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

Daniel L. Chen and Susan Yeh *

Abstract Does law shape values? We test a model of law and norms using an area of law with emotional salience and controversy. Since 1958, Democratic judges have been 10% more likely than Republicans to favor progressive free speech standards. Using the random assignment of U.S. federal judges setting geographically-local precedent, we estimate that progressive free speech standards liberalized sexual attitudes and behaviors and had secondary effects on sex-related crimes and diseases. To corroborate a causal channel we conduct a field experiment with data entry workers. Exposure to progressive decisions liberalized sexual attitudes and shifted norm perceptions but not self-reported behavior.

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

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

*Daniel L. Chen, Toulouse Institute for Advanced Studies, daniel.li.chen@gmail.com; Susan Yeh, George Mason University, syeh2@gmu.edu. We thank research assistants and colleagues with helpful comments at economic faculties at Columbia, NBER Economics, Religion, and Culture Conference, Economic Science Association, Transatlantic Economics of Crime Workshop, and Association for the Study of Religion, Economics, and Culture Conference and law faculties at University of Chicago, Georgetown, Duke, George Mason, Catholic University, American Law and Economics Association, Midwest Law and Economics Association, Canadian Law and Economics Conference, and Law and Society Annual Meetings. We thank Andres Sawicki for sharing data. Work on this project was conducted while Daniel Chen received financial support from the European Research Council, Swiss National Science Foundation, Ewing Marion Kauffman Foundation, Institute for Humane Studies, Templeton Foundation, and Agence Nationale de la Recherche. Both authors received financial support from the John M. Olin Center for Law, Economics, and Business at Harvard Law School.

"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

Where do normative commitments come from and what are their effects? From abolition of slavery, to women's liberation, to environmentalism, different groups often had different views on what they thought was the right or just thing to do, which led to group conflict. How do these groups innovate conceptions of rights? Law is speculated to play a key role in moral revolutions (Tushnet 2009; Appiah 2011; Bénabou and Tirole 2012; Acemoglu and Jackson 2014), yet little causal evidence exists to date. This paper illustrates economic mechanisms and behavioral influences on notions of justice and development of rights using both revealed preference and sparse data approaches. While many economists have focused on deterrence to explain the behavioral response to law (Becker 1968), a large body of work by psychologists suggests that laws can affect people's behaviors simply because it tells them what is the right thing to do (Tyler 2006; McAdams and Nadler 2008). Whether laws shape preferences, and in which direction it shapes preferences, is important for those conducting cost-benefit analyses of judicial decisions (Posner 1998b) and those who want to know whether the decisions accord with the democratic will of the people (Breyer 2006), especially if the decisions shape the democratic will.[1] Some suggest that war is about a conflict of "sacred values" (Bowles and Polania-Reyes 2012; Chen 2006, 2010; Chen and Schonger 2013, 2015) and an open question in international law is whether custom, which often hinders equity-increasing changes (Aldashev et al. 2012), can be shifted in the direction intended by formal institutions.

[1]Methods to evaluate the impact of judicial decisions may help judges who are interested in the broader empirical consequences of their decisions. Judge Richard Posner has lamented that, "[judicial] opinions lack the empirical support that is crucial to sound constitutional adjudication" (Posner 1998b); similarly Justice Breyer remarked, "I believe that a[n] interpretive approach that undervalues consequences, by undervaluing related constitutional objectives, exacts a constitutional price that is too high" (Breyer 2006).

The great econometric challenge to knowing whether laws shape values is two-fold: 1) causal inference, which we derive from random variation of legal precedent, and 2) distinguishing expressive from deterrence effects, since the behavioral response is in the same direction. To overcome these challenges, we employ an area of law, namely, obscenity law[2], since these laws are likely to have effects other than through economic sanctions alone because of their emotional salience and controversy. To explore the mechanism, we also employ an experiment whose relatively short time frame means that deterrence is unlikely to explain the behavioral changes. 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 controversy has arisen over obscenity. Many countries worried about the possible impact of obscenity have issued a number of regulations[3], while courts have wrestled with the interpretation and legality of these regulations. In the U.S., the Supreme Court greatly expanded civil liberties in the 1960s and states quickly liberalized their own regulations in response. A recent study attributes 50% of the sexual revolution to individuals' moral views on sexual rights shaped by laws and doctrine (Fernandez-Villaverde et al. 2014).[4] A fundamental rationale in the U.S. precedent in obscenity cases has been to protect what it perceives as the moral fabric of society. Moral harms and their "secondary effects" 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). Yet little is known about what regulations on obscene speech actually do and whether the rationale put forward by

[2]In particular, we analyze free speech laws related to obscenity. Free speech laws related to commercial speech, speech that proposes a commercial transaction, is likely to have effects through economic channels; so we focus on expressive conduct and symbolic speech. We refer to free speech obscenity regulation as free speech regulation or obscenity law for brevity.

[3]As social norms change and technology facilitates broader dissemination of media, obscene content continues to push previously-held boundaries. In India, couples who elope can be stoned and kissing in public has led to charges of obscenity. The Indian government has authorized the prosecution of Facebook, Yahoo!, and Google over obscene material. In Russia, newly enacted laws have banned obscenities in public performances. In Norway, the staggered introduction of internet broadband increased sex-related crimes, which is suggested to be due to the increased consumption of obscene content (Bhuller et al. 2013).

[4]Structural estimates of U.S. data over the past century attribute roughly 50% of the sexual revolution to socialization—individuals' moral views on sexual rights or the rights of illegitimate children—coming from the laws and doctrines that stigmatize and pressure parents to socialize (Fernandez-Villaverde et al. 2014).

policy makers and scholars of female empowerment[5] concerned about the commodification of women and the potential deleterious secondary effects (i.e., sexual violence, child sexual abuse, disease and drugs) are empirically justified (Dworkin and MacKinnon 1988; Radin 1996).[6]

Isolating the effects of laws from technological or other factors that facilitated the sexual revolution is challenging.[7] In the U.S., the three-part *Miller* test[8] and the *Roth* test[9] before it has, for about 50 years, instructed the courts to define obscenity according to community standards. So, if free speech precedent gives people more room for sexually progressive expression and if more progressive community standards make it easier to subsequently challenge regulations that are deemed as restrictive, this dynamic could lead to multiple steady-states, in which abrupt shifts in normative commitments could occur (Akerlof et al. 1996; Cooter et al. 2008), as laws mobilize individuals, are disseminated to the media, or are otherwise indirectly promulgated. Laws are theorized to influence the population through moralizing language designed to affect social norms and ultimately judgment and decision-making

[5]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, a link 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.

[6]The most systematic evidence on the effect of exposure to sexual media content comes from psychology, in the form of laboratory experiments on how subjects respond to exposure to pornographic material. Most studies find that pornography, especially violent pornography, increases sexual aggression (Donnerstein and Linz 1986; Allen et al. 1995), though some experiments find no effect or a reduction in sexual aggression after exposure to pornography (see, e.g., Zillman and Bryant (1984)). Some studies examine whether this effects translate into higher crime rates in the field. Kendall (2007) uses U.S. state-level panel data and finds a negative association between internet subscription and rape incidences, but Baron and Straus (1984) find a strong positive association between the circulation of eight pornographic magazines across U.S. states and crime, even after controlling for a number of possible confounders. Bhuller et al. (2013) exploit plausibly exogenous variation in internet use to deal with the standard problems of simultaneous causality and correlated unobservables. Their findings suggest that the increased consumption of obscene content increased sex-related crimes.

[7]Contraceptive use has been linked to the sexual revolution (Akerlof et al. 1996).

[8]Since 1973, the legal standard defining obscenity in the U.S. has been the three-part *Miller* test set out in the Supreme Court decision *Miller v. California*, 413 U.S. 15 (1973). The *Miller* test defines material as obscene if "the average person, applying contemporary community standards" would find that the material (1) "appeals to the prurient interest"; (2) has "patently offensive" depictions of sexual conduct; and (3) "lacks serious literary, educational, artistic, political, or scientific value."

[9]Before the *Miller* test, the *Roth* test allowed banning obscenity when the average person, applying contemporary community standards, would consider the dominant theme of the material, taken as a whole, appeals to prurient interests.

because the lawmaker has authority or because of peer effects that shape the perceived morality of the rule-breaker. Laws can discourage undesirable practices in ways that exceed the expected effects of punitive sanctions, by influencing the population through moralizing language designed to affect social norms and ultimately judgment and behavior (Sunstein 1996; Kahan 1997). The laws induce individuals to change their behavior because of pressure brought to bear upon them through societal sanction that differs from the official sanction imposed by the law (Anderson and Pildes 2000). Part of the econometric difficulty in isolating causal effects is that courts may make progressive decisions if social mores and therefore the community standards 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.

Several U.S. Supreme Court decisions and the Warren Court greatly expanding civil liberties coincide with the onset of the sexual revolution. To disentangle the effects of law from other social factors, 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 a manner 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. Only 2-3% 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. 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 or Democrats[10]–creates exogenous variation in the progressivity of free speech precedent, which we can use to estimate their causal impact on moral values and behavior. In principle, we have 612 experiments (across 51 years and 12 Circuits)[11] in contrast

[10]As shorthand, we will refer to judges appointed by Democratic presidents as "Democrats."
[11]The maximum is slightly lower since Circuit 11 was only created in 1981.

to the 50 experiments used in analyses of state law changes. Since Circuit Courts are only to hear cases presenting new legal issues (only 10-20% of District Court opinions are appealed), cases representing big legal changes occur throughout the time period.

Both advocates and critics of legal change recognize the possibility that laws can have effects through the moral messages that they convey.[12] Previous field studies of expressive law (Funk 2007), expressive externalities (i.e., spillover effects) of law (Fox and Griffin, Jr. 2009), and free speech regulations in particular (Paul et al. 2001), have only been cross-sectional or time-series and have lacked a clear control group.[13] Consistent with court decisions being able to precipitate rapid change, Bailey (2010) documents that following progressive Supreme Court obscenity precedent, state statutes quickly liberalized obscenity regulations. Several studies have also linked major court rulings with subsequent changes in public opinion where the case originates and suggested that media plays a prominent role (Hoekstra 2000).[14] In addition, information entrepreneurs, such as community organizations raising awareness can also act as a catalyst. For example, Weinrib (2012) documents how, in response to major Circuit Court free speech precedent, ACLU attorneys mobilized individuals towards a view that speech should be protected regardless of its social value. As a consequence of direct and indirect promulgation, 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).

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. Little is known about when law causes what is viewed

[12]Segregationists feared the effects of *Brown v. Board of Education* on the indoctrination of racial prejudice among white youth (Walker 2011).

[13]Some studies also document the mechanisms by which laws that affirm moral standards or sexual norms can lead to broader consequences. For example, Dennis's (2007) historical discussion detailed the cultural and literary consequences of the enactment of the Comstock Act; Stroebel et al.'s (2012) study finds 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 through peer effects to create positive feedback (Card and Giuliano 2011) and has also been linked to STDs (Klick and Stratmann 2003).

[14]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.

as moral to shift towards or against what the law intends. 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,[15] but a clear framework for assessing the likelihood of their occurrence would help policymakers (Lessig 1998; Ellickson 1998).

