. Progressive free speech precedent increases chlamydia incidence by 49 per 100,000, relative to a mean of 208. The full set of results are presented in Appendix Table XV. These results are consistent with Kremer (1996), whose model predicts people who are sexually active may sort and actively select against partners with visible STDs like gonorrhea and syphilis.

Chlamydia, known as the "silent" disease, typically produces no symptoms for several years among 70% of infected women and 50% of infected men in general. In one study, 86% of the infected partners of infected women were also found to be asymptomatic (Fish et al. 1989). Gonorrhea produces some visible symptoms in most men and mild or no symptoms in many women. About 90% of men infected with gonorrhea display symptoms within days days of infection, and 40-70% of infected women have symptoms within 10 days (Kretzschmar et al. 1996). Syphilis symptoms include sores within 10 to 90 days and rashes within 1 to 6 months of the primary infection. With more partners, one would expect a higher probability of infection. However, knowledge of a partner's STD could deter people from having sex or increase condom use to reduce the risk of transmission. Condom use, however, does not differentially affect transmission rates across these STD types (Holmes et al. 2004); even if people practice safer sex, chlamydia incidence could increase.[64] Given the non-significant increase of visible STDs (gonorrhea and syphilis) and the fact that infection rates are determined to a large extent by condom use (Nelson and Williams 2007), at least some of the increase in invisible STDs may be due to sorting or differential use of safe sex practices depending on the visibility of STDs.[65]

4.6. *Threats to Validity*

We examine to what extent our empirical framework approximates a randomized control trial through a series of robustness checks. We investigated all outcomes discussed in this paper, but due to space constraints, we discuss tables for one outcome in each category: extramarital sex, paid sex, offenses against family and children, and chlamydia. The average effects are presented in Appendix Tables III, VIII, XIII, and XVI, respectively. The full set of distributed lags are presented in Appendix Tables IV, IX, XIV, and XVII.

4.6.1. *Leads*

Tables VI to IX report that leads of free speech precedent are insignificant, which serves as a falsification check to verify that pre-existing social mores are not spuriously driving the observed effects of the law.

[64] We are unable to assess to what extent the increase in chlamydia is due to the increased number of sexual partners, changes in the practice of safe sex, or sorting.

[65] An alternative explanation for these results is differences in screening by disease type. If health care providers observe an increase in sexual activity (consistent with our findings of more partners), then they may respond by screening for chlamydia more frequently. Perhaps progressive free speech precedent causes screening for chlamydia to increase but not screening for gonorrhea and syphilis. Screening for STDs, however, typically occurs simultaneously.

TABLE IX

The Effects of Free Speech Precedents on Sexual Diseases

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

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

Further, the point estimates in a regression including four years of leads and one lag of the law are small in magnitude and not jointly statistically significant (last four columns of the last line in Appendix Tables IV, IX, XIV, and XVII. The standard errors are similar in magnitude to the lag effects, suggesting the lead coefficients are near 0 rather than being imprecisely estimated.

4.6.2. *Lag Structure*

Random assignment at both the District and Circuit Court level allows identification of the impulse response function without much concern about the number of lags and leads. The estimates are indeed robust to varying the lag structure beyond the main specification of four lags of the law and one lead. The last five rows of the Appendix Tables present results using only one lag, only two lags, two leads and four lags, or one lead and five lags. The magnitudes and precision of point estimates of the effect of progressive free speech decisions are stable across lag structures. Effects are typically strongest one to two years after the decision.

4.6.3. *Covariate Controls and Outlier Circuits*

How much does the inclusion or exclusion of covariates matter in the estimates of the effects of free speech decisions? The Appendix Tables report results that alternatively remove Circuit-specific time trends, Circuit- and year-fixed effects, individual-level controls, survey weights, community standards, and all controls except the presence of cases. Estimates generally become more precise with the inclusion of controls, especially fixed effects. The impact of free speech law on paid sex and offenses against family and children are particularly stable, with the point estimates being similar in magnitude across specifications.

A particular Circuit might be an outlier that could obscure the estimates. For example, the Ninth Circuit, which has a historically progressive reputation, may have very idiosyncratic responses to free speech decisions. We therefore drop observations from one Circuit at a time and re-run regressions on data from the remaining 11 circuits. The main results are robust. The average lag effects are

relatively stable and typically jointly significant.

4.6.4. *Over-Identification*

The random assignment of judges to Circuit free speech cases and the fact that their votes are correlated with their biographies produces a tremendous amount of experimental variation. While intuition motivates the random assignment of Democrat vs. Republican for identification, our estimates are robust when using alternative sets of instruments from the LASSO procedure.

In additional robustness checks, we construct the instrument as deviations from expectation (rather than control for expectations), drop judges (such as those who took senior status and those who are visiting from other courts) where we lack information about their expected assignment probability, and implement separate first-stages to reduce over-fitting. In specifications using District IV, we use $\tilde{w}_{ct}$. We also omit the need for District IV altogether with specifications where progressive decisions have a value of +1 while conservative decisions have a value of -1. We check the Anderson-Rubin weak-instruments robust test statistic. Estimates yield results qualitatively similar to other sensitivity checks.

4.7. *Backlash to Expressive*

We now turn to an analysis of the effects of free speech law during and after the sexual revolution. Given the rapidly changing social mores at that time, the model suggests that backlash should occur when relatively few individuals engage in stigmatized activities, whereas expressive law should occur when many individuals engage in the previously-stigmatized activities. Historical studies document backlash by conservatives to stop the Supreme Court from encroaching on state rights to control pornography during the 1950s and 1960s (Walker 2011). To conduct this analysis, our data are limited by the fact that the General Social Survey and Uniform Crime Reports begin data collection in the 1970s and a large number of years are needed to have a significant sample. For simplicity, we display the results for 1973-1993 vs. 1980-2000, though we checked that the results are robust to some variation in these cutoffs. First, we confirm that the first stage F-statistic remains high at 8.9 and 9.5, respectively for the two time periods. Table X shows that there is indeed generally a strong backlash effect in the earlier time period.

Paid sex, community vices (arrests for prostitution), partners per year, and social perception of homosexual sex all *decrease* following progressive free speech precedent when the sample is restricted to earlier years, whereas the opposite is true in later years. Moreover, the fact that self reports of paid sex and arrests for prostitution move in tandem suggests that the arrest data might not simply be due to police reporting bias.

The results further indicate that even if we remove the early 1970s spike in cases, free speech law still has an expressive effect, on average; the expressive effect is much larger once the early years' backlash effect is removed.

5. EXPERIMENT

Attitudes and behaviors in the GSS are self-reported. It is possible that an increased rate of sexual behaviors is due to openness in discussing topics previously considered to be private. We reject this mechanism using an online experiment with data entry workers whose final paragraph of data entry is a newspaper summary of a recent free speech obscenity decision, randomized to be progressive or

TABLE X

The Effects of Free Speech Precedents over Time

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

Notes: Significant at +10%, *5%, **1%. Attitudinal and behavioral data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and 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.

conservative. Our experiment uses progressive free speech decisions related to homosexuality to test the effects of free speech obscenity law.[66] The experiment verifies a response in progressive attitudes about same sex relations and is consistent with the authors' published results in a similar study (Chen and Yeh 2014a).

5.1. *Methodology*

We recruit workers through a labor market intermediary (LMI), Amazon Mechanical Turk. The LMI is designed to recruit a large number of workers in a short amount of time. Through an interface provided by the LMI, registered users perform tasks posted by buyers for money. The tasks are generally simple for humans, yet difficult for computers to do. Common tasks include captioning photographs, extracting data from scanned documents, and transcribing audio clips. The LMI also allows a researcher to implement randomization, although randomization is not inherent to the LMI. Although most buyers post tasks directly on the LMI website, they are also able to host tasks on an external site. We use this external hosting method: we post a single placeholder task containing a description of the work at the LMI and a link for workers to follow if they want to participate. The subjects are then randomized, via stratification in the order in which they arrived at the job, to one of several treatment conditions. Treatment is not revealed at this early stage. All workers see identical instructions.

The LMI can be used to implement anything from a natural field experiment to a laboratory

[66] Of the 175 obscenity cases in our database, 45% mention "gay" or "lesbian" in the opinion. Including the historical identifier, "pervert," increases the proportion of cases related to homosexuality to 65%.

experiment (Harrison and List 2004). Workers come to the marketplace naturally and are unaware they are in an experiment at the time of arrival; this lack of awareness alleviates the Hawthorne effects–demand or experimenter effects associated with knowing that one is participating in an experiment (Orne 1962; Titchener 1967). Even if people become aware of an experiment when asked to complete questions from the GSS, they are unaware that other subjects receive different treatment conditions. We ask workers to transcribe paragraphs from a Tagalog translation of Adam Smith's *The Wealth of Nations* as well as English paragraphs of dictionary definitions. This task is sufficiently tedious that no one is likely to do it "for fun," and it is sufficiently simple that all market participants can do the task. The source text was machine-translated to prevent subjects from finding the text elsewhere on the Internet. Time and money are the most cited reasons for participation in Mechanical Turk[67] Because subjects are unaware of an on-going experiment, differential attrition may arise at the time treatment is revealed (Reips 2001). We minimize attrition through a commitment mechanism. In all treatment conditions, workers face an identical "lock-in" task in order to minimize differential attrition before the treatment is revealed.

The payment for each paragraph is 10 cents with workers able to receive much more in bonuses, including a 50-cent bonus for completing the survey from the GSS at the end. A paragraph takes about 100 seconds to enter so the offered payment of 10 cents per paragraph is equivalent to $86.40 per day. At the time of the experiment, the federal minimum wage in the Unites States was $58/day. In India, payment rate depends on the type of work done, although the "floor" for data entry positions appears to be about $6.38/day.[68] An example paragraph is displayed on the first page of the external hosting site so workers are aware of the high payment before entering the study. In fact, one worker emailed saying that 10 cents was too high and that the typical payment for this sort of data entry was 3 cents per paragraph.

After a lock-in task of three paragraphs, treatment is revealed. This lock-in successfully reduces attrition (Chen and Horton 2009; Chen 2012). The data entry paragraphs are printed in the Appendix.

5.2. *Results*

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

$$(1) \qquad Outcome_{it} = \alpha + \beta_1 Treatment_t + \beta_2 X_{it} + \varepsilon_{it}$$

We control for whether the data worker is male and, in the experiment with 197 workers from around the world–mostly from India and the U.S.–a dummy indicator for whether the individual is from India. The second experiment restricts to the U.S. and has 548 workers. Progressive treatment is defined as 1, 0 (for control), or -1 (for conservative treatment).

[67] http://behind-the-enemy-lines.blogspot.com/2008/03/mechanical-turk-demographics.html. Some workers do it out of need. A disabled former United States Army linguist became a Turk Worker for various reasons and in nine months he made four thousand dollars (New York Times, March 25, 2007). Some drop out of college to pursue a full time career with these disaggregated labor markets (Web Worker Daily, October 16, 2008, Interview with oDesk CEO). For more information about the motivation and demographics of Mechanical Turk workers, see, e.g. Paolacci et al. (2010).

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

TABLE XI

The Effect of Exposure to Progressive Free Speech Decisions on Sexual Attitudes and Behaviors

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

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

Tables XI and XII report that progressive free speech precedent made people more likely to say homosexual sex is acceptable in both experiments (Column 4). At the baseline, 48% of workers said that homosexual sex is acceptable. Workers exposed to progressive decisions were 6% more likely to say homosexual sex is acceptable. The effects are similar in a probit specification (not shown). These effects remain even when Treatment 4, which explicitly refers to homosexual sex, is excluded. No effects on other sexual attitudes are found.

We do not observe self-reported sexual behaviors to shift in response to exposure to progressive free speech decisions (Table XI). Shifts in attitudes but not self-reported behaviors was replicated in Chen and Yeh (2014a) using 600 U.S. workers. Since these questions are asked immediately after data entry, actual behaviors are unlikely to change. This suggests that self-reporting norms are unlikely to explain the results from the population-based portion of our analyses.

Our second experiment replicates the findings from the first experiment. Exposure to progressive free speech decisions increase the perceived morality of same-sex sex by 4% (out of a baseline of 74%) and the likelihood that people favor sex education in public schools by 4% (out of a baseline of 66%). Exposure to conservative free speech jurisprudence does not increase beliefs about the negative externalities of free speech, such as whether sexual materials lead to the breakdown of morals or whether sexual materials lead to rape.[69]

5.3. *Model Validation*

We also investigate whether exposure to progressive free speech decisions affect beliefs about social norms. Verifying a key assumption in the theoretical model, workers reported believing a higher percentage of people have extramarital sex after being exposed to conservative free speech

[69]Neither experiment found this effect, results are available on request.

TABLE XII

THE EFFECT OF EXPOSURE TO PROGRESSIVE FREE SPEECH DECISIONS ON SEXUAL ATTITUDES AND BELIEFS

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

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

decisions. The effect is 2.5 percentage points out of a mean of 44.5%. This shift in norm perception helps explain the reversal from backlash to expressive effects of free speech law during and after the sexual revolution.