To the best of our knowledge, there exists one formal model that allows both expressive and backlash effects to occur (Bénabou and Tirole 2012), a simplified version of which we present below and apply to the context of court-made law in Section 2. The model assumes 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., as an information multiplier): Prohibitions cause people to think that the government sees a problem and that obscene activity is more prevalent. It is then easier for those who are motivated by intrinsic incentives to signal their honor to others, which we call 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 morality of stigmatized activities can increase substantially if the shift in beliefs about its prevalence cause stigmatized activities to become normalized. When the normalizing effect exceeds the signaling effect, we call this "backlash," since the law causes what is viewed as moral to shift against what the law values. In our application, the court is the social planner; it issues a precedent that raises the costs to bookstores or distributors, but this precedent, when promulgated, informs people that more people are conducting the inappropriate activity than they previously thought. Since survey data rarely ask individuals to report how many people are believed to conduct these activities, we need to combine methods from both field and lab in order to separate expressive

[15] For a sample of backlash claims, see: abortion (Pridemore and Freilich 2007), desegregation (Klarman 2005), multiculturalism (Mitchell 2004), globalization (Eckes 2000), environmentalism (Wolf 1995), voter mobilization (Mann 2010), private infrastructure investments (Lopez et al. 2009), health care (Mechanic 2001), Americans with Disabilities Act (Krieger 2000), and Warren Court (Feld 2003).

from deterrence effects.

In Section 3, we develop a method to test this model using random variation in jurisprudence. 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 cases (Sunstein et al., 2006; Songer and Haire, 1992). We cite historical evidence on information transmission of Circuit Court decisions and document that newspaper articles about Circuit obscenity decisions increase in the Circuits and years with 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. In Section 4, we estimate the subsequent impact of free speech precedent on self-reported sexual attitudes and behaviors (which are themselves a form of speech) and on government statistics of crime and disease (which are the secondary consequences perceived by judges and policymakers to follow from progressive free speech decisions). We choose these outcomes because these are the ones that motivate the U.S. courts. 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 by judges to justify the exercise of police powers in restricting expressions of obscenity.

We find that progressive free speech jurisprudence on average led to more progressive attitudes on premarital, extramarital, and homosexual sex; and more progressive sexual behavior, especially by men, in having more sexual partners, non-marital sex, and paid sex. 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., 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 chlamydia, one of the sexually transmitted diseases (STDs) with the

fastest increase in incidence. No effects are found for gonorrhea and syphilis.[16] 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 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, verifying many of the assertions put forth by previous scholars and judges. In terms of magnitudes, the effect sizes are much smaller than the 50% of the sexual revolution that has been attributed to individuals' moral views on sexual rights shaped by laws and doctrine (Fernandez-Villaverde et al. 2014) but are closer to the effect sizes found in studies with plausibly exogenous variation, such as the expansion of internet broadband on sex crimes in Norway (Bhuller et al. 2013), where 3.2% of the total number of rapes and 2.5% of the total number of sex crimes and child sex abuses that occurred between 2000 and 2008 would have been avoided if broadband internet had not been introduced.

Through three experiments–two of which are reported in this paper–we explore whether self-reported behavior reflects actual changes in underlying behavior and verify that exposure to free speech precedent could indeed affect attitudes. These experiments also put bounds on the total indirect effects of the law for individuals not directly exposed to free speech precedent. Two experiments are reported in Section 5 and the third in Chen and Yeh (2014a), all of which replicate the basic finding of law's expressive effects. Data entry workers were assigned to transcribe newspaper summaries of free speech 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 progressive sexual behaviors. This difference suggests that self-reported behavioral shifts in response to free speech decisions were not simply due to people's openness to discussing sexual behaviors.

[16] One reason may be sorting among sexual partners based on their disease status, which can also lead to multiple equilibria (Kremer 1996).

The 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. These workers also reported believing a lower percentage of people having extramarital sex, verifying the information multiplier in the theoretical model.

The theoretical model also suggests that whether law has expressive or backlash effects depends on the underlying social norms. When previously-stigmatized activities were relatively scarce and conservative free speech decisions caused people to update their beliefs that the stigmatized activities were more common than previously thought, these activities became normalized, which caused more people to do the previously-stigmatized activity. In our data, a large number of free speech decisions occured amid 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 to signal intrinsic motivations. Progressive free speech decisions then have expressive effects. Consistent with this prediction, 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. This prediction is corroborated experimentally in Chen and Yeh (2014a), which finds that in communities where stigmatized activity is rare, progressive free speech precedent caused individuals to backlash and lowered their subjective well-being. The opposite occured for individuals in communities where stigmatized activity is more common.

Besides the effects of law on attitudes, several additional pieces of evidence are consistent with expressive effects rather than deterrence effects as the main channel to explain the findings. First, the role of material penalties is unlikely to be significant in the short time frame of our experiments. Moreover, backlash effects would not be explained by deterrence.[17]

[17]We also collected data on state-level sales of pornographic magazines as pornography media providers were often parties in free speech litigation. However, magazine circulation did not respond to free speech decisions, though we cannot rule out a possible change in content or other channels for obscene content to shift and reach the public.

Finally, the effects of free speech law on paid sex reported by individuals and arrests for prostitution reported by the police move in tandem from backlash to expressive. This suggests that the effects found in the arrest data may reflect actual changes in underlying behavior and are not due to changes in law enforcement aggressively making arrests in response to court decisions.

Our findings may shed light on contemporary debates over same-sex marriage and discrimination. Though we emphasize that the decisions are about obscenity as defined in its historical context and not gay rights per se, of the 175 free speech cases in our database, 45% mention "gay" or "lesbian;" including the historical euphemism, "pervert," increases the proportion of cases related to homosexuality to 65%. As communities continue to evolve along with conceptions of rights, this model may help explain why, for example, harsh sentencing in gay hate crimes have been feared to lead to backlash.[18] Though this paper answers a question different from that addressed in the usual research on law and norms, it has the advantage of a relatively clear source variation that allows identification of any effects. Besides building on the empirical analyses of Bhuller et al. (2013) and Bailey (2010), which examine the respective contribution of technology and law, our study also contributes to a growing literature that estimates the causal effects of cultural influences (e.g., Gentzkow and Shapiro 2008; Dahl and Della Vigna 2009).

Our study focuses on the more basic and timeless question of whether laws influence conceptions of rights, albeit in a very particular setting that replicates across three experiments and the field. An analysis of individual judgments or a randomized control trial, however, would not capture general equilibrium effects, such as factor mobility in response to legal regulations. Our causal estimates of law on economy-wide outcomes incorporate these general equilibrium responses. We make three additional methodological contributions to the literature using the random assignemnt of judges. First, we estimate the delayed general equilibrium effects by instrumenting for the presence of Circuit cases using the random assignment of judges in the District Courts from which an appeal generates the Circuit cases

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

that we analyze. Data collection from both Circuit *and* District Courts allows isolating three separate parameters of policy interest: 1) the effect of progressive precedent where the counterfactual is conservative precedent, 2) the effect of progressive precedent where the counterfactual is no precedent, and 3) the effect of conservative precedent where the counterfactual is no precedent. Second, in common law, hard cases–i.e., compliers–precede easy cases–i.e., always-/never-takers. In this setting, the Local Average Treatment Effect becomes the Treatment on Treated Effect. Cases that reach the Circuit Courts are the more challenging and legally innovative cases. 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. Third, we link the experimental findings to the field estimates. The Treatment on Treated effect comprises of a direct effect on individuals exposed to and aware of the law and indirect effects in the form of expressive externalities. To the extent that one's priors are that the probability of direct exposure is small, then the probability of indirect exposure or the effect size of indirect exposure is large.

2 Background

We begin by defining what is being modeled. We define expressive effects as occurring when the law shifts preferences towards what the law values, and backlash effects as when the law shifts preferences away from what the law values. The theoretical framework is intended to assist in understanding when laws have expressive as opposed to backlash effects. 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, social sanctions provided by society affect actions. We focus on two sets of multipliers. The first is a social multiplier, where people accrue honor or stigma for actions outside the norm—for example, if very few people use drugs, then drug users receive stigma; if very few people donate millions, then generous donors receive honor. The second multiplier is 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). 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); Chen et al. (2015b); 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).

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 positive 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 social planner – in our application, the judge – sets the optimal payoffs and costs. We ignore the objective function of the social planner in this paper because we are interested in how court decisions affect people's actions and we exploit random variation in court decisions.

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

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

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

In our application, we can think of $a = 0$ as having extramarital sex and $a = 1$ as abstaining from extramarital sex—it does not matter whether the action is an inaction, so long as there is a duty associated with $a = 1$ and the perceived morality of individuals is higher if they choose $a = 1$. This is probably true for most of the time period that we study.

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); with ordinal utilities, we can rewrite net utilities as:

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

$$\text{if } a = 0 : U(0) = \mu E(x \mid 0)_s$$

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

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

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

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

where we define:

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

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

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

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

population have extramarital sex. Note that the action need not be observable by others for the model to apply.$E(x \mid a)_s$ could also capture one's own perception of intrinsic motivations in a self-signalling framework.

This expression $\Delta(v)$ maps onto our use of the General Social Survey (GSS), where people respond to questions about the morality of particular actions. The reason is that by reporting what is their perceived morality of an action, respondants are reporting the difference in the social perception of someone who chooses $a = 1$ vs. the social perception of someone who chooses $a = 0$.μ does not correspond to the GSS question because μ is the weight that respondants put on the morality of an action.

The expression $v^* + \mu\Delta(v^*)$ captures the marginal benefit for the people at the cutoff. This marginal benefit is the sum of intrinsic motivation and social motivation. If $1 + \mu\Delta'(v) > 0$, then as the cut-off increases, the marginal benefit will eventually equal the marginal cost c and there will be a fixed point. 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 cut-off 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.

Under reasonable assumptions,[19] Δ initially decreases, then increases. To see why, when v^* is small (i.e., most people choose $a = 1$), 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 large share of individuals with high v's, and this is 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 choose $a = 1$, whereas everyone to the left of v^* will choose $a = 0$. For $E(v_a \mid 1)$, 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 are cutting off a small fraction proportionately. The morality of individuals who choose $a = 0$ increases more than the morality of individuals who choose $a = 1$. In words, extramarital sex becomes normalized, so more people do it.

[19] See Bénabou and Tirole (2012).

However, 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 choosing $a = 1$ when everyone else is choosing $a = 0$. Whether v^* is on the left-side or the right-side of the distribution determines whether the social multiplier makes actions strategic substitutes (i.e., when more people choose $a = 0$, this makes the choice of $a = 1$ scarcer and more honorable, which causes more people to choose $a = 1$) or strategic complements (i.e., when more people choose $a = 0$, this normalizes the choice of $a = 0$, which causes more people to choose $a = 0$).

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, such as during the sexual revolution.

2.2 Information Multiplier Now suppose individuals misperceive the distribution–a phenomenon called pluralistic ignorance in the psychological literature. Consider the case where v^* is on the left side of the distribution of actions, meaning there are few publicly known extramarital sexual activities: this could be true in some communities. 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. The judge gathers information about v^* and issues a sanction when she believes v^* is too high. The judge has information about v^* because of the *Miller* community standard test, which incentivizes litigants to bring information to the judge. Upon hearing what the judge has to say, community leaders update their beliefs about the underlying distribution. Therefore, explicit sanctions substitute for norm-based stigma. That is, law undermines the intrinsic and social norm-based motivations for choosing $a = 1$, and we obtain a backlash effect. The previously stigmatized activity becomes normalized and the morality of choosing $a = 0$ increases faster than does the morality of choosing $a = 1$.

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 about the true distribution strengthens the stigma effect and complements the norm-based stigma. However, explicit sanctions indicating that the policymaker sees a problem does the opposite 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 decisions have backlash effects no matter the direction of pluralistic ignorance. The backlash effect can even exceed the sanctions effect if judges do not optimally account for these non-deterrence-based effects or miscalculate μ. Therefore, we might expect to see backlash effects in the time before the sexual revolution or during its early stages.

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. Now 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, however, shift beliefs about v^* to be even higher, which reinforces the honor effect. The mechanism works in reverse for progressive decisions: lowered sanctions shift beliefs about v^* to be lower (an assumption that we test experimentally), which reduces the honor effect and causes more people to choose $a = 0$. We might expect expressive effects in sexually liberated communities.[20]

[20] In Bénabou and Tirole (2012), government policy can also provide information about externalities. If, for example, conservative free speech decisions inform or remind individuals about the negative externalities and secondary consequences from obscenity exposure, 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 increases beliefs about the negative externalities of obscenity: Individuals are no more likely to believe that sexual materials lead to the breakdown of morals and no more likely to believe that 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. These findings support the identification of law's expressive effects via information about the prevalence of stigmatized activity rather than via information effects about its negative externalities.

3 Design of Field Study

3.1 Identification Strategy Free speech law in the U.S. is represented at several levels. At the local level, city ordinances, for example, disallow the showing of explicit films at theaters; on the federal level, FCC regulations prohibit television stations from broadcasting obscene content and federal statutes regulate 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.[21] As it happens, there is no umbrella federal statute in the U.S., so whether or not something is obscene 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 *stare decisis*—a 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 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

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

two other "regulated uses" (which includes 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. Appendix A provides additional examples of the changing legal doctrine on obscenity.

Each Circuit Court decides many thousands of cases per year that are *binding precedent* within that Circuit, but less than one case per Circuit per year is related to obscenity, which heightens their importance. 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 also *persuasive precedent* on state courts within the Circuit. Persuasive precedent must be adopted by the state courts to become binding precedent.[22]

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.[23] With some small exceptions, 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.

Experience, legal philosophy, or group identity may cause judges to view issues in different ways (Chen and Spamann, 2014; Berdejó and Chen 2014). It has been documented that judges' gender, race, religion, and political persuasion all are predictive of how judges vote (Peresie 2005; Chang and Schoar 2013; Chen and Yeh 2014b). Historians have also documented that judges rely on personal values influenced by historical forces (Klarman 2004). Our causal inference therefore comes from the random assignment of judges who interpret the facts and the law differently and, in particular, the fact that Democratic judges vote

[22]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.

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

differently from Republicans on free speech obscenity cases (Sunstein et al. 2006; Songer and Haire 1992). This exogenous variation in establishment of precedent across different regions allows us to identify the causal effects of obscenity law on sexual attitudes, behavior, crime, and disease.

Our identification strategy also exploits the random assignment of District Court judges. A District Court has several courthouses (also referred to as Divisions and not visible in Figure 2); in some Districts, random assignment is at the courthouse level. The demographic characteristics of District judge are correlated with whether the District judge is reversed by the Circuit Courts (Haire, Songer, and Lindquist 2003; Sen 2015; Barondes 2010; Steinbuch 2009), so expected reversal rates could encourage litigants to pursue an appeal. We use this variation to identify the presence of a Circuit case. This in turn permits the identification of a second and third counterfactual. The three counterfactuals of interest are (1) the effect of progressive precedent where the counterfactual is a conservative precedent, (2) the effect of progressive precedent where the counterfactual is no precedent, and (3) the effect of conservative precedent where the counterfactual is no precedent.

3.2 Data

3.2.1 Legal Cases Our empirical analysis draws on several sources of data on free speech cases—established datasets as well as our own data collection.Sunstein et al. (2006) and Kastellec (2013) collect data on all Circuit free speech decisions pertaining to obscenity and 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),[24] *Eckstein v. Melson* (selling magazines and books with explicit sexual imagery),[25] and *Penthouse v. McAuliffe* (showing a *Penthouse* movie).[26] More recently, cases in the 1990s and 2000s involve downloading images from the Internet[27] and making lewd phone calls.[28] We also collect all District Court obscenity cases, yielding 2,960 cases from 1957 to 2008. We use this data and the random assignment of District Court judges to identify the presence of a Circuit Court case.

Sunstein et al. (2006) and Kastellec (2011) code the cases in the following manner: Decisions supporting a finding that the activity was not obscene within the meaning of the law are coded as progressive, whether because the material itself was not obscene according to the three-part *Miller* test or because individual interest in free expression outweighed the state's interest in protecting individuals from the effects of obscenity (this rationale is articulated in *Ginsberg v. New York* (390 U.S. 629 (1968))). Appendix Table I lists all the cases and their coding.

Figure 4 plots the quantity of free speech cases that were decided progressively or conservatively over time. Table I indicates that, on average, there were 0.30 free speech cases per Circuit-year for a total of 175 cases. Roughly two-thirds of these are conservative decisions. The ratio of progressive to conservative decisions is lower after 1973, the year of the *Miller* decision, compared to 1958–1972, when *Roth* was the standard. Songer and Haire (1992) find the same results, which they attribute to the causal impact of *Miller*. A dramatic spike is also observed in both the number of free speech cases and the number of conservative 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,[29] Federal Judicial Center, and our own data collection. The final dataset includes information on vital statistics. Variables

[24] 259 F.2d 54 (3d Cir. 1958).
[25] 18 F.3d 1181 (4th Cir. 1994).
[26] 702 F.2d 925 (11th Cir. 1983).
[27] United States v. Thomas, 74 F.3d 701 (6th Cir. 1996).
[28] United States v. Landham, 251 F.3d 1072 (6th Cir. 2001).
[29] http://www.cas.sc.edu/poli/juri/attributes.html

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

include: geographic history, education, occupational history, governmental positions, military service, religion, race, gender, and political affiliations. Raw data on religion come from Goldman (1999).[30] 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.[31] In expectation, there are 0.43 Democrats per seat (i.e., 1.3 Democrats expected on a panel of 3 judges) (Table I). We calculate the expectations based on the composition of the Circuit pool of judges available to be assigned in any Circuit-year assuming that all judges have an equal probability of assignment.[32]

3.2.3 Attitudes and Behaviors We use the General Social Survey (GSS) to measure sexual attitudes and behaviors.[33] 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

[30] 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.

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

[32] 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.

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

and societal phenomena. The GSS provides responses from approximately 1,500 respondents for each survey year between 1973 and 1992, and approximately 2,900 respondents per survey year from 1994 to 2004, for a total of 44,897 sample individuals between 1973 and 2004. This is the same dataset that Fernandez-Villaverde et al. (2014) use in their structural estimates.

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). 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 perceptions of those who choose$a = 1$ as opposed to $a = 0$ in the model, i.e.$\Delta(v) = E(v_a \mid v_a > v) - E(v_a \mid v_a < v)$. We also construct a measure for community standards using the response to whether sexual materials lead to breakdown of morals. We include this an additional control because the *Miller* standard instructs judges to take into account the community's standards. We use GSS survey weights in our regressions as recommended by GSS.

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. 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. We also include standard controls for crime in the crime regressions: unemployment rate, per capita real income, police employment, the proportion of the population that is nonwhite, percent urban, infant mortality, and the age profile of the population in each state and year. These variables

are obtained from official U.S. government publications. County population numbers are used as weights.

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 (i.e., new cases) of sexually transmitted diseases–chlamydia, syphilis, and gonorrhea–for each state from 1984 to 2008 from the Centers for Disease Control and Prevention[34] and extend syphilis and gonorrhea back to 1960 from Klick and Stratmann (2003). The STD incidence rates are weighted by annual state population numbers from the U.S. Census.[35] Unweighted regressions are reported in robustness checks.

3.3 Empirical Intuition and 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. Consider the following conceptual illustration. Suppose one Circuit has a high proportion of judges who are Democrats and another Circuit has 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. 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. Rather, the strategy relies on the fact that, from year to year, there is random variation in the proportion of free speech cases that are assigned to Democrats in the first Circuit. This idiosyncratic variation is not predetermined since judicial assignments are not revealed to parties until after each litigant's briefs are filed. 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

[34]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.

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

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 Circuits and years. In the most parsimonious specification, we would simply examine the relationship between idiosyncratic variation in judicial assignment with outcomes that are intermediate (like the law) or final (like conceptions of rights and secondary effects). However, because the "units" of treatment (i.e., individuals, counties, and states) get repeatedly treated over time and because not every Circuit-year has a case, we need to address violations of SUTVA (Stable Unit Treatment Value Assumption) with additional modeling and data. See Chen and Yeh (2013) for an extended discussion of the empirical intuition without the technical detail that follows below.

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 as suggested by the data source, though in robustness checks we omit them. $Law_{c(t-n)}$ is the proportion of Circuit Court cases with a progressive outcome and $M_{c(t-n)}$ is the number of cases in Circuit c and time $t-n$.

In principle, we have 612 experiments (across 51 years and 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 typically 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;
- a vector of observable unit characteristics, X_{ict}, depending on the unit being observed (for example, (a) at the individual level: age and indicators for gender, educational attainment, and race; (b) at the state level: unemployment rate, per capita real income, police employment, the nonwhite proportion of population, percent urban, infant mortality, and the age profile of the population);
- and time-varying Circuit-level controls, $W_{c(t-n)}$, such as the characteristics of the pool of judges available to be assigned in Circuit c and time $t-n$ and lagged community standards since the *Miller* standard requires judges to follow community standards in making their decisions. We define community standards using an index of views on the effects of pornography. Because the GSS is sometimes biannual, 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.

Our main specification clusters standard errors at the Circuit level. Our results are robust to using wild bootstrap (Cameron, Gelbach, and Miller 2008), clustering standard errors at the Circuit-year level, and simulations that randomly assign legal variation to another Circuit. 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 amid transition from one steady-state to another. On the other hand, the effects of law may fade or the resolution of Circuit splits or spillover effects across Circuits may cause outcomes to converge.

Typically $Law_{c(t-n)}$ is 1 (100% progressive) or 0 (100% conservative) and there is less than

one free speech decision per Circuit per year. We define $Law_{c(t-n)}$ to be 0 when there is no case and add a dummy $\mathbf{1}[M_{c(t-n)} > 0]$. We weigh by the number of cases (weights are the geometric mean of $M_{c(t-n)}+1$ over the previous four years) in robustness checks and finds that estimates become more precise. We report $\frac{\sum_n \beta_{1n}}{n}$ and joint significance of the lag coefficients in the paper and the individual coefficients and wild bootstrap results in the appendix. As customary in distributed lag specifications, we rely on joint tests of significance of the individual lags (n= 0 up to 4) to determine whether the law has a statistically significant impact. We consider n= 0 as a lag because some statistics refer to calendar year. Most of the effects appear with some slight delay so excluding n= 0 in joint significance tests does not matter. We also report the joint significance of the leads (n=−1 up to −4) to allay concerns of spurious significance. We sometimes focus on $n = 0$ or suppress the time subscript of β for expositional purposes.

3.3.1 Instrumental Variable The structural specification may be biased due to omitted variables. If free speech decisions and population outcomes are systematically correlated for unobserved reasons, then β_1 may be too large or too small. 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, which suggests social trends may drive judicial decisions. 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. Therefore, ascertaining a causal effect from judicial decisions to social trends is difficult without idiosyncratic variation in judicial decisions.

We develop an instrumental variable, N_{ct}/M_{ct}, using judges' biographical characteristics. The rise of the religious right movement in the U.S. for a large part of the twentieth century means that Democrats would be associated with socially progressive views on matters of free speech (Chen and Lind 2007, 2014). We will refer to the number of Democratic judges assigned to free speech cases as N_{ct}. 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.