When previously-stigmatized activities are relatively scarce and conservative free speech decisions cause people to update their beliefs that the stigmatized activities are more common than previously thought, these activities become normalized, and the social multiplier causes more people to do the previously-stigmatized activity. A large number of free speech obscenity decisions occured during the sexual revolution and a large number were decided conservatively, greatly increasing the information multiplier. In the aftermath of the sexual revolution, progressive free speech decisions weaken the ability for individuals with high intrinsic motivations to signal their value by abstaining from normalized activities.

5.4. *Comparison Between Experimental and Population-Based Analyses*

These estimated effects are quite a bit larger than the estimates from the population-based analysis, which documents that a progressive free speech decision–as opposed to a conservative decision–led to a 0.3% points increase in likelihood to view homosexual sex as okay (Table VI). In contrast, in Tables XI and XII, someone who was exposed to a progressive free speech decision–as opposed to a conservative decision–was 1.7% to 3.2% points more likely to view homosexual sex as okay.[70] These effects are robust to dropping the control group.

The much larger magnitude in the experiment compared to the population-based analysis is possible since we do not know how many people in the population are directly or indirectly exposed to progressive free speech decisions. To see this, observe that the population analysis of a single lag estimates $LATE$, which is the effect on compliers. $LATE$ + effect on always-takers = TOT (Treatment on Treated) of the Circuit = (TOT_{direct} + TOT_{indirect} of individuals) * P(individual exposure in treated circuit). The experiment estimates TOT_{direct} for individuals.[71]

6. CONCLUSION

Social scientists and philosophers have long debated whether law shapes values. We show that fifty years of free speech jurisprudence shaped moral values and also led to secondary harms that judges and feminist scholars have opined about. Random assignment of judges to Circuit panels effectively

[70] The point estimates need to be divided by two to make this comparison because the law variable is coded as -1/0/+1 in the experiments rather than 0/1 in the population analysis.

[71] 0.3% + effect on always takers = (3.5% + TOT_{indirect}) * P(individual exposure in treated circuit)

created random variation in free speech jurisprudence. We find that progressive free speech precedent caused more liberal attitudes on non-marital sexual activity and behaviors relating to non-marital sexual activity, and higher rates of invisible STDs but not visible STDs. Progressive free speech precedent may have also increased prostitution and drug violation, providing empirical support for concerns by U.S. lawmakers (50 AM. JUR.2d §§ I, 2 (1995)). Corroborating the expressive–rather than deterrence–channel, workers randomly assigned to transcribing newspaper summaries of progressive (as opposed to conservative) court decisions reported more liberal sexual attitudes (but not sexual behaviors) and beliefs about the percentage of people who have extramarital sex decreased.

On a policy matter, we emphasize that we evaluate the effects of free speech obscenity law rather than pornography itself, but our findings may also inform the debate about pornography. With recent expansions of broadband Internet access and the adult entertainment market (Edelman 2009; Bhuller et al. 2013), understanding the direct effects of pornography is relevant, especially when its consumption is the highlighted channel through which laws might influence individual behavior. The large majority of studies linking pornography exposure to individual outcomes show correlations (Strouse et al. 1995; Martino et al. 2006; Brown et al. 1991) that are susceptible to reverse causality and omitted variable bias or results from small laboratory experiments that may lack external validity (Martino et al. 2006; Brown et al. 1991; Donnerstein and Linz 1986). We did not find an effect on magazine circulation, but future research should assess how much of the overall effect of free speech obscenity law is attributable to shifts to other forms of media or the type of content being presented.

Our results are consistent with a model of law and norms where legal decisions inform people about the social norms. Prohibitions cause people to think that the government sees a problem and that stigmatized activities are more prevalent, an assumption that we confirm in our experiment. Those who are motivated by intrinsic incentives have an easier time in signaling to others as honorable, as they realize fewer people are pretending to have high intrinsic value. We call this an "expressive effect," when law causes what is viewed as moral to shift towards what the law values. This expressive effect, however, only arises when a sufficient number of people perform the stigmatized activity. When few people do the stigmatized activity, the morality of stigmatized activities can increase substantially if the shift in beliefs cause stigmatized activities to become normalized: more people with higher intrinsic value are now perceived to do the stigmatized activity. When the normalizing effect exceeds the signaling effect, we call this a "backlash effect," as the law causes what is viewed as moral to shift against what the law values. This perspective helps explain why free speech decisions during the sexual revolution had a backlash effect while those in the aftermath have an expressive effect and may help explain why, for example, harsh sentencing in some hate crimes can be feared to lead to backlash.[72] Our study joins a growing literature that estimates the causal effects of cultural influences on economic outcomes (Gentzkow and Shapiro 2008; Dahl and Della Vigna 2009) and large, and primarily theoretical, literature on law and values (Appiah 2008; Tushnet 2009; Acemoglu 2012; Bénabou and Tirole 2012) and legitimacy (Tyler 2006; Gibson 1989; Dewatripont and Roland 1992).

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

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Figure 1

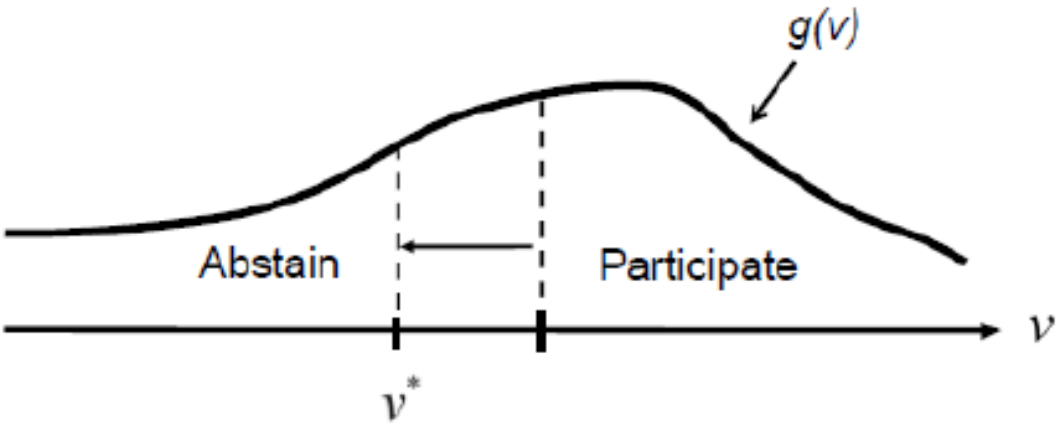


Figure 2

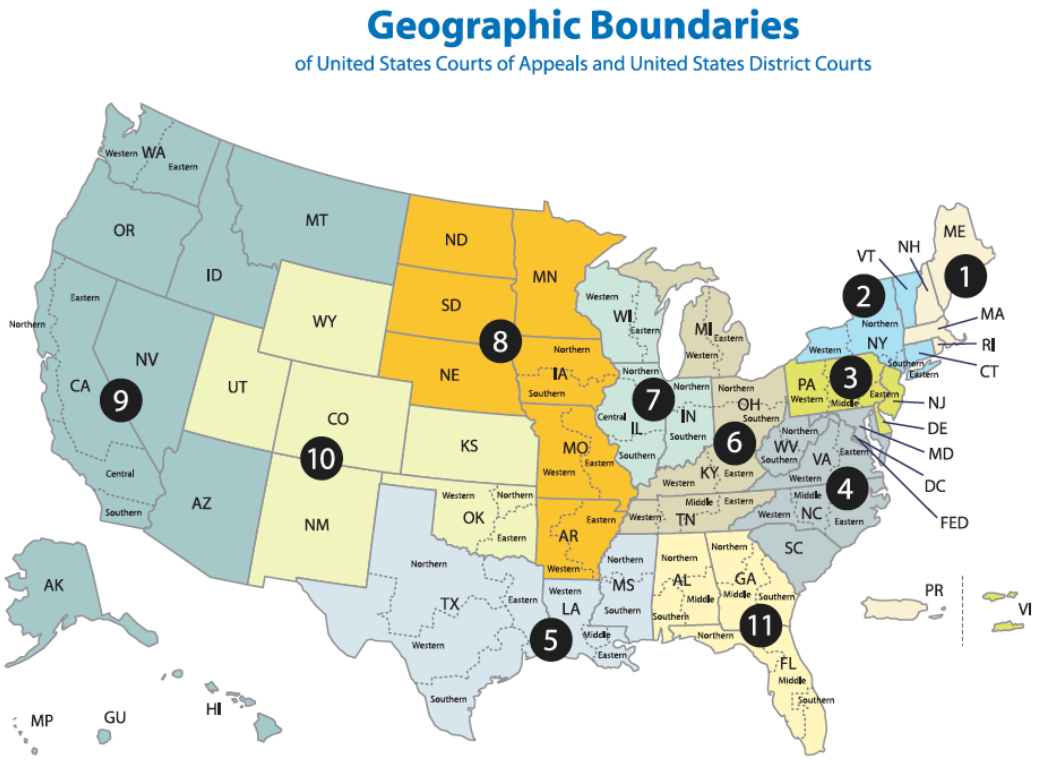
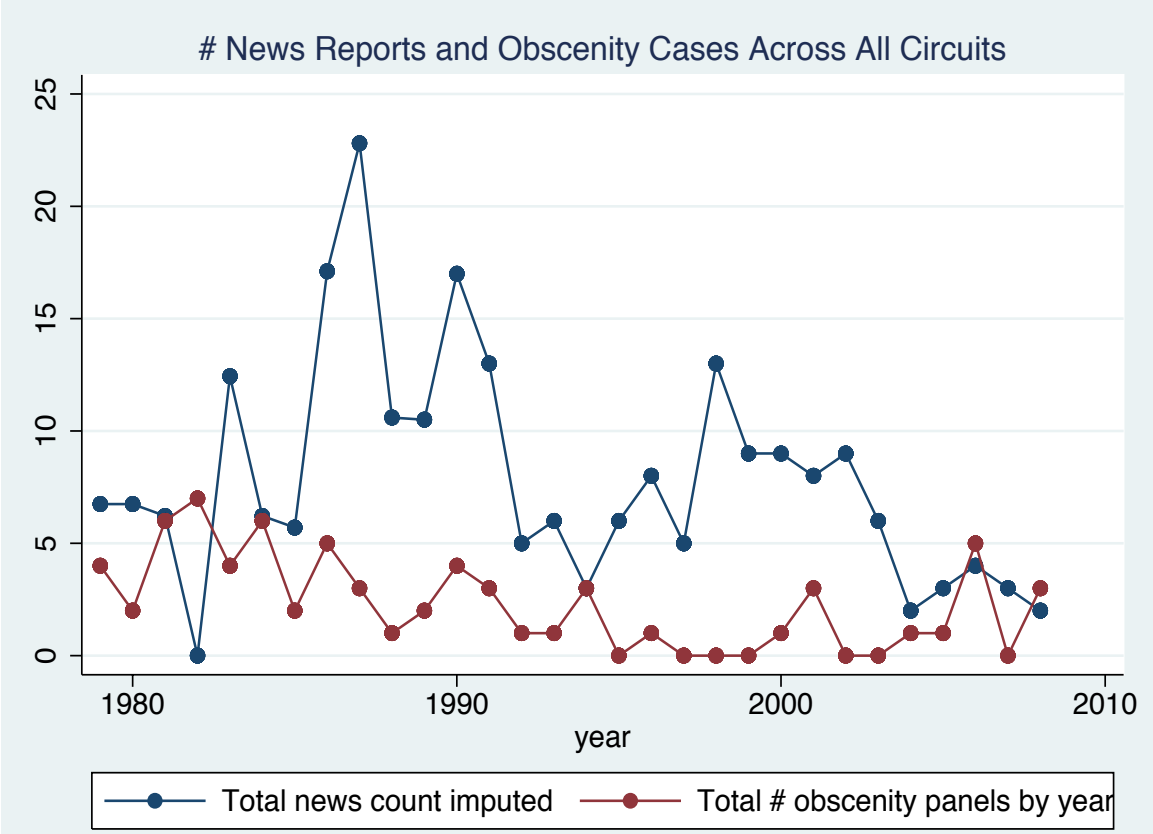


Figure 3


News Reports and Obscenity Cases Across All Circuits
0
5
10
15
20
25
1980
1990
2000
2010
year
Total news count imputed
Total # obscenity panels by year