Consider the deviation from expectation, $N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct})$.$\mathbf{E}(N_{ct}/M_{ct})$ is the expected proportion of judges who tend to be progressive in free speech cases. If we use $N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct})$ as the instrument, the moment condition for causal inference is $\mathbf{E}[(N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct}))\varepsilon_{ict}] = 0$. Figure 5 illustrates this identification strategy. Figure 5A displays N_{ct}/M_{ct} in a jagged line. 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. When the jagged line is disconnected, this means there is a year without a case.

Because there are years without cases, 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:

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

Observe that we can rewrite the moment condition for the new instrument as: $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{Pr}[M_{ct} > 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} > 0] + \mathbf{Pr}[M_{ct} = 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} = 0] = \mathbf{Pr}[M_{ct} > 0] * 0 + \mathbf{Pr}[M_{ct} = 0] * 0 = 0$. Notice where the original moment condition is substituted with 0, so the new moment condition is implied by the old one. 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 will estimate how outcomes Y_{ict} respond to idiosyncratic variation in N_{ct}/M_{ct}. Although this discussion focused on $n = 0$ for expositional purposes, the precise collection of instruments is $p_{c(t-n)}$ for n= -1 up to 4.

3.3.2 Counterfactuals Dummying for the presence of a case also permits the identification of additional counterfactuals.β_1 captures the effect of progressive precedent where the counterfactual is a conservative precedent, $\beta_1 + \beta_2$ captures the effect of progressive precedent

where the counterfactual is no precedent, and β_2 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}; and the bias is more severe for more distant lags while being non-existent for the most advanced lead. We assess whether this potential endogeneity is a significant concern by comparing β_1 when we instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. The demographic characteristics of District judge are correlated with whether the judge is reversed by Circuit Courts (Haire, Songer, and Lindquist 2003; Sen 2015; Barondes 2010; Steinbuch 2009), so expected reversal rates could encourage litigants to pursue an appeal. If $\mathbf{1}[M_{ct} > 0]$ and Law_{ct} are both identified, estimates should be roughly invariant to the inclusion or exclusion of additional lags and leads. Including lags that are important predictors of the outcome improves statistical precision, but losing data at the beginning and end of the dataset reduces precision. A test of the null hypothesis of lead coefficients being 0 provides an omnibus check of our instrumental variable being exogenous 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 β_1.

To instrument for $\mathbf{1}[M_{ct} > 0]$, we define our District IV in two ways. In the first definition, $w_{ct} = \frac{K_{1t} * \left(\frac{L_{1t}}{K_{1t}}\right) + ... + K_{6t} * \left(\frac{L_{6t}}{K_{6t}}\right)}{K_{1t} + ... + K_{6t}}$, where K_{it} denotes the number of cases filed and L_{it} denotes the number of assigned judges with a particular biographical characteristic in District *Court* i and time t. For expositional purposes, let i go to a maximum of 6. In 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* District *Court*. The second definition addresses two issues: the location of free speech controversies, K_{it}, may be endogenous and the presence of a case in a district courthouse is not guaranteed. 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. To see why, note that the

Law of Iterated Expectations (LIE) implies $E\left(K_i * \left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\right) = 0$. 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: $E\left[\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct}\middle| K_i\right] = E\left[E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right.\right.\right.$ Rearranging once again yields: $E\left[\epsilon_{ct} E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right)\middle|\epsilon_{ct}, K_i\right)\middle| K_i\right]$. 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, $E\left(\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right)\middle|\epsilon_{ct}, K_i\right) = 0$. Defining the instrument as the weighted sum—rather than the collection—of $\frac{L_{it}}{K_{it}} - E\left(\frac{L_{it}}{K_{it}}\right)$ lessens the $K_{it} = 0$ problem because it increases the chances of observing at least one district case in every Circuit-year.

Merging in courthouse information, which we tried to link in via docket number from the Administrative Office of the U.S. Courts (AOC) database on all cases filed, is not possible for many cases, which results in $\tilde{w}_{ct}$ being undefined in over 50% of Circuit-years. This forces us to include a dummy for missing values in $\tilde{w}_{ct}$ and define $\tilde{w}_{ct}$ to be 0 when it would otherwise be missing and re-introduces the endogeneity problem of presence of a case, this time at the District level. Our main tables use w_{ct} to avoid this problem. We view this as a provisional solution. Another solution is to impose an additional identification assumption—progressive and conservative decisions have opposite effects of equal size in absolute value—and define $Law_{c(t-n)}$ as the average of -1/0/+1 (progressive/no precedent/conservative decisions), which we do in additional robustness checks. This identification assumption allows omitting $\mathbf{1}[M_{ct} > 0]$ and the need to instrument for it altogether.

3.3.3 Many Instruments and Many Endogenous Variables A large number of biographical characteristics serve as valid instruments, which would result in a weak instruments problem if we used them all. The biographical database that we construct has thirty characteristics 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). Then, 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) yield a total of several thousand possible instruments.

There are two ways to reduce dimensionality: a priori theory and model selection. LASSO (least absolute shrinkage and selection operator) (Belloni et al. 2012) is commonly used for model selection. 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.

We find that characteristics related to religion, political party, and having attended non-elite schools are important in predicting free speech decisions. This is 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. All results are reported using LIML (limited information maximum likelihood) to minimize the concerns from weak instruments. The Appendix errs on showing more results for transparency rather than fewer to aid this development. The 2SLS estimates using LASSO and Democrat identification typically remain similar in sign and significance. However, LASSO yields smaller coefficients as well as smaller p-values, which is consistent with sparse methods providing greater precision.

We also conduct a "visual Hausman" test where we display the 2SLS estimates using the top 50 instruments that are strongest in terms of the first stage F-statistics from Circuit-year level regressions. These figures indicate that alternative sources of variation yield similar magnitudes. In a handful of cases, the magnitudes are much larger, which we omit in the

graph for readability (i.e., we winsorize the largest and smallest four estimates). These outliers may be due to the rarity of some instruments as some biographical combinations are quite rare. The fact that our 2SLS estimates are in the middle of the distribution of alternative 2SLS estimates reduces the concerns that of spuriously large 2SLS estimates when using instruments (like assignment of Democrats) that have a wide distribution of support or when using combinations of instruments (like LASSO).

At present, the literature on LASSO-IV is evolving, so future econometric developments may add precision. We report one exploration in the direction of greater precision. With many endogenous variables and many instruments, there is a danger of overfitting with instruments from the wrong year. We use the contemporaneous instruments to predict $Law_{c(t)}$ and $\mathbf{1}[M_{c(t)} > 0]$ and use the fitted values as instrumental variables in robustness checks. To see why "separate first stages" instrumentation works, suppose: $Y_{ict} = \beta_{10} Law_{c(t)} + \beta_{11} Law_{c(t-1)} + ... + \varepsilon_{ict}$. Let the first stage be: $L_{c(t)} = Z_0\Pi_0 + u_0$ and $L_{c(t-1)} = Z_1\Pi_1 + u_1$, where $Z_0 = \left[\begin{array}{c} p_{c(t)} \end{array} \right]$ and $Z_1 = \left[\begin{array}{c} p_{c(t-1)} \end{array} \right]$. Set $\hat{X} = [\ \hat{L}_{c(t)} \quad \hat{L}_{c(t-1)} \quad ... \quad \hat{L}_{c(t-j)}\ ]$ 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 $\sqrt{n}(\hat{\beta} - \beta) \to N(0, V), V = Q^{-1}\Gamma\Phi\Gamma Q^{-1}$.

3.3.4 Exogeneity and Exclusion Restriction

Randomization: Circuit Courts A few scholars argue that certain Circuits have not used random assignment (Hall 2010), so we consider three tests of this assumption. First, one of the authors, in Chen and Sethi (2011),[36] surveyed a number of courts of appeal and evaluated measures taken by them to ensure that the assignment of judges to panels is random. In one court, two to three weeks before the oral argument, a computer program is used to randomly assign available judges, including any visiting judges, to panels that will hear cases. The program used is an in-house creation. There is a mechanism in the program that ensures the same judges are not sitting together on panels. This is also checked manually, although the clerk could not remember ever having manually to change judicial assignments for this

[36]The extended discussion is repeated here for clarity.

reason. There is no specialization among judges; the cases are "all over the map" in regard to subject matter. Senior judges tell the clerk how often they are willing to sit and hear cases, and they are added to the program for randomized assignment in accordance with their schedules. There is an administrative office that sets the baseline number of cases senior judges must hear per term.

In another court, random assignment of panels occurs before the random assignment of cases. Panels of judges are organized to hear cases on a yearly basis, randomly assigned together by computer program and given dates for hearings. There are "holes" left in some of the panels by the program, and visiting judges are plugged into those spots by the chief judge. This program also ensures that the same judges are not seated together repeatedly. Thus, the judges know at the beginning of the year which days they will be hearing cases and the compositions of the panels on which they will sit.

Once all the briefing is completed, a case is put into a pool of cases "ready to calendar." If a panel of judges has previously looked at a case, it will be sent back to them (for example, if it was remanded to resolve one issue). Otherwise, a different program randomly assigns cases to these pre-established panels and dates. About eight weeks before the scheduled argument, a preliminary calendar is sent out and the judges review it for recusal. If a judge must recuse himself, the case is taken off the calendar and placed back in the pool for reassignment. Senior judges decide how many days and which months they will work, and this information is entered into the program for random assignment. Before the advent of computer programs, one judge did all of the panel assignments by hand, and the clerks randomly assigned the cases by hand.

Other variations from random assignment include: en banc cases that are heard by the entire pool of judges (or a significant fraction in Circuit 9). We do not use these cases, which are also relatively infrequent. Judges can also take sick leave or go on vacation, but this is determined far in advance. Not accounting for vacation, sick leave, senior status, en banc, remand, and recusal can lead to the inference that judges are not randomly assigned. Our identification strategy assumes that these kinds of deviations from random assignment are ignorable. Even a gold-standard random process — the roll of a die — has a deterministic

element. If known with precision, the force and torque applied to the die, the subtle air currents, the hardness of the surface, etc., might allow us (or a physicist) to determine with certainty the outcome of these "random" rolls. Despite this obvious non-randomness, we would still have faith in the outcome of a trial with treatment assignments based on die rolls because we are certain that the factors affecting the assignment have no impact on the outcome of interest and hence are ignorable.

As a second randomization check, Chen and Sethi (2011) formally tests for randomization by showing that 19 case characteristics as determined by District Courts are not correlated with the characteristics of the assigned Circuit judges assigned in 415 gender discrimination cases. As a third randomization check, we examine whether the sequence of judge assignment is like a random process, which we detail after explaining why the check is necessary.

Omnibus Tests: Circuit Courts Because our data comprise published opinions, several additional issues need to be considered: settlement, publication, and strategic use of citation. Some scholars argue that the decision not to publish is a compromise among judges who disagree about the correct outcome (Law 2005; Wald 1999). Our response to the question of publication is twofold. First, unpublished cases are not supposed to have precedential value. Second, 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). Therefore, even were we to have the unpublished cases, the judge identity would not predict the decision in unpublished cases (and the decision should not have an impact, being unreported and lacking precedent), so the Local Average Treatment Effect of our estimates would be the same.

Regarding settlement, 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. In one empirical study, the earlier announcement of judges assigned to cases in the D.C. Circuit did not affect settlement rates (Jordan 2007).

We cannot rule out strategic use of keywords or citation of Supreme Court precedent, so we

propose an omnibus test to collectively address all of these deviations from strict exogeneity—We examine how similar the string of actual panel assignments is to a random string (Chen 2013). 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 as their assignment would be positively autocorrelated.

Figure 5 suggests visually that panel composition is not serially correlated. We formally investigate this by:

1. Proposing a statistic that can be computed from the sequence of numbers of Democrats per seat within a Circuit.
2. Computing the statistic for the actual sequence, s^*.
3. Computing the statistic for each of 1,000 bootstrap samples from the actual sequence, i.e., s_1, s_2, s_3 . . .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. Computing the empirical p-value, p_i by determining where s^* fits into s_1, s_2, s_3 . . .s_n.
5. Repeating steps 1-4 and calculate p_i for each unit.

We use the following statistics:

Autocorrelation: We see if the value in the j$^{\text{th}}$ 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).

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

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.

With a truly random process, the collection of all unit p-values should be uniformly distributed. The 1001^{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 approach the CDF of a uniform distribution. Figure 6 presents each Circuit as one dot. Table II shows that the Kolmogorov-Smirnov test statistic cannot reject the distribution of p-values is different from the uniform.

Future data availability may allow direct assessment of publication, settlement, and strategic keyword/citation issues. The U.S. government would have to allow the judge identity to be merged into the AOC database and provide finer case categories. At present, the AOC database contains all cases filed, but the judge identities are scrubbed from the codebook and the numerical identifier for judges have been deleted from the dataset. Even if AOC could be merged with PACER data for judge identity, free speech regulations of obscenity is not one of the AOC three-digit case categories.

Randomization: District Courts District Courts assign one judge to a case randomly or rotationally (Taha 2009; Bird 1975).[37] For example, one District told us that random assignment occurs within 24 hours of a case filing, which is handled in the order of its arrival.Waldfogel (1995) reports that one District Court uses three separate randomization

[37] Cases being returned on remand from the Circuit court are not randomly assigned. We do not use remanded cases in our dataset.

wheels and each wheel corresponds to the anticipated case length. Senior judges can elect not to be assigned to certain wheels. Another District Court uses, instead of wheels, thirteen computer generated decks of cards–one deck for each case category and an identical number of cards (two or five) for each active judge.[38] The decks refill when the majority of the deck has been exhausted. Senior judges can request to be assigned to certain decks. Even within a deck, senior judges can ex ante request a "bye" for specialized case types. Within each District Court are several courthouses (also referred to as Divisions). The appropriate Division is determined by where the parties are located and where the cause of action arose. Some Divisions get their own deck of cards.Taha (2009) reports that in 29 Districts, a case may be assigned to any judge in that District, while in the others, the cases are assigned to a geographic Division within the District and randomly assigned to one of the judges in that Division. We confirm the method of random assignment by contacting all of the District Courts.

The ideal construction of $\tilde{w}_{ct}$ takes a weighted sum across wheels of deviation from expectations, $E\left(\frac{L_{it}}{K_{it}}\right)$, separately for senior and non-senior judges. Since $E\left(\frac{L_{it}}{K_{it}}\right)$ is uncomputable for senior judges (we would need to know the senior "byes" in every District courthouse), but may be endogenous, we drop senior District judges for $\tilde{w}_{ct}$; for similar reasons, we also drop visiting judges (since judges routinely visit other courts to assist with caseload) and magistrate judges (who assist District Court judges but do not have life tenure and we do not have their biographical data). Non-ideological cases are referred to magistrate judges (Nash 2015), so omitting them will not matter. Dropping these judges result in less than 10% sample loss. Identification is unaffected by dropping judges even if they are in the same wheel.

Some courts spin separate random wheels for District judges and for magistrate judges. In some Districts, parties can decline assignment to a magistrate judge within a certain time period and request another random draw. This will not affect identification because it happens before the random assignment that we use. In some Districts, when the federal government is a litigant on the case, the U.S. attorney can pick the wheel. In sum, conditional

[38]http://www.mnd.uscourts.gov/cmecf/Order-for-Assignment-of-Cases.pdf

on case type, there is random assignment at the court or courthouse level, and we must only calculate the yearly expected composition of judges in District courthouses. As stated before, we are unable to merge enough courthouse information for this legal topic, so we only use $\tilde{w}_{ct}$ in robustness checks. Moreover, in simulations, measurement error in calculations of expectations can lead to large bias when these expectations are themselves correlated with social trends. Measurement error in expectations can arise if the econometrician, for example, does not know the amount of time it takes for a new judge to be assigned a full caseload or if the econometrician misidentifies who is a visiting judge.

Related cases (meaning cases where one decision will substantially resolve all cases) may also be consolidated if filed within a few weeks.Waldfogel (1995) reports that plaintiffs can argue the case is related to another pending case and, if the judge agrees, the cases will be consolidated. A clerk reported 8% of filed cases were accepted as related in 1991 in SDNY. In another District Court, if a clerk identifies and two judges agree that a new civil case is related to another open civil case, they will be consolidated in the interests of justice or judicial economy. The clerk brings the possible connection to the attention of the judge of the new case, who then confers with the judge of the earlier case to determine whether they are in fact related cases. Consolidation would only occur for relatively high-frequency case types, which does not include free speech. We assume the decisions about case relatedness occur in a manner plausibly exogenous to judge assignment for the handful of District cases that do overlap such that they are consolidated.

Omnibus Tests: District Courts District Courts judges are revealed much earlier. Ideally, we would use docket filings in the Administrative Office of the U.S. Courts pertaining to free speech. Judges are omitted for most cases prior to 2000, so we must use published District opinions to construct our District IV. We buttress the assumption that settlement, publication, and strategic use of keywords or citations are plausibly exogenous: First, in District Courts, judges are much more constrained and ideology has been found to play hardly any role. Judicial ideology does not predict settlement rates (Ashenfelter et al. 1995; Nielsen et al. 2010), settlement fees (Fitzpatrick 2010), publication choice (Taha 2004), or

decisions in published or unpublished cases (Keele et al. 2009). This finding is consistent with the District judge identity only affecting outcomes through the presence of an appeal and not through the District Court decision. Second, we examine these issues directly. The random strings test is ineffective because some Districts use rotational assignment or random drawing of judges from card decks without replacement. So, we test whether District Court judicial biographical characteristics in *filed* cases jointly predict publication. We link PACER filing data, which has judge identity, to AOC data, which has information on publication. We obtained all freely available PACER (Public Access to Court Electronic Records) data on District cases from 32 districts for 1980 to 2008 for a total of 359,595 non-duplicated cases. This data contains the name of the District where the case was filed, the filing and termination date (missing for 10% of cases), the assigned docket number, and the name of the District or magistrate judge presiding on the case. We merge the names of the judges into the Administrative Office of the U.S. Courts (AOC) database. We use LASSO to select biographical characteristics and no characteristic was chosen. We assume that remaining deviations from random assignment (like vacation days) are ignorable.

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. 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.

3.4 Interpretation of Magnitudes and Channels

3.4.1 Local Average Treatment Effect In common law, hard cases precede easy cases. 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' biographical characteristics may influence decisions. Ambiguity has been shown to cause polarization along partisan lines (Baliga et al. 2013) so

even when judges intend to be fair, misperceptions of what is fair can significantly affect case outcomes (Chen et al. 2015c). These hard cases are also the ones where judges likely seek guidance. The common practice is to construct policy arguments (Posner 1998b; Breyer 2006; Abramowicz et al. 2011) as previously there was no way to empirically evaluate Circuit judges' decisions. Our 2SLS estimates capture the effects of hard cases, where biographical characteristics affect decisions, but these are also the very cases with ambiguity and where judges seek guidance. If there was strong legal precedent, then the judge simply follows the rule. Indeed, despite 70% of cases having both Democratic and Republican judges, only 8% of cases have dissents, suggesting that judges do generally agree on what is the right decision based on past precedent (Berdejó and Chen 2014). In the Local Average Treatment Effect framework, this means that compliers (i.e., the hard cases) precede the always-takers and never-takers (i.e., the easy cases).

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 most settings, we only know $LATE$ and not TOT. Recall, $TOT \equiv E[Y_{1i} - Y_{0i}|R_i = 1] = E[Y_{1i} - Y_{0i}|R_{1i} > R_{0i}]Pr(R_{1i} > R_{0i}|R_i = 1) + E[Y_{1i} - Y_{0i}|R_i = 1]Pr(R_{1i} = R_{0i} = 1|R_i = 1)$, 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. 2SLS estimates of β_{10} measure the effect of hard cases at $t = 0$. These are the complier cases whose decisions are affected by judicial biography.β_{1n} captures hard cases n years ago; their subsequent effects at $t = 0$ can be decomposed into delayed direct effects and to subsequent easy cases that cite these hard cases. These subsequent easy cases are the always-takers and never-takers for any $t > -n$. Thus, $\sum_{n=0}^{\infty} \beta_{1n} = \sum_{n=0}^{\infty} TOT_{ct}^{n} = \sum_{n=0}^{\infty} LATE_{ct}^{n}$.

Table IV provides indirect inference revealing that the effect of hard free speech cases is largely through cases subsequently *not* litigated and published in Circuit Courts. That is, through *stare decisis*, subsequent cases in lower courts simply follow the legal rule or are never brought into courts in the first place (or are deemed unworthy of publishing because they do not present a new legal issue). Few cases occur per Circuit-year. Table IV shows that 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. Theoretically, litigants should settle the easy cases, and even if they do not, judges should leave easy cases unpublished. Since published cases are predominantly hard cases, their decisions correlate with biographical characteristics. In addition, the strong correlation with biographical characteristics suggests that any bias that results from the presence of non-compliers is likely to be small. The bias from non-monotonicity is given by $\frac{Pr[Defier]}{Pr[Complier]-Pr[Defier]}(\beta^{Complier} - \beta^{Defier})$, which is small when the magnitude of the first stage is large (to see this, observe that the denominator is more likely to be large and the numerator small when the first stage is large).

3.4.2 Comparison Between Experimental vs. Population-Based Analyses and Indirect Channels Court-made laws can have direct and indirect effects that are difficult to completely catalog. For example, laws can influence the population even though any individual person need not be aware of the law nor the channel through which the law eventually affects him or herself. Moralizing language can induce individuals to change their behavior (Sunstein 1996; Kahan 1997) because of pressure brought to bear upon them through societal sanction that differs from the official sanction imposed by the law (Anderson and Pildes 2000).

Data limitations make it practically impossible to study all the channels through which law has its effects, but we can begin to elucidate these channels by comparing experimental and population-based analyses. Since we do not know how many people in the population are directly or indirectly exposed to free speech decisions, we can only provide an equation. Observe that the population analysis of a single lag will estimate $LATE$, 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. The unknown parameters in the equation are TOT_{indirect} and P(individual exposure in treated circuit). Note that the individual need not be directly exposed; indirect exposure in the form of expressive externalities may be large.

For example, 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.Bailey (2010) documents the quick response of states to progressive Supreme Court obscenity precedent. Cities rewrite their ordinances after court decisions.[39] Changes in a locality's enforcement of existing regulations relating to obscene or licentious conduct may also 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 even if they do not mention the court decision explicitly. Court decisions influence preferences among people in the community where the case originates (Hoekstra 2003).

To buttress the plausibility that Circuit decisions could eventually reach community leaders 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.[40] Figure 3 displays a plot comparing the number of free speech decisions and the number of newspaper articles about obscenity decisions from 1979 to 2008. However, not every newspaper is available for every year, so we divide the number of newspaper articles by the proportion of newspapers available. For example, if only half of the typical newspaper coverage is available because of data limitations, we would multiply by a factor of two to make a consistent series in the figure. This allows us to compare graphically the number of Circuit decisions and newspaper articles about obscenity over time. We find a positive correlation that is statistically significant at the 10% level even with the inclusion of Circuit- and year-fixed effects. We lack newspaper data before 1979, but the salience of free speech law was potentially even greater during this time period. Heightened salience of obscenity law is suggested by the large number of law review articles written in response to obscenity

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

[40]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".

decisions during the 1960s (Kalven 1960, Magrath 1966, Lockhart 1960).