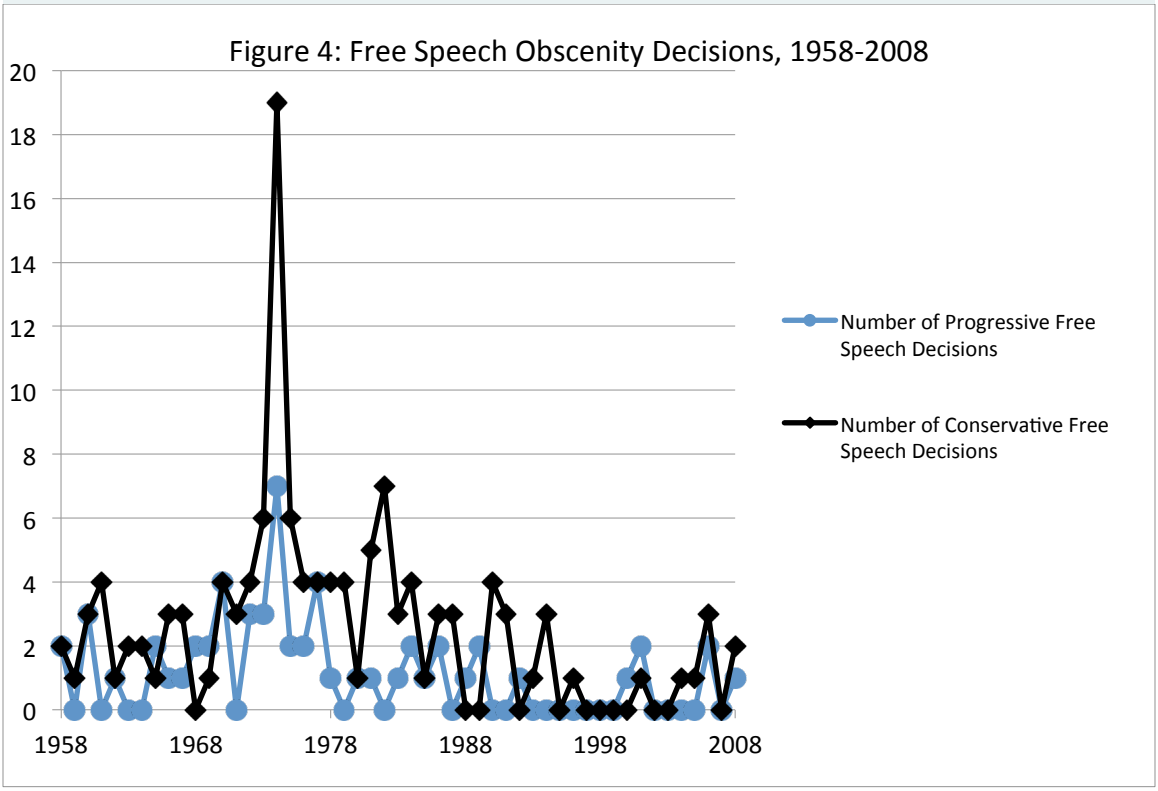

Figure 4: Free Speech Obscenity Decisions, 1958-2008
0
2
4
6
8
10
12
14
16
18
20
1958
1968
1978
1988
1998
2008
Number of Progressive Free Speech Decisions
Number of Conservative Free Speech Decisions

Figure 5A

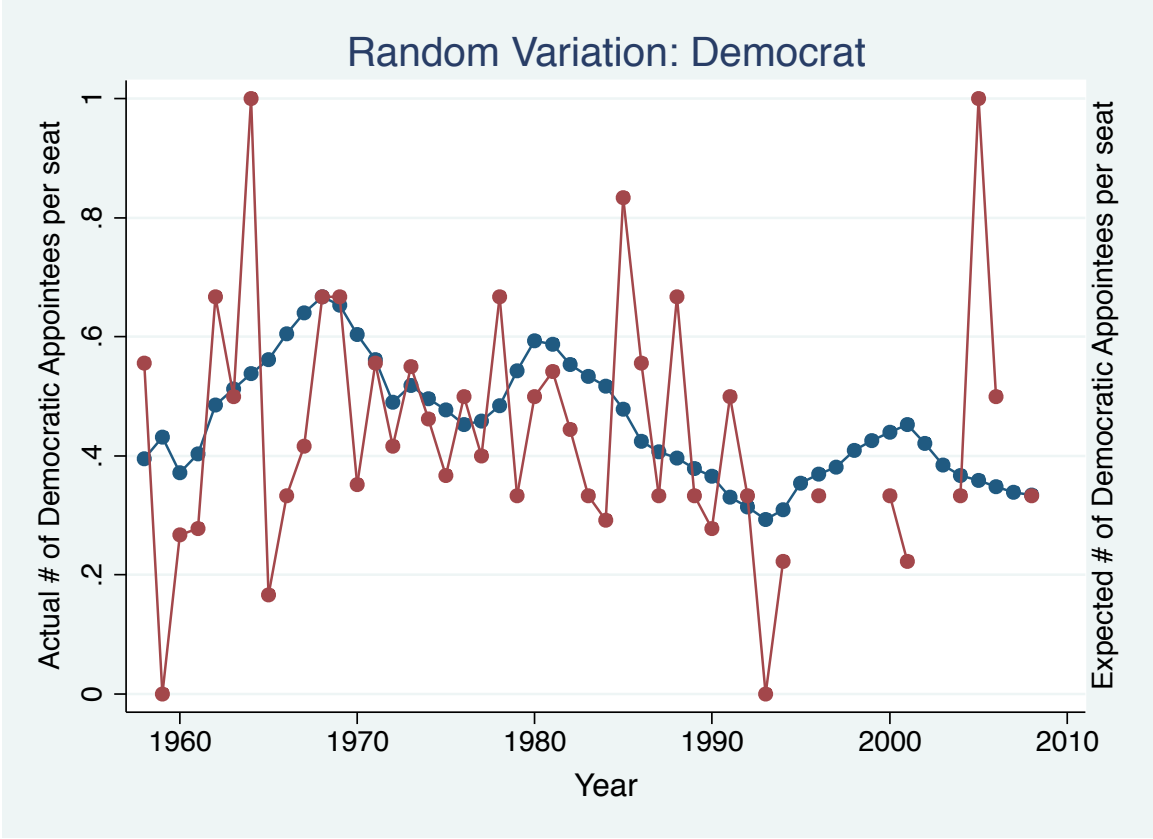


Figure 5B

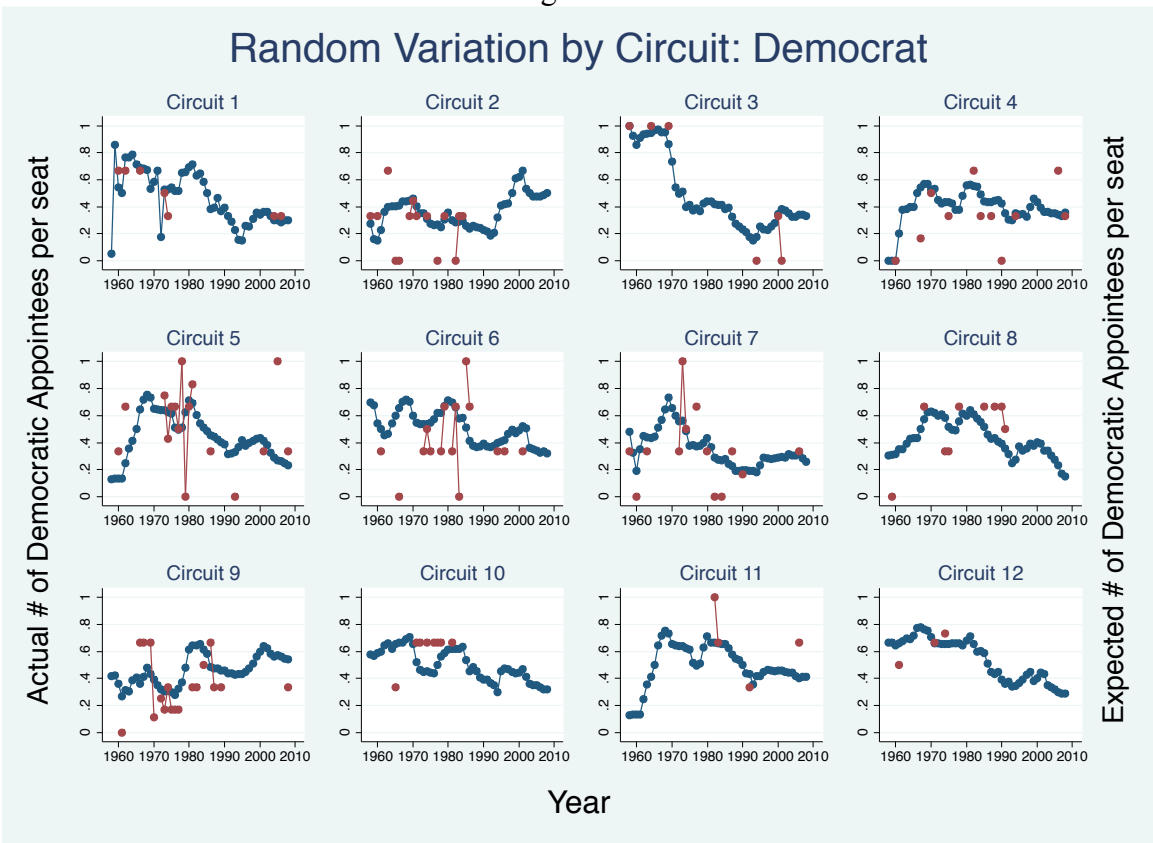
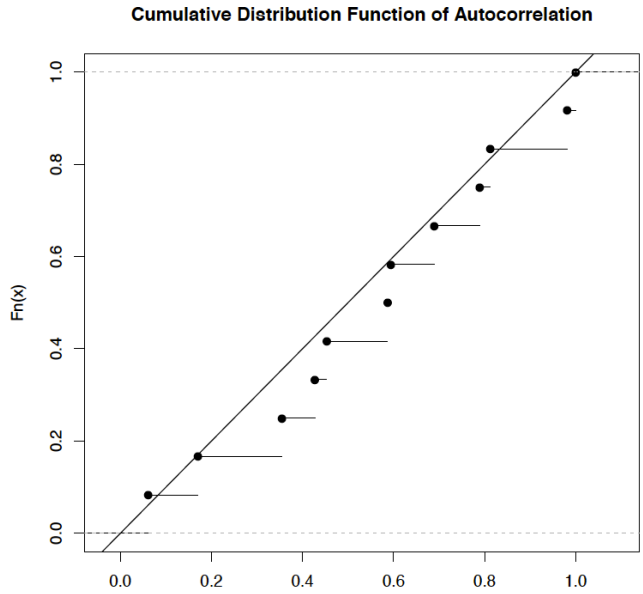
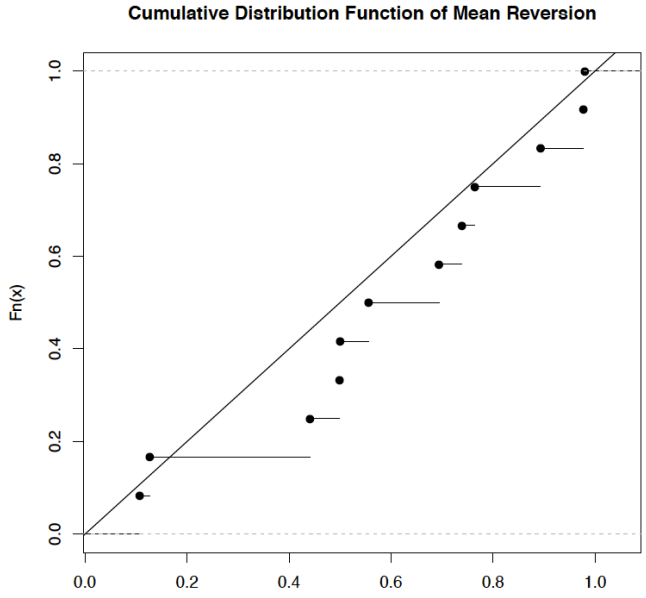