Information need not transmit directly from the Circuit Court decision itself.Chen and Yeh (2014b) verifies 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. We further assess *stare decisis* by reading the District cases that cite the Circuit cases, verifying that District Court cases do follow Circuit precedent. We also quantitatively assess *stare decisis.* Several empirical challenges make it difficult to examine whether law creates precedent. First, law is rarely randomly decided, so social trends may drive both the law and subsequent decisions. Second, cases in courts are endogenously selected based on legal standard. Chen et al. (2014a) examines all District cases on a legal topic filed before the Circuit Court decision but resolved after the Circuit decision in that legal topic. Such a methodology requires a legal topic that appears with relatively high frequency (e.g., piercing corporate veil cases). Then, using the random assignment of judges setting precedent along with all relevant cases filed in District Courts holding fixed the selection issue, we can quantitatively verify *stare decisis.*[41]

Finally, we illustrate the outsized features of the federal judiciary with a contemporary example in another legal area–the right to have an abortion (Chen, Levonyan, and Yeh 2014c). A Mississippi statute would have shut down its sole abortion clinic by requiring its doctors to obtain admitting privileges at local hospitals, but the Fifth Circuit required that the statute not be implemented. However, the same Circuit Court upheld a Texas law requiring these admitting privileges, which resulted in one-third of abortion clinics in Texas shutting down, forcing some women to drive more than 100 miles to obtain an abortion. A new Texas statute requires abortion clinics to meet the building standards of ambulatory surgery centers; as the time of this writing, the Court will decide whether to invalidate the

[41] Further evidence of the information transmission channel of Circuit Court decisions include the market response to Circuit Court decisions (Araiza et al. 2014).

new statute. If upheld, this statute would reduce the number of centers operating in the state to fewer than 10.[42] These examples illustrate how Circuit Courts greatly influence matters of constitutional interpretation. In sum, we can feel reasonably confident that states and District Courts are predominantly following the precedent of Circuit Courts that contain them.

3.5 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 since 1957 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 (Songer and Haire 1992). The pattern also 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 (Sunstein et al. 2006).

Table III Panel A shows that Democrats are 10% more likely to vote progressively in free speech cases.[43] Panel B shows that an additional Democrat per seat on a three-judge panel increases the chances of a progressive decision by 26%. At both the judge level and the case level, point estimates and statistical significance remain stable or increase with controls for Circuit, year, and the expected proportion of Democrats per seat. Panel C shows that an additional Democrat per seat increases the proportion of progressive decisions by 36% at the Circuit-year level.[44] The F-statistic is 6.7 and increases with the inclusion of controls up to 10.4.

[42] http://www.nytimes.com/2014/07/30/us/mississippi-abortion-clinic-Federal-court-blocks-closing.html

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

[44] 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.

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

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.

Panels D and E repeat the investigation at the level of analysis, i.e., the individual data merged with laws. The estimates are slightly different because of the differing numbers of individuals per Circuit. The joint F statistic on the instrument is past the conventional threshold for weak instruments (Stock and Yogo 2005) and is betwen 12 and 13, and the F statistics again increase with the inclusion of fixed effects and additional Circuit-year controls. Column 6 of Panels D and E show that the slope of the first stage increases in recent years; this finding is consistent with increasing polarization in the U.S. along social issues and political party (Chen and Lind 2014). Finally, Table IV displays a falsification of the instrumental variables, which indicates that legal decisions are not related to the assignment of Democrats per seat in the one or two years before and after the true instrument.

To check whether the 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

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.

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 exogenous increases in Democrats assigned to cases corresponds to more progressive free speech decisions. Figure 7B shows that there is no relationship between expected number of Democrats per seat and the proportion of progressive free speech decisions.

The instruments chosen by the LASSO procedure are characteristics related to religion, political party, and having attended non-elite schools. We verify that the instrument choices do not vary much with the inclusion or exclusion of controls, though we should expect some variation because the number of observations per Circuit-year and the number of years of data varies across outcomes. 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. Given the large number of outcomes, we suppressed the reporting of LASSO-selected instruments, but these are available on request.

4 The Impact of Free Speech Laws

4.1 Attitudes Table V reports the effects of free speech precedent on sexual attitudes. Column 1 presents OLS estimates. Column 2 presents 2SLS estimates of the causal impact of law using variation from the assignment of Democrat judges. Column 3 presents 2SLS estimates using variation from the assignment of Democrat judges at the Circuit level to instrument for law and variation in assignment of District judges to instrument for the presence of a case. Column 4 presents 2SLS estimates using variation from LASSO-selected instruments at both the Circuit and District level. To streamline presentation, the table only reports the average lag effect ($\frac{\sum_n \beta_{1n}}{n}$), but the joint significance of lags and joint significance of leads are reported. Appendix Table II reports the full set of coefficients along with additional statistics of interest, such as the wild bootstrap of the LASSO specification. Results from the wild bootstrap corroborate the validity of our main estimates even though there are only 12 Circuit clusters.

The OLS estimates may be biased upwards because judges make progressive decisions when sexual mores are progressive or OLS may be biased downwards because judges make

conservative decisions when sexual mores are perceived as too progressive. Comparing OLS to our preferred specification in the LASSO column reveals that OLS estimates are generally smaller than the IV estimates. This is consistent with OLS being downwards biased due to courts ruling in a manner that they articulate: They make more conservative decisions when they perceive harms from secondary effects and progressive sexual mores to be high. Appendix Table II reports that progressive free speech decisions are less likely when the previous year's attitudes towards premarital sex were more progressive (Column 6). That is, when people are more likely to view premarital sex as never wrong or wrong only sometimes, judicial panels are more likely to make a conservative obscenity precedent. This potential endogeneity of free speech precedent confirms the need for an empirical strategy relying on exogenous variation in precedent.

Comparing OLS estimates to the Appellate IV estimates reveals that the signs of OLS and 2SLS estimates are not always the same; later we will also show that the signs of OLS estimates and LASSO IV estimates can differ. This suggests that the 2SLS estimates are not simply spurious magnifications of OLS due to the many/weak instruments problem. Our preferred specification in Column 4 reports that the average lag effect is jointly significant and estimated to be 0.008. This means 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.

We examine to what extent our empirical framework approximates a randomized control trial through a series of robustness checks. Even with random assignment of judges, there are some concerns regarding publication, settlement, and use of keywords that could lead to spurious correlation even if the assignment of judges passes the omnibus test described above. In addition, the magnitudes may be too large due to weak instruments. Our response to this

TABLE V

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.

has two parts. First, we assess the leads and the sensitivity to varying the lag structure. Second, we vary the covariates and the choice of instrument. Table V shows that the lead coefficients are never statistically significant in the IV columns. The final row of Appendix Table III shows that a specification with 4 *leads* and 1 lag yields no jointly significant effects of the leads and a borderline significant point estimate for the lag that is between the LASSO IV and Appellate IV estimates. The final row of Appendix Table IV shows that the lead coefficients are smaller than the lag coefficients, while the standard errors are similar in magnitude to the standard errors of the lag effects. These results ameliorate concerns regarding endogeneity of the instrument as well as spurious magnitudes.

The estimates are also robust to varying the lag structure. The bottom of Appendix Table III shows that the average lag effect and the joint significance of the lags are very robust to adding a lead or a lag. Appendix Table IV shows that the point estimates of individual lags are also robust to this variation. Results are less statistically significant with only 1 or 2 lags, possibly because the jointly significant effects occur with some delay or because of data limitations. Other sections of Appendix Table III show that dropping one Circuit at a time and the inclusion or exclusion of covariates (Circuit-specific time trends, Circuit- and

year-fixed effects, individual-level controls, survey weights, community standards) usually do not affect the estimates and the joint significance of the lags. In particular, the most parsimonious specification that drops all controls except the presence of a case yields similar inferences.

While intuition motivates the use of Democrat vs. Republican assignment to identify free speech precedent, our estimates are robust when using alternative sets of instruments from the LASSO procedure. Moreover, we show in Appendix D that the estimates using any of the top 50 instruments in terms of F-statistic strength yields point estimates near what we report in Table V. Each red dot represents the average effect size from an alternative instrument. The yellow line indicates the estimate from the Democrat IV. For interpretability, we only present estimates that use only 1 biographical instrument at a time. Even though the lag effects on attitudes towards homosexual sex and extramarital sex are jointly significant when using only the Democrat instrument, the main purpose of these graphs is to assess whether the magnitude is unusually large and due to random chance. The figures show that most of the average impacts on attitudes towards homosexual sex using other instruments are positive; and the average impacts on attitudes towards extramarital sex are often larger when using alternative instruments. LASSO IV estimates tend to be smaller than the 2SLS estimates from the Democrat instrument. This suggests that the preferred estimates using LASSO are unlikely to be spuriously large simply due to the selection of unusual instruments.

Finally, Appendix Table II assesses whether this potential endogeneity of presence of a case is a significant concern. Comparing Columns 4 and 5, 9 and 10, and 14 and 15 show that instrumenting for the presence of a case hardly affects β_1. In additional unreported robustness checks, we construct the instrument as deviations from expectation, drop judges such as those who took senior status and those who are visiting from other courts as 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.

4.2 Sexual Behavior We next turn to tangible manifestations of the shift in attitudes. Table VI reports that sexual behavior becomes more progressive 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. 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 perhaps because they are less likely to enter early marriage.

We subject these estimates to the same battery of robustness checks. Appendix Tables V to VII report the full set of coefficients and wild bootstrap results. The final row of Appendix Table VIII shows that a specification with 4 *leads* and 1 lag yields no jointly significant effects of the leads. The final row of Appendix Table IX shows that the lead coefficients are slightly smaller than the lag coefficients, while the standard errors are similar in magnitude to the standard errors of the lag effects. The estimates are also robust to varying the lag structure. The bottom of Appendix Table VIII shows that the average lag effect and the joint significance of the lags are very robust to adding a lead or a lag. Appendix Table IX shows that the point estimates of individual lags are also robust to this variation. Results are less statistically significant with only 1 or 2 lags, possibly because the jointly significant effects occur with some delay or because of data limitations. Other sections of Appendix Table VIII show that dropping one Circuit at a time and the inclusion or exclusion of covariates usually do not affect the estimates and the joint significance of the lags. In particular, the most parsimonious specification that drops all controls except the presence of a case yields similar inferences but an average lag effect smaller than 0.05 percentage points. The last point estimate may be more reasonable given the mean dependent variable is 0.3%.

Our estimates are robust when using alternative sets of instruments from the LASSO

procedure. The average lag effect on paid sex is stable across choice of instruments in Table VI and Appendix Table V. Appendix D reports estimates using any of the top 50 instruments in terms of F-statistic strength. Point estimates are near what we report in Table VI and often of the same sign, which again suggests that the preferred estimates using LASSO are unlikely to be spuriously large due to the selection of unusual instruments. Appendix Tables V to VII also report that when LASSO IV is used, β_1 is quite stable whether or not presence of a case is instrumented for.

4.3 Crime Sexual crimes are among the secondary effects of free speech law that has concerned advocates and policy-makers. Table VII 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.[45] Previous results without random variation found a decrease in sex crimes after the government relaxed restrictions on explicit materials (Ben-Veniste 1971), which is consistent with the explanation for why OLS and IV differs: Governments make progressive decisions when secondary effects are less of a concern. Appendix Tables X to XII report more details and the full set of coefficients.