Figure 6: Randomization Check
P-Values of Democrat Appointee strings





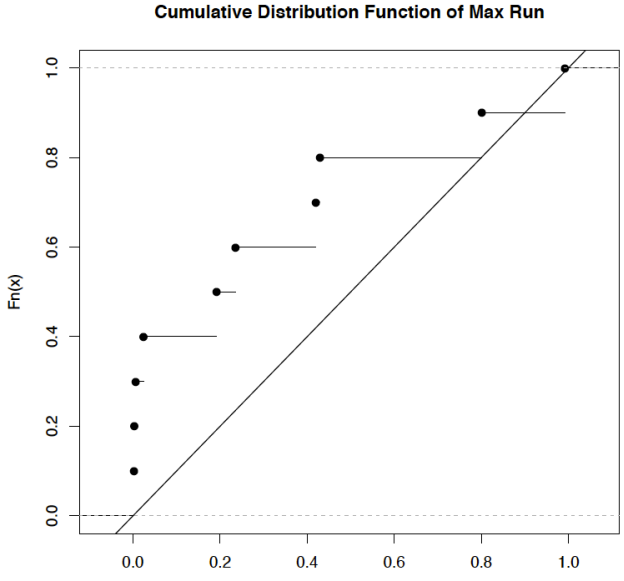


Figure 7A

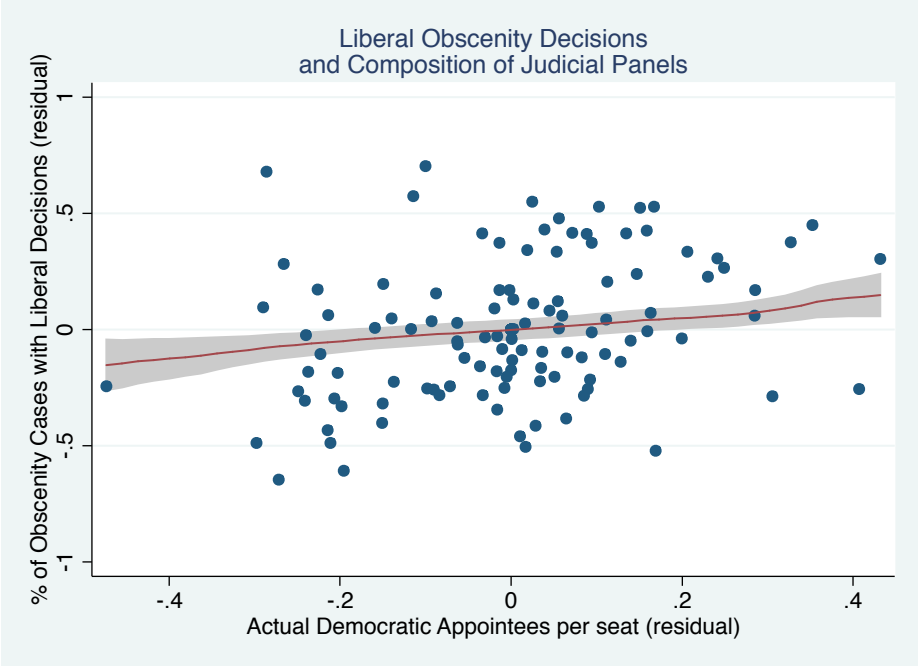
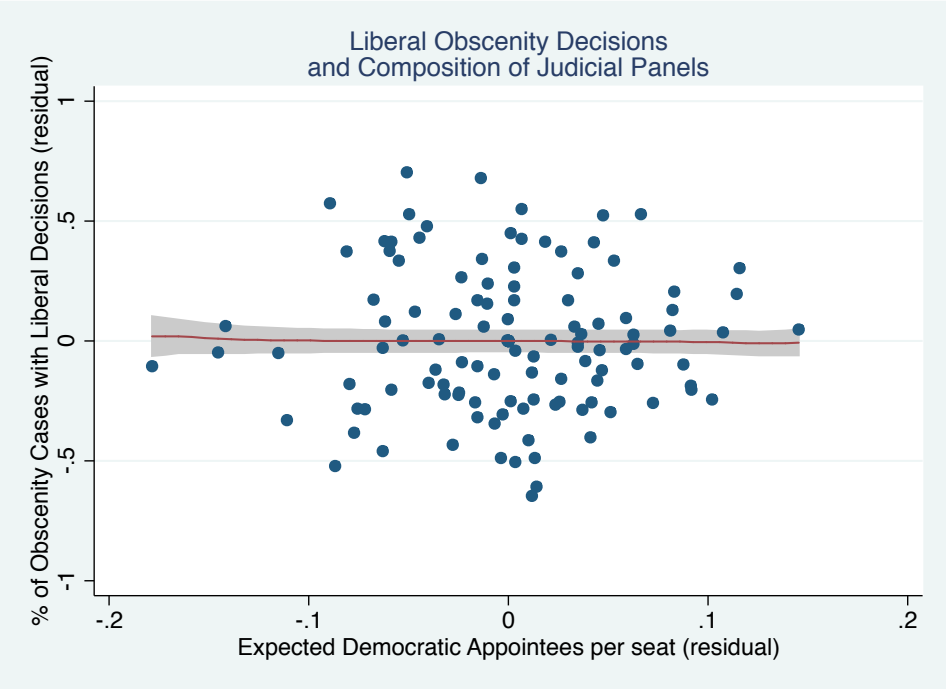


Figure 7B



Nonparametric local polynomial estimates are computed using an Epanechnikov kernel. Rule-of-thumb bandwidth is used. Shaded area indicates 90 percent confidence bands. The residuals are calculated removing circuit and year fixed effects.

APPENDIX A: U.S. LEGAL BACKGROUND: TESTS APPLIED TO DETERMINE WHAT IS OBSCENE

Protecting the moral fabric of society has persisted as a fundamental rationale in U.S. Circuit and Supreme Court free speech cases pertaining to obscenity.[73] "History has shown that obscenity cases can be decided mainly on the basis of moral harm and do not necessarily rest on potential physical harm or other effects" (Koppelman 2005) (Louis Henkin, quoted in Andrew Koppelman, p.1637, FN 12.). Major doctrinal developments from Circuit cases are shown below.

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

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

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

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

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

APPENDIX B: PARAGRAPHS FOR DATA ENTRY EXPERIMENT

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

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

[73] Both moral harms and their "secondary effects," e.g., crime and venereal disease, 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).

because they applied only to films and photos sold in hardcore pornography stores. The speech was not completely limited since other stores, such as regular newsstands, could still sell the material.

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

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

Treatment 4 (Progressive Obscenity Precedent): The Boys of Cocodorm – Snow Bunni, J Fizzo, et al – are staying put, after a federal judge ruled that the gay porn website has a right to film out of its Edgewater home. Cocodorm.com features black and Hispanic men, known as "dorm dudes," who share a webcam-filled house together and have sex on schedule. For that they are paid at least $1,200 a month, plus free room and board. Miami has tried to shut the house down, arguing it constitutes an adult business illegally operation in a residential area. The city's Code Enforcement Board in 2007 agreed, but Cocodorm responded to the code enforcement proceedings by suing in federal court. From the outside, the Cocodorm house looks like any other residence. Those who want to see Cocodorm's "hottest and horniest" do so via the Internet, with a credit card.

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

APPENDIX C: APPENDIX FIGURES AND TABLES

Appendix Table I: List of Free Speech Appellate Precedent

Citation	Case Name	Circuit	Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
252 F.2d 333	Glanzman v. Schaffer	2	1958	0	prohibition on sending payment for obscene material through the mails	"obscene material"
262 F.2d 357	United States v. Padell	2	1958	0	prohibition on mailing obscene material	books containing "dreary pornography"
259 F.2d 54	United States v. Keller	3	1958	1	prohibition on mailing obscene material	postcards containing references to adultery
260 F.2d 670	Capitol Enterprises, Inc. v. Chicago	7	1958	1	Chicago, IL obscenity ordinance	sexually explicit film
271 F.2d 140	Alexander v. United States	8	1959	0	shipment of obscene materials via common carrier	sexually explicit books
273 F.2d 799	Flying Eagle Publications, Inc. v. United States	1	1960	1	prohibition on mailing obscene material	"lewd, lascivious, vile, indecent..."--partially clothed illustration of a woman accompanying
283 F.2d 780	Collier v. United States	4	1960	0	prohibition on mailing obscene material	circulars telling where obscene material might be found; pornographic photographs
276 F.2d 433	Grove Press, Inc. v. Christenberry	2	1960	1	prohibition on mailing obscene material	Lady Chatterley's Lover--book containing explicit sex
274 F.2d 598	Cain v. United States	5	1960	0	prohibition on mailing obscene material	letter containing sexually explicit language
273 F.2d 529	Empire Pictures Distributing Co. v. Ft. Worth	5	1960	1	Fort Worth, TX city ordinances banning a movie theater from showing exp	pornographic films
277 F.2d 631	United States v. Hochman	7	1960	0	transportation of obscene materials in interstate commerce using a comn	sexually explicit books
294 F.2d 204	Womack v. United States	12	1961	0	mailing obscene matter	pornographic ads
289 F.2d 455	Manual Enterprises, Inc. v. Day	12	1961	0	mailing obscene matter	pornographic magazines
290 F.2d 517	United States v. Oakley	6	1961	0	prohibition on mailing obscene materials	pornographic photographs
293 F.2d 449	Ackerman v. United States	9	1961	0	mailing obscene material	sexually explicit letters
309 F.2d 362	Excellent Publications, Inc. v. United States	1	1962	1	prohibition on mailing obscene material	photos of nude/partially nude women
300 F.2d 78	Kahm v. United States	5	1962	0	prohibition on mailing obscene materials	pornographic written materials, advertisements for those materials
316 F.2d 813	United States v. Darnell	2	1963	0	prohibition on mailing obscene material	private letter using swear words
316 F.2d 873	United States v. Zuideveld	7	1963	0	prohibition on mailing obscene materials	sexually suggestive magazines, membership in a sexual pen pal club
338 F.2d 12	United States v. Ginzburg	3	1964	0	prohibition on mailing obscene material	pornographic magazine
333 F.2d 963	Outdoor American Corp. v. Philadelphia	3	1964	0	PA obscenity statute	pornographic magazines
350 F.2d 155	United States v. Klaw	2	1965	1	prohibition on mailing obscene material	pornographic illustrations
353 F.2d 614	United States v. Davis	2	1965	0	prohibition on mailing obscene material	records and record labels which depicted sex in some way
340 F.2d 59	Haldeman v. United States	10	1965	1	mailing obscene matter	sexually explicit pamphlets and advertising
358 F.2d 935	Books, Inc. v. United States	1	1966	0	prohibition on mailing obscene material	book explicitly describing characters' sexual adventures
359 F.2d 402	Wenzler v. Pitchess	9	1966	0	CA obscenity law	pornographic videos
357 F.2d 855	United States v. West Coast News Co.	6	1966	0	prohibition on mailing obscene material, transportation of obscene mate	sexually explicit book
367 F.2d 889	United States v. One Carton Positive Motion Picture Film	2	1966	1	prohibition on importation of obscene material	sexually explicit film which also depicts self-mutilation
385 F.2d 209	Culbertson v. California	9	1967	1	CA statute prohibiting sale of obscene material	photographs of "scantily clad women"
373 F.2d 635	United States v. 56 Cartons	4	1967	0	importation of obscene material	pornographic magazines
373 F.2d 633	United States v. 392 Copies of Magazine	4	1967	0	importation of obscene material	pornographic magazines
384 F.2d 694	Armijo v. United States	9	1967	0	mailing obscene material	sexually explicit letters
404 F.2d 196	United States v. A Motion Picture Entitled "I am Curious-Yellow"	2	1968	1	prohibition on importation of obscene material	film with sexually explicit scenes
389 F.2d 200	Luros v. United States	8	1968	1	prohibition on mailing obscene materials	nudist magazines and sexually explicit novels
418 F.2d 1051	United States v. Baranov	9	1969	1	mailing obscene material	booklets containing pornographic photos
418 F.2d 82	Grove Press, Inc. v. Philadelphia	3	1969	1	PA obscenity statute and common law nuisance	pornographic film
422 F.2d 34	United States v. Wild	2	1969	0	prohibition on mailing obscene material	slides of pornographic images
435 F.2d 228	Drive In Theatres, Inc. v. Huskey	4	1970	1	NC state obscenity law as interpreted by the Rutherford County sheriff	any movie not rated G
436 F.2d 1289	Overstock Book Co. v. Barry	2	1970	0	distribution of pornography	books, magazines, etc. which included "hard-core pornography"
431 F.2d 655	Miller v. United States	9	1970	0	mailing obscene material	obscene books, magazines, and ads
433 F.2d 1252	United States v. Dellapia	2	1970	1	prohibition on mailing obscene material	pornographic films
433 F.2d 932	United States v. Jacobs	9	1970	0	mailing obscene material	pornographic photographs and ads
431 F.2d 272	Childs v. Oregon	9	1970	0	disseminating obscene matter in violation of OR state law	sexually explicit book
432 F.2d 705	United States v. 35 MM. Motion Picture Film etc.	2	1970	1	importation of obscene material	sexually explicit film
432 F.2d 420	United States v. Ten Erotic Paintings	4	1970	1	importation of obscene material	sexually explicit paintings
470 F.2d 386	Huffman v. United States	12	1971	0	DC obscenity ordinance	pornographic magazines
448 F.2d 583	United States v. Manarite	2	1971	0	prohibition on mailing obscene material	pornographic magazines, films, and playing cards
445 F.2d 945	United States v. Ewing	10	1971	0	mailing obscene matter	pornographic material and advertisements
467 F.2d 41	United States v. Pellegrino	9	1972	1	mailing obscene material	advertisements for two sexually explicit books
465 F.2d 282	Tallman v. United States	7	1972	0	uttering obscene language on the radio	language is not described
465 F.2d 1096	United States v. Young	9	1972	0	mailing obscene material	obscene advertisements
455 F.2d 899	United States v. Miller	9	1972	0	mailing obscene material	obscene advertisements
459 F.2d 282	Southeastern Promotions, Ltd. v. Oklahoma City	10	1972	1	Oklahoma City's refusal to lease its auditorium	the musical "Hair"
454 F.2d 280	United States v. Fesenmeyer	9	1972	0	transporting in interstate commerce obscene material	unclear
467 F.2d 1126	United States v. Smith	7	1972	1	uttering obscene language on the radio	used profane language on a radio broadcast
481 F.2d 605	United States v. Gates	5	1973	0	prohibition on mailing obscene materials	a letter which included sexually explicit language
486 F.2d 894	Southeastern Promotions, Ltd. v. Conrad	6	1973	0	TN obscenity common law and statutes	a performance of the play "Hair"
475 F.2d 65	United States v. Palladino	1	1973	1	prohibition on mailing obscene material	books and brochures depicting and describing porn and sex
473 F.2d 1297	Cinecom Theaters Midwest States, Inc. v. Ft. Wayne	7	1973	1	Fort Wayne, IN city ordinance prohibiting nudity in drive-in movies	films involving nudity
481 F.2d 307	United States v. Hamling	9	1973	0	mailing obscene material	obscene advertisements and books
481 F.2d 206	United States v. One Reel of Film	1	1973	0	prohibition on importation of obscene material	pornographic film
487 F.2d 331	United States v. Millican	5	1973	0	prohibition on mailing obscene materials	pornographic film and magazine advertising the film
485 F.2d 574	United States v. Cote	5	1973	0	prohibition on mailing obscene materials	pornographic films, magazines, and advertisements for those films and magazines
484 F.2d 1149	United States v. Thevis	5	1973	1	transporting obscene material on a common carrier in interstate commer	pornographic magazines
494 F.2d 499	United States v. Groner	5	1974	0	transporting obscene material on a common carrier in interstate commer	"obscene books"
502 F.2d 973	Brubaker v. Board of Education	7	1974	1	dismissal of teachers for distributing obscene material to minors	a brochure describing Woodstock and its sexual excess
487 F.2d 1300	Patterson v. United States	5	1974	0	prohibition on mailing obscene materials	a letter containing pornographic photographs
502 F.2d 1300	United States v. Ratner	5	1974	0	federal obscenity statute	advertisements for pornographic materials
490 F.2d 499	United States v. Palladino	1	1974	1	prohibition on mailing obscene material	book and brochure which depicted/described pornographic photos
507 F.2d 294	United States v. Harding	10	1974	0	receipt of obscene matter transported through interstate commerce	obscene books and films
490 F.2d 78	United States v. Sulaiman	5	1974	0	federal obscenity statute	pornographic ads and films
505 F.2d 1247	United States v. Miller	9	1974	0	mailing obscene material	pornographic books and magazines
507 F.2d 1100	Miller v. United States	9	1974	0	mailing obscene material	pornographic books and magazines
491 F.2d 956	, Sharpie, Inc.	2	1974	0	importation of obscene material	pornographic film
502 F.2d 391	United States v. Pryba	12	1974	0	interstate transportation of obscene materials	pornographic film