Table VII shows that the lead coefficients are statistically significant in only one IV model out of 15. The final row of Appendix Table XIII shows that a specification with 4 *leads* and 1 lag yields no jointly significant effects of the leads. The final row of Appendix Table XIV shows that the lead coefficients are smaller than the lag coefficients, while the standard errors are larger in magnitude to the standard errors of the lag effects. The bottom of Appendix Table XIII shows that the average lag effect and the joint significance of the lags are very robust to adding a lead or a lag. Appendix Table XIV shows that the point estimates of individual lags are also robust to this variation. Lag effects are jointly significant with as few

[45] 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 VI

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.

as two lags. Other sections of Appendix Table XIII and XIV show that dropping one Circuit at a time and the inclusion or exclusion of covariates usually do not affect the estimates and the joint significance of the lags, which are far more significant than the leads. The models with the largest point estimates tend to have lag effects that are not jointly significant. Appendix Tables X to XII report that when LASSO IV is used, β_1 is quite stable regardless of instrumenting for presence of a case. Taken together, these results reduce concerns regarding endogeneity of the instrument, endogeneity of presence of a case, and spurious magnitudes.

Our estimates are robust when using alternative sets of instruments from the LASSO procedure. Appendix D shows a set of "visual Hausman" tests that reveal strong patterns on the crimes included in the secondary effects that worry judges and reveal no effect on property crime. Progressive free speech precedent increases prostitution and drug violations. The 2SLS estimate from the Democrat IV is smaller than many of the alternative 2SLS estimates. Progressive free speech precedent decreases offenses against family. The graph of property crime estimates show a distribution of 2SLS estimates that are uniformly distributed over a wide range of support including both positive and negative values. These results suggest that the estimates in Table VII are unlikely to be spuriously large due to the selection of unusual instruments.

It is important to note the difficulty of interpreting the magnitudes. The usual approach to interpreting the effect of an experiment is to compare against another experiment. Lacking alternative experiments, we might compare the treatment effect with the mean dependent variable. However, the mean dependent variable represents the net sum of a large number of potential experiments or causal effects of socioeconomic factors, but the number and importance of these other factors are unknown. In any event, arrest data may be reflect underlying social values in terms of people's willingness to come forward to report a crime, law enforcement's openness to investigate crimes, or local community leads making people aware of what constitutes a crime. Different norms of policing and crime reporting make arrest data difficult to interpret. They are susceptible to underreporting, particularly by victims in sex-related crimes. In conservative areas, people may be less likely to report rapes. Some of the effects on arrests could reflect changing stigma. However, not all of the

TABLE VII

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.

results are simply about stigma. We show that progressive decisions decreased reports of child abuse, which can be associated with stigma.

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, leading them to be more aggressive in making arrests. On the other hand, it is perhaps equally likely that conservative free speech precedent empowers police to arrest more often. 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.

4.4 Disease Sexually transmitted disease (STD) outcomes are another one of the secondary effects that judges cite in justifying police power to regulate free speech. They also provide a measure to counteract concerns about the reliability of self-reported sexual behaviors. Table VIII reports that progressive free speech precedent increases incidence of chlamydia, but not 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, and is the fastest increasing in recent years among the STDs for which we have data. 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. 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); and even if people practice safer sex, chlamydia incidence could increase.

We find that progressive free speech precedent increases chlamydia incidence by 49 per 100,000, relative to a mean of 208 per 100,000. Data limitations prevent assessing to what extent the increase in chlamydia is due to the increased number of sexual partners, changes in safe sex practices, or sorting. Given the non-effect on 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. 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.

The full set of results are presented in Appendix Table XV. Table VIII shows that the lead coefficients are never statistically significant. The final row of Appendix Table XVI shows that a specification with 4 *leads* and 1 lag yields no jointly significant effects of the leads. The final row of Appendix Table XVII shows that the lead coefficients are smaller than the lag coefficients, while the standard errors are similar in magnitude to the standard errors of the lag effects. These results lessen the concerns regarding endogeneity of the instrument as well as spurious magnitudes.

The estimates are also robust to varying the lag structure. The bottom of Appendix Table XVI shows that the average lag effect and the joint significance of the lags are robust to adding a lead or a lag and are very robust in specifications with as few lags as only 1 lag. Appendix Table XVII shows that the point estimates of individual lags are also robust to this variation. Other parts of Appendix Table XVI show that dropping one Circuit at a time and the inclusion or exclusion of covariates usually do not affect the estimates nor the joint significance of the lags.

Our estimates are also robust when using alternative sets of instruments from the LASSO procedure. Appendix D shows that the estimates using any of the top 50 instruments in terms of F-statistic strength yields point estimates near what we report in Table VIII. The distribution of estimated effects on gonorrhea and syphilis reveals large estimates both positive and negative. However, the distribution of estimated effects on chlamydia are more concentrated and positive. Taken together, these results suggest that the preferred estimates using LASSO are unlikely to be spuriously large due to the selection of unusual instruments.

We investigated all outcomes discussed in this paper in the battery of robustness checks, but due to space constraints, we do not discuss them all.

4.5 Summary and Discussion of Counterfactuals 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 IX summarizes the following parameters for each outcome: β_1, $\beta_1 + \beta_2$, and β_2, scaled by the number of cases per year to report the

TABLE VIII

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.

typical effect per year of free speech precedent. To compute the effect of progressive precedent in a typical Circuit-year, we multiply the coefficient on Law_{ct} by $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$, the typical proportion of decisions that are progressive when there are Circuit cases, and by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of Circuit-years with a Circuit case. A similar calculation can be made for the typical effect of progressive precedent taking into account the presence of an appeal: $\mathbf{1}[M_{ct} > 0]$*$\mathbf{E}[\mathbf{1}[Progressive_{ct} > 0]]$+$Law_{ct}$*$\mathbf{E}[\mathbf{1}[Progressive_{ct} > 0]]$. The results of these calculations are presented in lower panels of Appendix Tables II, V, VI, VII, X, XI, XII, XV, and XVIII and summarized in Table IX. This results in a smaller magnitude than the unscaled coefficients because the typical Circuit-year is unlikely to experience a free speech precedent (175 decisions occur over the 51-year time period)

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. Since government actors respond strongly to Circuit decisions, if they defer issuing regulations restricting obscenity until a favorable legal regime, then the absence of a case serves as a "supercontrol". See Crépon et al. (2013) for a

national experiment that randomizes both the presence of an employement training program across cities as well as the training of individuals when there was a program. Differences between trained and non-trained individuals reflect our first counterfactual. Differences between trained individuals in treated cities and non-trained individuals in control cities reflect our second counterfactual. Differences between the first and second counterfactuals are what Crépon et al. (2013) refers to as "displacement". Trained individuals displace non-trained individuals from employment when there is a limited supply of positions. Similarly, if there is a limited set of free speech regulations, government actors may issue the regulation only in a favorable legal regime. The lack of displacement effects for sexual attitudes and behavior is not surprising, however, 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 laws during this time period. On net, free speech laws contributed to the sexual revolution, but bear in mind the final estimates rely on identification from w_{ct}.

Finally, we assess the magnitudes of our estimates in comparison to recent studies with plausibly exogenous variation. In Appendix E, we present a graphical analysis of the counterfactual in the absence of any obscenity law. The solid line is the actual crime rate and the dashed line is the counterfactual crime rate in the case of no obscenity law. The counterfactual crime rate is given by the actual crime rate minus the predicted effect of obscenity law on crime. Since the majority of cases were decided in the conservative direction, the actual crime rate is lower than the counterfactual for prostitution and drug violations. The impact on property crimes is imperceptible. The effect sizes are considerably smaller than the 50% of the sexual revolution that has been attributed to individuals' moral views on sexual rights shaped by laws and doctrine (Fernandez-Villaverde et al. 2014), but are closer to the effect sizes found in the expansion of internet broadband on sex crimes in Norway (Bhuller et al. 2013), where 3.2% of the total number of rapes and 2.5% of the total number

TABLE IX

Summary of Results

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

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

of sex crimes and child sex abuses that occurred between 2000 and 2008 would have been avoided if broadband internet had not been introduced.

4.6 Deterrence, Backlash, and Expressive Effects We now turn to an analysis of the effects of free speech law during and after the sexual revolution. Sexual norms have changed dramatically during the time period of our study. As noted by Fernandez-Villaverde et al. (2014), in 1958, 35% of U.S. women engaged in premarital sex by the age of 19 compared to 75% today. They estimate however, if individuals' moral views had not changed, a little over 50% of U.S. women would have had premarital sex by the age of 19 today. Changes in moral views include: In 1968, only 15% of women had a permissive attitude towards premarital sex, but this increased to 45% by 1983. In 1957, 57% of Americans believed that adults who preferred to be single were "immoral", but today, it is no longer considered a moral issue and more than 50% of adults are single. Bearing children out-of-wedlock was once extremely rare, but today more than half of births to women under 30 occur outside of marriage (Klinenberg 2012).Fernandez-Villaverde et al. (2014) report that between 1710 and 1750, 69% of all criminal cases in New Haven were for premarital sex, which was punished by fines, jail, and public flogging. Five times in the last 25 years, the South Korean Constitutional Court has decided on the legality of a law that makes adultery a crime, and in the past six years alone, 5,500 people have been arrested and arraigned. In 2008, a legal opinion in India held that rape by a father-in-law was simply adultery with coercion, and the woman involved not only brought shame upon the family, but was ordered to leave her husband and live with the rapist (Vatuk 2008).

These dramatic differences raise the question: What causes a revolution in conceptions of rights and what role does the law play? The model suggests that backlash should occur when relatively few individuals engage in previously-stigmatized activities, whereas expressive law should occur when many individuals engage in the previously-stigmatized activities. More specifically, 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 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 type by choosing $a = 1$.

Historical studies of the advent of the sexual revolution document backlash by conservatives to stop the Supreme Court from encroaching on state rights to control pornography during the 1950s and 1960s. From 1959 to 1966, bans on three books with explicit erotic content were challenged and overturned. Prior to this time, a patchwork of regulations, local customs, and vigilante actions governed what could and could not be published. For example, the United States Customs Service banned James Joyce's Ulysses by refusing to allow it to be imported into the United States. Different cities and organizations had their own rules for allowable content. The Warren Court greatly expanded civil liberties and in *Memoirs v. Massachusetts* and other cases curtailed the ability of municipalities to regulate the content of literature, plays, and movies. For six years, it reversed summarily—without further opinion—scores of obscenity rulings by lower state and federal courts, culminating in the 1969 decision[46] that held that people could view whatever they wished in the privacy of their own homes. The last ruling caused the U.S. Congress to fund the President's Commission on Obscenity and Pornography. Yet, the 1970 Commission's findings that there was "no evidence to date that exposure to explicit sexual materials plays a significant role in the causation of delinquent or criminal behavior among youths or adults", "no evidence that exposure to explicit sexual materials adversely affects character or moral attitudes regarding sex and sexual conduct", and conclusion that "legislation prohibiting the sale, exhibition, or distribution of sexual materials to consenting adults should be repealed" were roundly rejected and criticized by Congress. In the immedate aftermath, opposing groups authored minority reports that dissented with the Commission's view, which was subsequently cited by the U.S. Supreme Court in later conservative decisions. When Chief Justice Warren was to be replaced by Justice Fortas, a conservative group led by Senator Thurmond organized the "Fortas Obscene Film Festival," (it featured transvestites) which not only led to the res-

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

ignation of Justice Fortas but also the nomination of Justice Burger instead, who by 1973 issued the *Miller* test which repudiated the "utterly without redeeming social value" standard from *Memoirs* in favor of the markedly less liberal "lacks serious literary, artistic, political, or scientific value"Boyce (2008).