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

Citation	Case Name	Circuit	Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
803 F.2d 174	United States v. Marchant	5	1986	0	knowingly receiving child pornography	pornographic magazines featuring children
791 F.2d 463	Paducah v. Investment Entertainment, Inc.	6	1986	1	Paducah, KY obscenity ordinance	pornographic movie theaters, adult bookstores, etc.
826 F.2d 708	Moses v. County of Kenosha	7	1987	0	Kenosha County, WI obscenity ordinance	adult bookstores
819 F.2d 451	United States v. Guglielmi	4	1987	0	prohibition on mailing obscene material; use of common carrier to transp	films depicting bestiality
816 F.2d 1326	Polykoff v. Collins	9	1987	0	AZ obscenity statute	materials sold at adult bookstores
848 F.2d 923	United States v. Zangger	8	1988	1	mailing obscene material	a pornographic videotape
868 F.2d 1043	Ripplinger v. Collins	9	1989	1	AZ obscenity statute	"mainstream" pornographic materials
867 F.2d 1188	Dworkin v. Hustler Magazine, Inc. v. King County	9	1989	1	none-Andrea Dworkin sued Hustler for libel, invasion of privacy, among o	sexually explicit illustrations and photographs
911 F.2d 80	Walker v. Kansas City	8	1990	0	Kansas City zoning ordinance	exotic dancing at a bar
900 F.2d 748	United States v. Pryba	4	1990	0	RICO and state obscenity law	pornographic books and videos
902 F.2d 513	Kucharek v. Hanaway	7	1990	0	WI obscenity law	pornographic films, magazines, photographs, etc.
901 F.2d 630	Sequoia Books, Inc. v. Ingemunson	7	1990	0	IL obscenity statute	sexually explicit magazines, books, etc., sold by adult bookstore (plaintiff)
943 F.2d 825	Alexander v. Thornburgh	8	1991	0	RICO with obscenity violations as predicate offenses	pornographic videos and magazines
927 F.2d 1442	United States v. Easley	8	1991	0	mailing obscene material	sexually explicit videotapes and magazines
952 F.2d 155	United States v. ABC, Inc.	8	1991	0	transportation of obscene materials in interstate commerce using a comn	unclear
960 F.2d 134	Luke Records v. Navarro	11	1992	1	Florida county sheriff claiming the song is obscene	rap song by 2 Live Crew
10 F.3d 263	United States v. Investment Enterprises, Inc.	5	1993	0	interstate transportation of obscene materials	sexually explicit box covers and video tapes
25 F.3d 1314	United States v. Skinner	6	1994	0	engaged in business of selling or transferring obscene matter	adult bookstores
18 F.3d 1181	Eckstein v. Melson	4	1994	0	federal obscenity statute	pornographic books/magazines
31 F.3d 135	United States v. Schein	3	1994	0	prohibition on mailing obscene material	sexually explicit film
74 F.3d 701	United States v. Thomas	6	1996	0	federal obscenity laws	an electronic bulletin board on which Thomas sold sexually explicit photos
230 F.3d 649	United States v. Various Articles of Merchandise, Schedule 287	3	2000	1	importation of obscene material	nudist magazines from France and Germany
237 F.3d 251	United States v. Loy	3	2001	1	receiving and possessing child pornography; after conviction, Loy was pre	convicted for sexually explicit films of children; prevented from viewing any pornographic
248 F.3d 394	United States v. Fox	5	2001	0	receipt of child pornography through the internet	images depicting child pornography
251 F.3d 1072	United States v. Landham	6	2001	1	making obscene interstate phone calls	Landham made obscene phone calls to his wife solely to harrass her
377 F.3d 49	United States v. Gravenhorst	1	2004	0	use of the internet to solicit minors	explicit photographs and language used in emails to minors
426 F.3d 765	United States v. Ragsdale	5	2005	0	mailing obscene materials	violent porn
459 F.3d 80	United States v. Fabrizio	1	2006	0	child porn statute	depictions of "lascivious conduct"
466 F.3d 938	United States v. Eckhardt	11	2006	0	prohibition on making harrassing phone calls	obscene phone calls
444 F.3d 1286	United States v. Williams	11	2006	1	statute banning promotion of child porn	promoting (obscene) child porn
470 F.3d 1074	Giovani Carandola, Ltd. v. Fox	4	2006	0	NC statute regulating erotic dancing	simulated sexual acts--something defined by *Miller as obscene and therefore regulable*
469 F.3d 641	Entm't Software Ass'n v. Blagojevich	7	2006	1	statute regulating video games	violent/sexually explicit video games
550 F.3d 326	United States v. Whorley	4	2008	0	child porn statute	child porn which also qualified as "obscene" under Miller
546 F.3d 965	United States v. Schales	9	2008	0	child porn statute	child porn which also qualified as "obscene" under *Miller*
517 F.3d 738	Reliable Consultants, Inc. v. Earle	5	2008	1	TX ban on sale of sexual devices	private intimate conduct

Appendix Table II.— The Effects of Free Speech Precedents on Attitudes

Dependent Variable	Extramarital Sex is OK						Premarital Sex is OK						Homosexual Sex is OK					
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE	(11)	(12)	(13)	(14)	(15)	Wild BS %LE
Proportion Progressive Free Speech	-0.000817	0.00247	-0.272	-0.000585	0.0188	0.69	-0.0421*	-0.387	-0.0186	-0.0294	-0.0284	0.73	-0.00374	0.0854	-0.304	-0.0243	-0.0224	0.90
Appellate Decisions$_{t+1}$	(0.00995)	(0.0606)	(0.486)	(0.0142)	(0.0187)		(0.0182)	(0.284)	(0.292)	(0.0256)	(0.0278)		(0.0152)	(0.0708)	(0.887)	(0.0329)	(0.0341)	
Proportion Progressive Free Speech	-0.0192	-0.0136	-0.0501	-0.0179	-0.0310+	0.09	0.0611	0.0856	0.00340	0.0644+	0.0614+	0.45	-0.0113	-0.0314	-0.232	0.0125	0.0137	0.80
Appellate Decisions$_{t}$	(0.0147)	(0.0812)	(0.410)	(0.0161)	(0.0159)		(0.0358)	(0.413)	(0.926)	(0.0347)	(0.0365)		(0.0358)	(0.140)	(0.510)	(0.0411)	(0.0447)	
Proportion Progressive Free Speech	0.00770	-0.0547	0.259	0.0183	0.0389+	0.18	-0.0613+	-0.0947	-0.224	-0.0644+	-0.0627+	0.39	-0.0133	-0.0624	-0.165	-0.0410	-0.0369	0.64
Appellate Decisions$_{t-1}$	(0.0111)	(0.0741)	(0.670)	(0.0193)	(0.0233)		(0.0286)	(0.515)	(0.785)	(0.0351)	(0.0356)		(0.0242)	(0.144)	(0.958)	(0.0461)	(0.0603)	
Proportion Progressive Free Speech	-0.00296	0.0484	0.0430	0.0209	0.0197	0.60	0.00118	-0.243	0.119	0.0190	0.0299	0.69	0.0219	0.126	-0.214	0.0772**	0.0904**	0.05
Appellate Decisions$_{t-2}$	(0.0120)	(0.138)	(0.570)	(0.0198)	(0.0232)		(0.0281)	(0.335)	(0.515)	(0.0309)	(0.0328)		(0.0241)	(0.238)	(0.909)	(0.0213)	(0.0190)	
Proportion Progressive Free Speech	0.0256+	-0.0303	-0.287	0.0175	0.00465	0.81	-0.00424	-0.0823	0.259	0.0260	0.0278	0.85	-0.0105	-0.114+	0.454	-0.0361	-0.0364	0.48
Appellate Decisions$_{t-3}$	(0.0137)	(0.0393)	(1.447)	(0.0150)	(0.0289)		(0.0198)	(0.497)	(3.728)	(0.0282)	(0.0302)		(0.0306)	(0.0660)	(2.168)	(0.0317)	(0.0426)	
Proportion Progressive Free Speech	0.0142	0.0534*	-0.102	0.00224	0.00661	0.94	0.00468	0.0491	0.0792	0.0284+	0.0153	0.94	0.0182	0.165+	-0.0601	-0.00737	-0.0151	0.97
Appellate Decisions$_{t-4}$	(0.0109)	(0.0254)	(0.161)	(0.0179)	(0.0194)		(0.0180)	(0.263)	(2.421)	(0.0158)	(0.0164)		(0.0147)	(0.0859)	(0.462)	(0.0304)	(0.0252)	
N	18874	18874	18874	18874	18874		18801	18801	18801	18801	18801		18073	18073	18073	18073	18073	
R-sq	0.014	0.012	.	0.013	0.013		0.028	0.014	0.015	0.028	0.028		0.057	0.052	.	0.057	0.056	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level	Individual						Individual						Individual					
Mean dependent variable	0.097	0.097	0.097	0.097	0.097		0.633	0.633	0.633	0.633	0.633		0.267	0.267	0.267	0.267	0.267	
Average Law$_{ct}$ effect	**0.005**	**0.001**	**-0.027**	**0.008**	**0.008**		**0.000**	**-0.057**	**0.047**	**0.015**	**0.014**		**0.001**	**0.017**	**-0.043**	**0.001**	**0.003**	
P-value of Law$_{ct}$ lags	**0.002**	**0.001**	**0.639**	**0.135**	**0.001**		**0.126**	**0.666**	**0.815**	**0.001**	**0.000**		**0.805**	**0.000**	**0.574**	**0.000**	**0.000**	
P-value of Law_{ct} leads	0.936	0.968	0.576	0.967	0.315		0.041	0.174	0.949	0.251	0.307		0.810	0.228	0.732	0.460	0.510	
Average 1[M_{ct}>0] lag	0.001	0.002	-0.003	0.000	-0.001		0.005	0.036	0.007	0.002	0.001		0.006	-0.002	0.060	0.006	0.006	
P-value of 1[M_{ct}>0] lags	0.379	0.270	0.738	0.346	0.814		0.001	0.091	0.983	0.000	0.000		0.053	0.585	0.760	0.221	0.000	
P of Law_{ct}+1[M_{ct}>0] lags	0.001	0.000	0.866	0.000	0.000		0.894	0.871	0.914	0.000	0.012		0.000	0.000	0.539	0.000	0.000	
Typical Law_{ct} effect	0.000	0.000	-0.001	0.000	0.000		0.000	-0.003	0.002	0.001	0.001		0.000	0.001	-0.002	0.000	0.000	
Unconditional effect - progressive	0.000	0.000	-0.002	0.000	0.000		0.000	-0.001	0.003	0.001	0.001		0.000	0.001	0.001	0.000	0.001	
Unconditional effect - conser	0.000	0.000	-0.001	0.000	-0.000		0.001	0.007	0.001	0.000	0.000		0.001	-0.000	0.011	0.001	0.001	
Unconditional effect - all	0.001	0.000	-0.002	0.000	0.000		0.001	0.006	0.004	0.001	0.001		0.001	0.000	0.011	0.001	0.002	
P of 1[M_{ct}>0] leads	0.063	0.466	0.514	0.018	0.041		0.371	0.383	0.999	0.631	0.581		0.122	0.971	0.592	0.203	0.154	
P of Law_{ct}+1[M_{ct}>0] leads	0.178	0.623	0.650	0.329	0.075		0.106	0.176	0.990	0.292	0.371		0.376	0.108	0.831	0.721	0.850	

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

APPENDIX TABLE III

Impact of Free Speech Precedent on Sexual Attitudes
Robustness of 2SLS Estimates

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

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

APPENDIX TABLE IV

Impact of Free Speech Precedent on Sexual Attitudes
Robustness of 2SLS Distributed Lag Estimates

The Effect of Appellate Free Speech Precedent on Extramarital Sex is OK						
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
No Trends	-0.004	-0.020	-0.010	0.020	0.009	
	(0.020)	(0.021)	(0.014)	(0.016)	(0.019)	
No FE	-0.002	-0.011	-0.015	0.032	0.001	
	(0.033)	(0.024)	(0.029)	(0.019)	(0.021)	
State Cluster	-0.001	-0.018	0.018	0.021	0.018	
	(0.018)	(0.019)	(0.022)	(0.028)	(0.016)	
No Individual-Level Controls	0.002	-0.017	0.003	0.019	0.018	
	(0.020)	(0.023)	(0.019)	(0.022)	(0.015)	
No Survey Weights	-0.002	-0.017	0.002	0.010	-0.005	
	(0.018)	(0.019)	(0.019)	(0.021)	(0.017)	
No Community Standards	0.019	-0.031 *	0.038	0.020	0.005	
	(0.019)	(0.015)	(0.024)	(0.025)	(0.028)	
No Controls except 1[M_{ct}>0]	0.012	-0.020	0.036	0.034	-0.004	
	(0.042)	(0.014)	(0.047)	(0.026)	(0.043)	
Drop Circuit 1	-0.002	-0.019	0.022	0.019	0.017	
	(0.014)	(0.015)	(0.019)	(0.020)	(0.015)	
Drop Circuit 2	-0.005	-0.002	0.031	0.018	0.022	
	(0.015)	(0.014)	(0.024)	(0.021)	(0.014)	
Drop Circuit 3	0.001	-0.033 *	0.022	0.005	0.015	
	(0.014)	(0.015)	(0.016)	(0.021)	(0.012)	
Drop Circuit 4	0.001	-0.008	-0.006	0.023	0.019	
	(0.015)	(0.015)	(0.028)	(0.017)	(0.017)	
Drop Circuit 5	-0.005	-0.011	0.001	0.030	0.015	
	(0.015)	(0.020)	(0.024)	(0.020)	(0.015)	
Drop Circuit 6	-0.001	0.004	0.037	-0.012	0.029 *	
	(0.020)	(0.016)	(0.023)	(0.021)	(0.015)	
Drop Circuit 7	-0.006	-0.019	0.026	0.022	0.026	
	(0.012)	(0.023)	(0.025)	(0.020)	(0.023)	
Drop Circuit 8	-0.005	-0.017	0.013	0.015	0.015	
	(0.011)	(0.013)	(0.021)	(0.021)	(0.022)	
Drop Circuit 9	0.025 +	-0.035 **	-0.004	0.031 +	0.022	
	(0.013)	(0.012)	(0.019)	(0.025)	(0.019)	
Drop Circuit 10	-0.004	-0.015	0.034 *	0.011	0.027 +	
	(0.015)	(0.014)	(0.014)	(0.023)	(0.016)	
Drop Circuit 11	-0.000	-0.020	0.021	0.005	0.014	
	(0.016)	(0.019)	(0.016)	(0.013)	(0.016)	
Drop Circuit 12	0.003	-0.018	0.019	0.022	0.011	
	(0.012)	(0.017)	(0.020)	(0.019)	(0.013)	
1 current 1 lag	-0.021	0.007				
	(0.019)	(0.039)				
1 current 2 lag	-0.022	0.028	0.013			
	(0.018)	(0.035)	(0.023)			
2 leads 4 lags	-0.004	-0.015	0.037 +	0.005	0.009	
	(0.017)	(0.018)	(0.020)	(0.019)	(0.016)	
1 lead 5 lags	-0.008	-0.012	0.023	0.009	0.005	0.016
	(0.015)	(0.016)	(0.018)	(0.019)	(0.013)	(0.011)
4 leads 1 lag	-0.006	0.013	-0.006	0.001	-0.034	0.040 +
(t0, t1, f4, f3, f2, f1)	(0.027)	(0.020)	(0.016)	(0.020)	(0.021)	(0.023)

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

Appendix Table V.— The Effects of Free Speech Precedents on Sexual Behaviors

Dependent Variable	Paid Sex						Number of Partners per Year						Number of Female Partners					
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE	(11)	(12)	(13)	(14)	(15)	Wild BS %LE
Proportion Progressive Free Speech	-0.00176	0.00381	-0.00422	-0.00521*	-0.00279	0.32	0.135	-0.537	0.439*	0.0509	0.105	0.60	-0.281	-1.281	-1.319	-3.370	-2.207	0.34
Appellate Decisions$_{t+1}$	(0.00216)	(0.0142)	(0.00364)	(0.00254)	(0.00249)		(0.126)	(1.020)	(0.179)	(0.201)	(0.148)		(1.827)	(4.829)	(3.749)	(2.265)	(2.347)	
Proportion Progressive Free Speech	0.00600	-0.00621	0.0123*	0.00767+	0.00627+	0.14	-0.300	2.025	-0.159	-0.130	-0.240	0.57	3.111	1.702	-0.657	5.432*	5.176*	0.01
Appellate Decisions$_{t}$	(0.00360)	(0.0360)	(0.00525)	(0.00414)	(0.00341)		(0.241)	(2.608)	(0.280)	(0.245)	(0.220)		(1.805)	(24.70)	(7.551)	(2.461)	(2.130)	
Proportion Progressive Free Speech	-0.000137	-0.00135	0.00212	0.00266	0.00115	0.72	0.753+	1.291*	0.994*	1.177**	0.861*	0.40	3.829*	-0.0335	8.222**	6.648**	7.772**	0.04
Appellate Decisions$_{t-1}$	(0.00344)	(0.0104)	(0.00511)	(0.00493)	(0.00355)		(0.405)	(0.615)	(0.423)	(0.434)	(0.400)		(1.280)	(8.566)	(2.253)	(2.178)	(1.668)	
Proportion Progressive Free Speech	0.00632**	0.0218	0.00731+	0.00631*	0.00713**	0.15	0.0420	-0.558	0.421+	0.0901	0.0954	0.93	3.262+	2.834	9.065+	4.172+	4.958*	0.10
Appellate Decisions$_{t-2}$	(0.00201)	(0.0155)	(0.00431)	(0.00272)	(0.00203)		(0.192)	(1.068)	(0.220)	(0.187)	(0.171)		(1.526)	(3.765)	(5.381)	(2.402)	(1.985)	
Proportion Progressive Free Speech	0.00499*	0.0205	0.00525	0.00531+	0.00584**	0.08	-0.198	-0.767	-0.394	0.0709	-0.131	0.79	0.780	3.416	8.824*	2.078	4.019+	0.19
Appellate Decisions$_{t-3}$	(0.00223)	(0.0207)	(0.00362)	(0.00319)	(0.00222)		(0.196)	(1.223)	(0.375)	(0.145)	(0.166)		(0.927)	(6.657)	(4.291)	(1.743)	(2.132)	
Proportion Progressive Free Speech	-0.000925	-0.00612	0.00109	0.000490	0.000132	0.88	0.0313	0.593	0.104	0.108	0.0724	0.55	1.268	-1.661	1.004	2.284+	3.217**	0.03
Appellate Decisions$_{t-4}$	(0.00208)	(0.00676)	(0.00206)	(0.00280)	(0.00261)		(0.111)	(0.800)	(0.159)	(0.123)	(0.107)		(0.938)	(9.933)	(2.663)	(1.262)	(1.133)	
N	16659	16659	16659	16659	16659		15346	15346	15346	15346	15346		13833	13833	13833	13833	13833	
R-sq	0.002	.	0.002	0.002	0.002		0.010	.	0.009	0.009	0.010		0.005	0.004	0.003	0.004	0.004	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level	Individual						Individual						Individual					
Mean dependent variable	0.003	0.003	0.003	0.003	0.003		1.129	1.129	1.129	1.129	1.129		6.296	6.296	6.296	6.296	6.296	
Average Law$_{ct}$ effect	**0.003**	**0.006**	**0.006**	**0.004**	**0.004**		**0.066**	**0.517**	**0.193**	**0.263**	**0.132**		**2.450**	**1.252**	**5.292**	**4.123**	**5.028**	
P-value of Law$_{ct}$ lags	**0.022**	**0.075**	**0.100**	**0.101**	**0.001**		**0.348**	**0.001**	**0.000**	**0.061**	**0.181**		**0.095**	**0.961**	**0.000**	**0.003**	**0.000**	
P-value of Law_{ct} leads	0.434	0.789	0.247	0.040	0.263		0.306	0.598	0.014	0.800	0.477		0.881	0.791	0.725	0.137	0.347	
Average 1[M_{ct}>0] lag	-0.001	-0.002	-0.002	-0.002	-0.001		0.088	-0.012	0.077	0.019	0.069		-0.705	-0.317	-2.419	-1.319	-1.645	
P-value of 1[M_{ct}>0] lags	0.129	0.043	0.232	0.062	0.072		0.562	0.110	0.085	0.005	0.351		0.028	0.279	0.001	0.000	0.000	
P of Law_{ct}+1[M_{ct}>0] lags	0.067	0.478	0.074	0.159	0.008		0.285	0.000	0.000	0.025	0.152		0.142	0.894	0.002	0.037	0.000	
Typical Law_{ct} effect	0.000	0.000	0.000	0.000	0.000		0.002	0.013	0.005	0.007	0.003		0.058	0.030	0.126	0.098	0.120	
Unconditional effect - progressive	0.000	0.000	0.000	0.000	0.000		0.004	0.013	0.007	0.007	0.005		0.042	0.022	0.068	0.067	0.080	
Unconditional effect - conser	-0.000	-0.000	-0.000	-0.000	-0.000		0.010	-0.001	0.009	0.002	0.008		-0.079	-0.035	-0.269	-0.147	-0.183	
Unconditional effect - all	-0.000	-0.000	-0.000	-0.000	-0.000		0.014	0.011	0.015	0.009	0.013		-0.037	-0.013	-0.201	-0.080	-0.103	
P of 1[M_{ct}>0] leads	0.270	0.409	0.252	0.603	0.238		0.239	0.675	0.293	0.349	0.267		0.235	0.901	0.049	0.496	0.299	
P of Law_{ct}+1[M_{ct}>0] leads	0.085	0.914	0.126	0.011	0.058		0.171	0.639	0.094	0.443	0.209		0.364	0.789	0.293	0.065	0.094	

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

Appendix Table VI.— The Effects of Free Speech Precedents on Sexual Behaviors

Dependent Variable	Number of Partners per Year (reported by Men)						Number of Female Partners (reported by Men)						Extramarital Sex (reported by Men)					
						Wild BS						Wild BS						Wild BS
	(1)	(2)	(3)	(4)	(5)	%LE	(6)	(7)	(8)	(9)	(10)	%LE	(11)	(12)	(13)	(14)	(15)	%LE
Proportion Progressive Free Speech	0.160	-2.660	0.749*	-0.0470	0.0501	0.61	1.466	-7.887	-5.880	-5.195	-2.703	0.32	-0.0142	-0.0747	0.00240	-0.0553	-0.0298	0.65
Appellate Decisions$_{t+1}$	(0.356)	(1.862)	(0.368)	(0.469)	(0.376)		(3.835)	(8.287)	(9.012)	(4.170)	(4.139)		(0.0290)	(0.296)	(0.0563)	(0.0423)	(0.0383)	
Proportion Progressive Free Speech	-0.810	3.451	-0.787+	-0.423	-0.673	0.44	5.722	16.09	3.321	11.27*	10.49*	0.03	0.0705	0.500	0.0251	0.102+	0.0927	0.41
Appellate Decisions$_{t}$	(0.561)	(3.125)	(0.442)	(0.589)	(0.535)		(3.374)	(13.45)	(15.71)	(4.980)	(4.136)		(0.0584)	(1.262)	(0.0770)	(0.0589)	(0.0584)	
Proportion Progressive Free Speech	1.858+	2.653	2.266*	2.767**	2.080*	0.33	8.739**	6.962	19.05**	15.42**	16.89**	0.03	0.107*	0.279	0.0872	0.133*	0.122*	0.03
Appellate Decisions$_{t-1}$	(0.904)	(2.246)	(0.934)	(0.991)	(0.909)		(2.669)	(7.593)	(4.855)	(3.767)	(3.390)		(0.0448)	(0.519)	(0.0710)	(0.0517)	(0.0493)	
Proportion Progressive Free Speech	0.0799	0.0437	0.205	0.103	0.185	0.49	10.04**	9.426	18.69+	12.65**	13.62**	0.05	0.0583+	-0.0482	0.110**	0.0826*	0.0774*	0.03
Appellate Decisions$_{t-2}$	(0.349)	(1.627)	(0.467)	(0.315)	(0.321)		(2.280)	(8.386)	(10.42)	(4.910)	(3.846)		(0.0308)	(0.368)	(0.0424)	(0.0370)	(0.0341)	
Proportion Progressive Free Speech	-0.647	-0.307	-1.054	-0.00362	-0.510	0.72	1.633	4.608	17.85*	5.162+	8.658+	0.24	0.0572	-0.100	0.0600	0.0691	0.0667	0.12
Appellate Decisions$_{t-3}$	(0.491)	(1.872)	(0.773)	(0.363)	(0.441)		(1.944)	(5.878)	(8.611)	(2.958)	(4.676)		(0.0434)	(0.354)	(0.0534)	(0.0478)	(0.0501)	
Proportion Progressive Free Speech	0.188	1.425	0.336	0.468	0.306	0.71	2.519	-0.257	4.862	5.619*	7.055**	0.03	-0.0131	-0.0632	-0.0434	-0.00149	-0.0132	0.69
Appellate Decisions$_{t-4}$	(0.298)	(2.206)	(0.304)	(0.328)	(0.275)		(1.886)	(9.863)	(7.326)	(2.416)	(2.031)		(0.0267)	(0.788)	(0.0265)	(0.0328)	(0.0285)	
N	6626	6626	6626	6626	6626		6077	6077	6077	6077	6077		7170	7170	7170	7170	7170	
R-sq	0.023	0.006	0.022	0.022	0.023		0.010	0.008	0.006	0.009	0.009		0.010	.	0.010	0.010	0.010	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level	Individual						Individual						Individual					
Mean dependent variable	1.421	1.421	1.421	1.421	1.421		14.041	14.041	14.041	14.041	14.041		0.161	0.161	0.161	0.161	0.161	
Average Law$_{ct}$ effect	**0.134**	**1.453**	**0.193**	**0.582**	**0.278**		**5.730**	**7.366**	**12.756**	**10.025**	**11.342**		**0.056**	**0.113**	**0.048**	**0.077**	**0.069**	
P-value of Law$_{ct}$ lags	**0.095**	**0.581**	**0.000**	**0.016**	**0.017**		**0.001**	**0.049**	**0.000**	**0.000**	**0.000**		**0.014**	**0.968**	**0.000**	**0.003**	**0.003**	
P-value of Law_{ct} leads	0.662	0.153	0.042	0.920	0.894		0.709	0.341	0.514	0.213	0.514		0.635	0.801	0.966	0.192	0.437	
Average 1[M_{ct}>0] lag	0.237	-0.154	0.231	0.073	0.185		-1.190	-1.596	-5.435	-2.643	-3.167		-0.023	-0.027	-0.021	-0.030	-0.027	
P-value of 1[M_{ct}>0] lags	0.241	0.465	0.090	0.004	0.055		0.008	0.053	0.000	0.000	0.000		0.029	0.919	0.009	0.000	0.000	
P of Law_{ct}+1[M_{ct}>0] lags	0.008	0.003	0.000	0.001	0.000		0.005	0.149	0.000	0.010	0.000		0.000	0.989	0.000	0.000	0.000	
Typical Law_{ct} effect	0.003	0.036	0.005	0.015	0.007		0.140	0.179	0.311	0.244	0.276		0.001	0.003	0.001	0.002	0.002	
Unconditional effect - progressive	0.009	0.033	0.011	0.016	0.012		0.111	0.141	0.178	0.180	0.199		0.001	0.002	0.001	0.001	0.001	
Unconditional effect - conser	0.027	-0.018	0.027	0.008	0.021		-0.134	-0.179	-0.611	-0.297	-0.356		-0.003	-0.003	-0.002	-0.003	-0.003	
Unconditional effect - all	0.037	0.015	0.037	0.025	0.033		-0.023	-0.039	-0.433	-0.117	-0.157		-0.002	-0.001	-0.002	-0.002	-0.002	
P of 1[M_{ct}>0] leads	0.337	0.259	0.816	0.349	0.336		0.145	0.726	0.147	0.340	0.180		0.008	0.892	0.003	0.013	0.001	
P of Law_{ct}+1[M_{ct}>0] leads	0.357	0.207	0.135	0.716	0.490		0.604	0.281	0.269	0.109	0.161		0.077	0.809	0.225	0.005	0.034	

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

Appendix Table VII.— The Effects of Free Speech Precedents on Sexual Behaviors

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

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

APPENDIX TABLE VIII

Impact of Free Speech Precedent on Sexual Behaviors
Robustness of 2SLS Estimates

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

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

APPENDIX TABLE IX

Impact of Free Speech Precedent on Sexual Behaviors
Robustness of 2SLS Distributed Lag Estimates

	The Effect of Appellate Free Speech Precedent on Paid Sex											
	(t0)		(t1)		(t2)		(t3)		(t4)		(t5)	
No Trends	-0.002		0.002		0.001		0.001		0.003			
	(0.003)		(0.002)		(0.004)		(0.003)		(0.002)			
No FE	-0.000		-0.001		-0.002		0.001		0.003			
	(0.002)		(0.001)		(0.004)		(0.002)		(0.003)			
State Cluster	-0.005		0.008	+	0.003		0.006		0.005			
	(0.004)		(0.004)		(0.005)		(0.003)		(0.005)			
No Individual-Level Controls	-0.006		0.007		0.004		0.005		0.004	+		
	(0.004)		(0.004)		(0.005)		(0.003)		(0.002)			
No Survey Weights	-0.006	*	0.008	*	0.007		0.007	*	0.012	**		
	(0.003)		(0.004)		(0.005)		(0.003)		(0.003)			
No Community Standards	-0.003		0.007	+	0.002		0.007		0.006	**		
	(0.003)		(0.004)		(0.004)		(0.002)		(0.002)			
No Controls except 1[M_{ct}>0]	-0.000		0.003	*	-0.003		0.002		0.001			
	(0.002)		(0.001)		(0.004)		(0.002)		(0.002)			
Drop Circuit 1	-0.005	*	0.008	+	0.003		0.007	*	0.006	+		
	(0.003)		(0.004)		(0.005)		(0.003)		(0.003)			
Drop Circuit 2	-0.006	**	0.008	+	0.004		0.006	**	0.005			
	(0.002)		(0.005)		(0.005)		(0.003)		(0.003)			
Drop Circuit 3	-0.004		0.013	**	0.006	+	0.007		0.007	**		
	(0.002)		(0.005)		(0.003)		(0.003)		(0.002)			
Drop Circuit 4	-0.001		0.003	+	-0.001		0.005		0.004	*		
	(0.002)		(0.002)		(0.003)		(0.002)		(0.002)			
Drop Circuit 5	-0.004		0.007	*	-0.004		0.007		0.001			
	(0.004)		(0.003)		(0.004)		(0.004)		(0.004)			
Drop Circuit 6	-0.006		0.010	+	0.004		0.007		0.010	**		
	(0.006)		(0.006)		(0.004)		(0.003)		(0.002)			
Drop Circuit 7	-0.005	+	0.003		0.002		0.005	+	0.005	+		
	(0.003)		(0.004)		(0.004)		(0.002)		(0.003)			
Drop Circuit 8	-0.007	*	0.011	*	0.008	*	0.006	*	0.006			
	(0.003)		(0.005)		(0.004)		(0.003)		(0.004)			
Drop Circuit 9	-0.002		0.003		0.001		0.008		0.006	**		
	(0.002)		(0.003)		(0.004)		(0.003)		(0.002)			
Drop Circuit 10	-0.003		0.007		0.003		0.007		0.007	*		
	(0.002)		(0.004)		(0.005)		(0.003)		(0.003)			
Drop Circuit 11	-0.002		0.004		-0.003		0.005		0.002			
	(0.003)		(0.004)		(0.005)		(0.002)		(0.003)			
Drop Circuit 12	-0.005	+	0.008	+	0.003		0.007	+	0.006	+		
	(0.002)		(0.004)		(0.005)		(0.003)		(0.003)			
1 current 1 lag	0.004		0.000									
	(0.004)		(0.004)									
1 current 2 lag	0.003		-0.001		-0.003							
	(0.003)		(0.004)		(0.003)							
2 leads 4 lags	-0.003		0.009	*	0.002		0.007		0.005	+		
	(0.003)		(0.004)		(0.005)		(0.003)		(0.003)			
1 lead 5 lags	-0.003		0.005		-0.001		0.003		0.003		-0.002	
	(0.002)		(0.003)		(0.004)		(0.003)		(0.003)		(0.003)	
4 leads 1 lag	0.004		0.003		0.002		0.003		0.004		-0.001	
(t0, t1, f4, f3, f2, f1)	(0.004)		(0.003)		(0.003)		(0.006)		(0.003)		(0.004)	

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

Appendix Table X.— The Effects of Free Speech Precedents on Crimes

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

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

Appendix Table XI.— The Effects of Free Speech Precedents on Crimes

Dependent Variable	Drug Violations per 100,000						Forcible Rapes per 100,000					
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE
Proportion Progressive Free Speech	12.59	254.6	-74.52	144.0	105.7	0.94	2.231+	6.604	-0.923	14.60	0.838	0.99
Appellate Decisions$_{t+1}$	(22.86)	(176.1)	(156.3)	(98.99)	(332.2)		(1.220)	(4.628)	(4.384)	(13.03)	(5.805)	
Proportion Progressive Free Speech	58.97	126.0+	272.4+	141.7**	62.82	0.77	-0.648	4.394	8.918	11.18	9.335	0.02
Appellate Decisions$_{t}$	(41.18)	(68.06)	(144.5)	(48.43)	(221.5)		(0.867)	(3.218)	(8.373)	(15.11)	(7.986)	
Proportion Progressive Free Speech	10.92	37.83	-19.57	56.69	294.1	0.41	-0.105	4.935	3.665	11.92	2.979	0.58
Appellate Decisions$_{t-1}$	(39.35)	(31.15)	(212.1)	(36.78)	(397.7)		(2.245)	(5.333)	(10.14)	(8.537)	(11.75)	
Proportion Progressive Free Speech	3.219	10.45	-10.53	-4.894	-69.43	0.44	-0.273	4.122	2.749	12.37	2.301	0.53
Appellate Decisions$_{t-2}$	(22.50)	(50.28)	(197.1)	(43.31)	(201.2)		(0.948)	(4.242)	(5.573)	(10.11)	(6.752)	
Proportion Progressive Free Speech	30.58	67.50	36.36	65.38	127.1	0.56	0.469	8.496	-4.052	7.324	-4.044	0.36
Appellate Decisions$_{t-3}$	(24.21)	(49.49)	(86.60)	(41.53)	(183.3)		(1.084)	(5.570)	(6.101)	(9.786)	(5.153)	
Proportion Progressive Free Speech	51.09	105.2*	174.4*	115.8*	-236.9	0.26	-1.510	1.123	1.764	4.129	0.380	0.91
Appellate Decisions$_{t-4}$	(36.39)	(47.47)	(81.75)	(52.21)	(376.2)		(1.577)	(4.068)	(4.745)	(8.157)	(3.639)	
N	43992	43992	43992	43992	43992		67017	67017	67017	67017	67017	
R-sq	0.335	0.323	0.322	0.329	0.302		0.077	0.051	0.039	.	0.043	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level	ORI Agency - Year						ORI Agency - Year					
Mean dependent variable	286.987	286.987	286.987	286.987	286.987		10.044	10.044	10.044	10.044	10.044	
Average Law$_{ct}$ effect	**30.956**	**69.391**	**90.613**	**74.925**	**35.542**		**-0.413**	**4.614**	**2.609**	**9.385**	**2.190**	
P-value of Law$_{ct}$ lags	**0.038**	**0.002**	**0.000**	**0.000**	**0.002**		**0.367**	**0.268**	**0.103**	**0.000**	**0.268**	
P-value of Law_{ct} leads	0.594	0.148	0.633	0.146	0.750		0.097	0.154	0.833	0.262	0.885	
Average 1[M_{ct}>0] lag	-20.745	-42.342	-61.412	-42.898	-44.445		0.035	-1.643	-0.985	-3.534	-1.001	
P-value of 1[M_{ct}>0] lags	0.001	0.000	0.000	0.003	0.038		0.200	0.044	0.252	0.515	0.425	
P of Law_{ct}+1[M_{ct}>0] lags	0.016	0.256	0.005	0.001	0.269		0.536	0.309	0.004	0.000	0.008	
Typical Law_{ct} effect	1.450	3.251	4.245	3.510	1.665		-0.027	0.301	0.170	0.612	0.143	
Unconditional effect - progressive	0.511	1.355	1.462	1.604	-0.446		-0.027	0.216	0.118	0.425	0.086	
Unconditional effect - conser	-2.394	-4.886	-7.086	-4.950	-5.128		0.006	-0.290	-0.174	-0.625	-0.177	
Unconditional effect - all	-1.848	-3.482	-5.520	-3.311	-5.402		-0.019	-0.085	-0.061	-0.217	-0.092	
P of 1[M_{ct}>0] leads	0.240	0.154	0.898	0.107	0.626		0.241	0.264	0.444	0.350	0.769	
P of Law_{ct}+1[M_{ct}>0] leads	0.042	0.198	0.352	0.376	0.870		0.294	0.128	0.850	0.239	0.749	

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

Appendix Table XII.— The Effects of Free Speech Precedents on Property Crimes

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

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

APPENDIX TABLE XIII

IMPACT OF FREE SPEECH PRECEDENT ON CRIMES
ROBUSTNESS OF 2SLS ESTIMATES

The Effect of Appellate Free Speech Precedent on Offenses Against Family and Children per 100,000

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

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

APPENDIX TABLE XIV

Impact of Free Speech Precedent on Crimes
Robustness of 2SLS Distributed Lag Estimates

The Effect of Appellate Free Speech Precedent on Offenses Against Family and Children per 100,000											
	(t0)		(t1)		(t2)		(t3)		(t4)		(t5)
No Trends	-91.353		-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 +10%, *5%, **1%. Data consist of UCR arrests reported by ORI agencies (at the state-county level). Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, and a dummy for whether there were any cases in that Circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of free speech precedent. Instruments are selected by LASSO. Population weights are population reporting to ORI agency.

Appendix Table XV.— The Effects of Free Speech Precedents on Diseases

Dependent Variable	Chlamydia Incidence						Gonorrhea Incidence						Syphilis Incidence					
	(1)	(2)	(3)	(4)	(5)	Wild BS %LE	(6)	(7)	(8)	(9)	(10)	Wild BS %LE	(11)	(12)	(13)	(14)	(15)	Wild BS %LE
Proportion Progressive Free Speech	-11.04	142.5	-171.7	80.06	67.48	0.54	2.683	47.26	-306.2	70.32	-245.6	0.17	0.327	-3.205	7.592	-5.412	3.787	0.90
Appellate Decisions$_{t+1}$	(13.64)	(137.3)	(549.8)	(148.5)	(100.2)		(10.65)	(41.09)	(2436.5)	(46.22)	(1767.9)		(0.725)	(4.190)	(16.24)	(3.950)	(18.11)	
Proportion Progressive Free Speech	-1.047	186.1*	-71.07	-84.61	249.2*	0.07	4.518	64.70	-30.98	133.6+	-47.10	0.78	-0.386	-2.318	10.55	-5.495	9.191	0.31
Appellate Decisions$_{t}$	(14.03)	(94.12)	(818.5)	(374.9)	(115.2)		(10.42)	(66.96)	(816.6)	(71.03)	(630.0)		(0.922)	(6.006)	(13.29)	(5.245)	(11.62)	
Proportion Progressive Free Speech	14.21	70.15	446.0	380.1	209.1	0.40	8.016	44.79	457.1	115.3	391.2	0.33	-1.263	-6.492	6.928	-8.808	9.680	0.93
Appellate Decisions$_{t-1}$	(19.56)	(67.48)	(1431.5)	(247.2)	(194.4)		(11.05)	(75.12)	(2026.4)	(81.69)	(1613.1)		(0.857)	(7.131)	(20.97)	(6.121)	(21.27)	
Proportion Progressive Free Speech	34.45+	43.20	76.93	157.3	-124.4	0.39	20.84	56.46	365.9	99.35	311.6	0.80	-0.878	-7.445	3.459	-9.131	7.140	0.28
Appellate Decisions$_{t-2}$	(17.44)	(207.0)	(320.3)	(158.0)	(304.5)		(13.36)	(61.55)	(1097.6)	(87.67)	(762.3)		(0.848)	(6.115)	(15.45)	(7.685)	(13.62)	
Proportion Progressive Free Speech	3.188	89.09	264.8	102.2	-79.83	0.52	18.67	72.87+	-563.6	118.4	-500.6	0.82	-0.643	-2.442	0.368	-1.975	-3.938	0.15
Appellate Decisions$_{t-3}$	(16.69)	(78.89)	(1192.9)	(431.6)	(257.6)		(12.52)	(38.36)	(2679.5)	(72.55)	(2039.9)		(0.894)	(5.010)	(23.47)	(5.384)	(18.57)	
Proportion Progressive Free Speech	14.34	48.46	-346.0	355.2	-5.852	0.98	12.58	20.34	615.8	83.74	521.4	0.24	-0.228	3.261	0.0919	6.604	5.797	0.65
Appellate Decisions$_{t-4}$	(17.59)	(117.1)	(925.0)	(329.2)	(193.9)		(11.98)	(36.27)	(2585.3)	(89.54)	(1991.3)		(1.238)	(5.082)	(26.07)	(8.815)	(25.56)	
N	1117	1117	1117	1117	1117		2141	2141	2141	2141	2141		2141	2141	2141	2141	2141	
R-sq	0.736	0.648	0.055	0.369	0.491		0.724	0.707	.	0.642	.		0.576	0.528	0.451	0.467	0.412	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV	N	N	Lasso IV	N	Lasso IV	Lasso IV
Aggregation Level	State - Year						State - Year						State - Year					
Mean dependent variable	207.509	207.509	207.509	207.509	207.509		243.911	243.911	243.911	243.911	243.911		6.748	6.748	6.748	6.748	6.748	
Average Law$_{ct}$ effect	**13.029**	**87.392**	**74.130**	**182.040**	**49.636**		**13.367**	**40.036**	**221.957**	**101.040**	**186.113**		**-3.601**	**-0.243**	**1.853**	**1.025**	**0.681**	
P-value of Law$_{ct}$ lags	**0.014**	**0.000**	**0.979**	**0.211**	**0.000**		**0.404**	**0.263**	**0.987**	**0.027**	**0.980**		**0.172**	**0.946**	**0.598**	**0.589**	**0.756**	
P-value of Law_{ct} leads	0.435	0.299	0.755	0.590	0.501		0.842	0.368	0.900	0.199	0.888		0.906	0.609	0.599	0.705	0.562	
Average 1[M_{ct}>0] lag	0.754	-34.057	-34.856	-56.527	-23.852		7.277	-5.505	-86.507	-32.242	-67.354		1.070	-0.196	-0.890	-0.458	-0.841	
P-value of 1[M_{ct}>0] lags	0.147	0.000	0.507	0.483	0.055		0.477	0.003	0.990	0.159	0.990		0.078	0.966	0.886	0.862	0.599	
P of Law_{ct}+1[M_{ct}>0] lags	0.005	0.064	0.998	0.269	0.012		0.268	0.174	0.985	0.067	0.965		0.328	0.331	0.828	0.431	0.619	
Typical Law_{ct} effect	0.519	3.482	2.953	7.252	1.977		1.107	3.316	18.381	8.368	15.413		-0.158	-0.011	0.081	0.045	0.030	
Unconditional effect - progressive	0.654	2.531	1.863	5.955	1.223		2.237	3.742	14.677	7.455	12.869		-0.132	-0.023	0.050	0.030	-0.008	
Unconditional effect - conser	0.069	-3.140	-3.214	-5.212	-2.199		1.145	-0.866	-13.616	-5.075	-10.602		0.102	-0.019	-0.084	-0.043	-0.080	
Unconditional effect - all	0.613	-0.757	-1.384	0.218	-0.991		2.708	2.105	-0.649	1.275	0.596		-0.018	-0.036	-0.035	-0.015	-0.080	
P of 1[M_{ct}>0] leads	0.482	0.445	0.957	0.408	0.547		0.163	0.758	0.875	0.946	0.859		0.731	0.477	0.473	0.621	0.577	
P of Law_{ct}+1[M_{ct}>0] leads	0.709	0.252	0.756	0.432	0.538		0.184	0.230	0.930	0.091	0.930		0.822	0.773	0.815	0.842	0.718	

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

APPENDIX TABLE XVI

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

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

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

APPENDIX TABLE XVII

Impact of Free Speech Precedent on Sexually Transmitted Disease
Robustness of 2SLS Distributed Lag Estimates

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

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

Appendix Table XVIII.— The Effects of Free Speech Precedents over Time

Dependent Variable	1973-1993								1980-2000							
	Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK		Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Proportion Progressive Free Speech	-0.00252	-0.00526	7.321+	-3.559	-1.277+	-0.597	-0.0249*	-0.0397	-0.00153	0.00277	-4.355	37.10+	0.0851	-0.466	-0.00427	0.0854
Appellate Decisions$_{t+1}$	(0.00529)	(0.00426)	(3.669)	(10.07)	(0.601)	(0.510)	(0.00892)	(0.0962)	(0.00228)	(0.0125)	(3.487)	(21.78)	(0.133)	(0.892)	(0.0151)	(0.0706)
Proportion Progressive Free Speech	0.00397	-0.00174	10.71*	-4.110	-1.475+	-0.360	0.0113	-0.0594	0.00605	-0.00560	1.015	11.29	-0.308	1.796	-0.0119	-0.0315
Appellate Decisions$_{t}$	(0.00433)	(0.00300)	(4.475)	(13.67)	(0.674)	(0.338)	(0.0219)	(0.104)	(0.00352)	(0.0320)	(5.287)	(17.10)	(0.217)	(2.292)	(0.0361)	(0.123)
Proportion Progressive Free Speech	-0.00301	-0.00932*	9.339*	-13.48	0.0950	0.509	-0.0186	-0.0897	-0.0000587	-0.00204	0.512	0.0865	0.733	1.208+	-0.0131	-0.0623
Appellate Decisions$_{t-1}$	(0.00490)	(0.00385)	(3.520)	(19.53)	(0.505)	(0.494)	(0.0169)	(0.0908)	(0.00339)	(0.0100)	(2.197)	(7.168)	(0.417)	(0.682)	(0.0241)	(0.140)
Proportion Progressive Free Speech	0.00841+	0.00200	6.538	5.140	-1.065	-0.807+	-0.00664	-0.0141	0.00660*	0.0188	1.373	10.92	-0.0173	-0.516	0.0215	0.126
Appellate Decisions$_{t-2}$	(0.00441)	(0.00508)	(4.924)	(12.91)	(0.639)	(0.464)	(0.0177)	(0.0729)	(0.00213)	(0.0126)	(4.586)	(12.28)	(0.186)	(0.836)	(0.0237)	(0.219)
Proportion Progressive Free Speech	0.00387	-0.00493	3.438	-16.55	-0.728	-0.551*	-0.0128	-0.0971	0.00520*	0.0183	-2.531	-0.394	-0.244	-0.694	-0.0101	-0.114+
Appellate Decisions$_{t-3}$	(0.00526)	(0.00744)	(2.724)	(13.63)	(0.556)	(0.254)	(0.0148)	(0.126)	(0.00229)	(0.0174)	(3.577)	(11.16)	(0.206)	(0.994)	(0.0300)	(0.0614)
Proportion Progressive Free Speech	0.00609+	0.00185	7.293+	18.76**	-0.446	0.365	0.0115	0.00922	-0.00102	-0.00566	6.450	24.00*	0.0502	0.545	0.0190	0.165+
Appellate Decisions$_{t-4}$	(0.00328)	(0.00384)	(3.373)	(5.851)	(0.411)	(0.326)	(0.0134)	(0.117)	(0.00212)	(0.00572)	(5.016)	(10.17)	(0.112)	(0.744)	(0.0150)	(0.0851)
N	9969	9969	26961	26961	9392	9392	20930	20930	16659	16659	43992	43992	15346	15346	18073	18073
R-sq	0.003	0.002	0.160	0.156	0.014	0.012	0.044	0.041	0.002	.	0.146	0.133	0.009	0.001	0.057	0.052
Appellate IV	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
District IV	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
Mean dependent variable	0.003	0.003	5.060	5.060	1.130	1.130	0.219	0.219	0.003	0.003	5.104	5.104	1.129	1.129	0.267	0.267
Average Law$_{ct}$ effect	**0.004**	**-0.002**	**7.463**	**-2.050**	**-0.724**	**-0.169**	**-0.003**	**-0.050**	**0.003**	**0.005**	**1.364**	**9.181**	**0.043**	**0.468**	**0.001**	**0.017**
P-value of Law$_{ct}$ lags	**0.083**	**0.000**	**0.108**	**0.000**	**0.101**	**0.047**	**0.394**	**0.008**	**0.036**	**0.123**	**0.056**	**0.050**	**0.348**	**0.031**	**0.771**	**0.000**
P-value of Law_{ct} leads	0.643	0.217	0.074	0.724	0.057	0.242	0.018	0.680	0.514	0.824	0.240	0.089	0.535	0.601	0.783	0.227
Average 1[M_{ct}>0] lag	-0.004	0.001	-3.616	1.365	0.394	0.358	0.003	0.022	-0.001	-0.002	-0.898	-3.985	0.112	0.003	0.006	-0.002
P-value of 1[M_{ct}>0] lags	0.087	0.000	0.202	0.004	0.318	0.164	0.193	0.195	0.129	0.017	0.089	0.025	0.537	0.133	0.064	0.600
P of Law_{ct}+1[M_{ct}>0] lags	0.831	0.000	0.186	0.007	0.094	0.127	0.182	0.001	0.095	0.450	0.002	0.181	0.313	0.000	0.000	0.000
Typical Law_{ct} effect	0.000	-0.000	0.545	-0.150	-0.026	-0.006	-0.000	-0.005	0.000	0.000	0.064	0.430	0.001	0.012	0.000	0.001
Unconditional effect - progressive	0.000	-0.000	0.301	-0.054	-0.012	0.007	-0.000	-0.004	0.000	0.000	0.023	0.260	0.004	0.012	0.000	0.001
Unconditional effect - conser	-0.001	0.000	-0.630	0.238	0.060	0.055	0.001	0.006	-0.000	-0.000	-0.104	-0.460	0.013	0.000	0.001	-0.000
Unconditional effect - all	-0.001	0.000	-0.330	0.180	0.048	0.062	0.001	0.003	-0.000	-0.000	-0.079	-0.203	0.017	0.012	0.002	0.000
P of 1[M_{ct}>0] leads	0.801	0.582	0.416	0.768	0.245	0.154	0.714	0.847	0.253	0.403	0.373	0.192	0.246	0.620	0.111	0.971
P of Law_{ct}+1[M_{ct}>0] leads	0.110	0.290	0.116	0.938	0.015	0.510	0.111	0.590	0.096	0.891	0.316	0.041	0.206	0.677	0.402	0.104

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