To conduct this analysis quantitatively, 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. Column 2 shows that progressive free speech laws reduced paid sex by 0.2 percentage points in the early time frame and Column 4 shows that it increased paid sex by 0.5 percentage points in the later time frame. Using the same source of identification, Table VI shows a positive effect of 0.6 percentage points for the entire time frame. Table X shows that progressive free speech laws reduced arrests for prostitution by 2.1 in the early time frame and increased arrests for prostitution by 9.2 in the later time frame. Using the same source of identification, Table VI shows a positive effect of 9.6 for the entire time frame. Table X shows that progressive free speech laws reduced the number of sexual partners per year by 0.17 in the early time frame and increased the number of sexual partners per year by 0.5 in the later time frame. Using the same source of identification, Table VI shows a positive effect of 0.5 for the entire time frame. Table X shows that progressive free speech laws reduced acceptance of homosexual sex by 5 percentage points in the early time frame and increased acceptance of homosexual sex by 1.7 percentage points in the later

time frame. Using the same source of identification, Table VI shows a positive effect of 1.7 percentage points points on the entire time frame.

Data limitations make a thorough investigation and definitive conclusion difficult. We investigated all outcomes discussed in this paper using the same battery of robustness checks, but due to space constraints, we only report the minimal omnibus test regarding the lead coefficient. Table X shows that the lead coefficients are statistically significant in 1 out of 8 IV models. Appendix Table XVIII shows the full set of coefficients along with additional statistics of interest.Chen and Yeh (2014a) shows that in communities where stigmatized activity is rare, backlash effects occur and progressive free speech decisions lower subjective well-being, while in communities where stigmatized activity is more prevalent, progressive free speech decisions have expressive effects and increased subjective well-being. In a different area of law, Chen et al. (2014c) documents that abortion jurisprudence led to immediate backlash in campaign donations and abortion preferences.[47] Finally, Chen (2013) uses random variation in the application of the death penalty and shows backlash effects among some individuals for whom the death penalty spurred rather than deterred crime. In sum, causal evidence suggests that laws can have backlash effects that transcend the deterrence effects of sanctions.

The finding of backlash effects is inconsistent with deterrence effects as the sole channel to explain the findings. Some of other effects in the field data are also inconsistent with deterrence. We collected data on 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. We

[47]This paper differs from Chen et al. (2014c) in that abortion is inherently private while obscenity is less so, and so the model presented in this paper differs from the model in Chen et al. (2014c). The informational difference between abortion and obscenity is related to a law and economics literature on concealable characteristics and the consequences of mandatory disclosure in diverse settings such as virginity tests (Chen 2004), industry-physician relationships (Chen et al. 2014b), perceived masculinity (Chen et al. 2015a), and public vs. private truths on judicial panels (Chen et al. 2015b).

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.

cannot rule out a possible change in content nor can we rule in or out other channels for obscene content to shift and reach the public.

On a policy matter, we emphasize that we evaluate the effects of free speech law rather than pornography itself. 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 now turn to the experiment, where the short time frame more definitively precludes deterrence effects from being the main mechanism for these findings.

5 Experiment

Attitudes and behaviors in the GSS are self-reported. It is possible that an increased rate of progressive 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 decision, randomized to be progressive or conservative. Our experiment uses progressive free speech decisions related to homosexuality because 45% of our cases mention "gay" or "lesbian" in the opinion. Including the historical identifier, "pervert," increases the proportion of cases related to homosexuality to 65%. We report the results of two experiments, both of which find that progressive free speech decisions cause progressive attitudes about same sex relations. This finding is replicated in a third experiment detailed in Chen and Yeh (2014a). These three experiments vary the research design to assess robustness of the finding. In the experiments, free speech decisions do not affect self-reported sexual behaviors. This suggests that reported sexual behaviors are not simply about openness to discussing the exercise of sexual rights.

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 experiment (Harrison and List 2004; Shaw et al. 2011). 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, i.e., the 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.[48] Because subjects are unaware of an on-going experiment, differential attrition may arise at the time treatment is revealed (Reips

[48]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).

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.[49] 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 2011; Chen and Horton 2014). 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

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

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).

Tables XI and XII Column 4 report that progressive free speech precedent made people more likely to say homosexual sex is acceptable in both experiments. At the baseline, 48%

[49] 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	**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

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 are robust to dropping the control group. These effects also remain when Treatment 4 is excluded. Treatment 4 explicitly refers to homosexual sex.

Table XI shows that self-reported sexual behaviors do not shift in response to progressive free speech decisions. 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. Table XII reports that 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%). Some of the estimates for some moral views are not statistically significant, but the fact that the basic patterns replicate across three experiments, including Chen and Yeh (2014a), suggests a strong expressive effect of free

TABLE XII

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

Attitudes	**Premarital Sex is OK** (1)	**Extramarital Sex is OK** (2)	**Teen Sex is OK** (3)	**Homosexual Sex is OK** (4)	**Favor Sex Ed in Public School** (5)	**Percentage of People who have Extramarital Sex** (6)
Progressive Free Speech Decision	0.00942	0.0145	-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

speech decisions.Chen and Yeh (2014a) verifies that effects on sexual attitudes are robust to an aggregation via a calculation of average effect size.

Shifts in attitudes but not self-reported behaviors was replicated in Chen and Yeh (2014a) using 600 U.S. workers. In this experiment, one group was asked to report their own standards of morality while another group was asked to estimate the other workers' standard of morality and was offered payment incentives for accuracy. We also asked one group to report their own sexual behaviors and another group to estimate the prevalence of the other workers' sexual behaviors with incentive pay for accuracy. This design differs from the first two experiments in that we used monetary incentives to measure belief-updating of others' moral views (community standards), separated individual from community standards, and measured subjective utility. Exposure to progressive free speech decisions caused more progressive sexual attitudes and increased the perceived prevalence of progressive sexual attitudes. But individuals from less progressive communities became stricter in their own standards of morality (reporting less progressive sexual attitudes) and identified more strongly as Republicans, while perceiving others to become more progressive. Progressive decisions also caused both groups to believe that extramarital sex was less prevalent. These results provide evidence for the law having indirect social effects that may amplify or attenuate deterrence effects and suggest that legitimacy of law can affect utility and self-identification.

5.3 Modeling Implications of Experimental Results Table XII also investigates whether exposure to progressive free speech decisions affect beliefs about social norms. Recall that the theoretical model assumes that when legal authorities increase sanctions against a par-

ticular activity, people infer that more people are doing this activity. The downwards bias of OLS estimates as compared to IV estimates is also consistent with judges make conservative decisions when v^* is too high, or equivalently, judges make progressive decisions when v^* is low. Verifying that people do make inferences about v^* upon hearing a court decision, workers reported believing a lower percentage of people having extramarital sex after being exposed to progressive free speech decisions. The effect is 2.5 percentage points out of a mean of 44.5%. Exposure to conservative free speech jurisprudence did 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. Neither experiment found this effect (results are available on request). This suggests that information about negative externalities is not the channel for the expressive effects in our study. A shift in norm perception in response to the law helps explain the reversal from backlash to expressive effects of free speech law during and after the sexual revolution.

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

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. Recall that $LATE$ + 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. Filling in parts of this equation yields: 0.3% + effect on always takers = (3.5% + TOT_{indirect}) * P(individual exposure in treated circuit). Assuming that 3.5% * P(individual exposure in treated circuit) is the direct deterrence or expressive effects from hearing about the case, then TOT_{indirect} * P(individual exposure in treated circuit) captures the expressive externalities

on individuals who did not hear about the case. Further, if we allow different probabilities for the direct and indirect exposures, it is reasonable to believe that P(individual direct exposure in treated circuit) is quite small while P(individual indirect exposure in treated circuit) can be large. Further modeling or data analysis is needed to pin down the other parameters.

6 Conclusion

How do moral revolutions occur? The origins of rights have long interested legal and humanistic scholars (Tushnet 2009; Appiah 2011; Chen 2015). Both advocates and critics of legal change recognize the possibility that laws can have effects through the moral messages that they convey. Social scientists and philosophers have long debated whether law shapes values. We present an analysis of fifty years of free speech jurisprudence. Random assignment of judges to Circuit panels effectively created random variation in common law precedent. We find that progressive free speech precedent caused more progressive attitudes and behaviors on non-marital sexual activity, some sex-related crimes, and higher rates of STDs. In particular, progressive free speech precedent appears to have increased prostitution and drug violations. Notably, progressive free speech precedent decreased child sex abuse. Corroborating the expressive rather than deterrence channel, workers randomly assigned to transcribing newspaper summaries of progressive (as opposed to conservative) court decisions reported more progressive sexual attitudes (but not sexual behaviors). Progressive court decisions also decreased the perceived prevalence of extramarital sex.

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. Those who are motivated by intrinsic incentives have an easier time in signaling to others their type, as they realize fewer people are pretending to have high instrinsic motivations. This induces an "expressive effect," when law causes what is viewed as moral to shift towards what the law values. The 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 high intrinsic motivations are now perceived to do the

stigmatized activity. When the normalizing effect exceeds the signaling effect, this induces a "backlash effect," as the law causes what is viewed as moral to shift against what the law values. This perspective helps explain why laws have backlash or expressive effects in certain times and places.

References

Abramowicz, Michael, Ian Ayres, and Yair Listokin, 2011, Randomizing Law, *University of Pennsylvania Law Review* 159, 929–1005.

Acemoglu, Daron, and Matthew O. Jackson, 2014, Social Norms and the Enforcement of Laws, NBER Working Paper 20369, National Bureau of Economic Research.

Akerlof, George A., Janet L. Yellen, and Michael L. Katz, 1996, An Analysis of Out-of-Wedlock Childbearing in the United States, *The Quarterly Journal of Economics* 111, 277–317.

Aldashev, Gani, Imane Chaara, Jean-Philippe Platteau, and Zaki Wahhaj, 2012, Using the Law to Change the Custom, *Journal of Development Economics* 97, 182–200.

Allen, Mike, Dave D'Alessio, and Keri Brezgel, 1995, A Meta-Analysis Summarizing the Effects of Pornography II Aggression After Exposure, *Human Communication Research* 22, 258–283.

Anderson, Elizabeth S., and Richard H. Pildes, 2000, Expressive Theories of Law: A General Restatement, *University of Pennsylvania Law Review* 148, 1503–1575.

Andreoni, James, 1989, Giving with Impure Altruism: Applications to Charity and Ricardian Equivalence, *The Journal of Political Economy* 97, 1447–1458.

Appiah, Kwame Anthony, 2011, *The Honor Code: How Moral Revolutions Happen* (W. W. Norton).

Araiza, Alberto G., Daniel L. Chen, and Susan Yeh, 2014, Does Appellate Precedent Matter? Stock Price Responses to Appellate Court Decisions on FCC Actions, in Yun chien Chang, ed., *Empirical Legal Analysis: Assessing the Performance of Legal Institutions*, volume 19 of *Economics of Legal Relationships* (Routledge).

Ashenfelter, Orley, Theodore Eisenberg, and Stewart J. Schwab, 1995, Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes, *Journal of Legal Studies* 24, 257–281.

Badawi, Adam B., and Daniel L. Chen, 2014, The Shareholder Wealth Effects of Delaware Litigation, Working paper, ETH Zurich, Mimeo.

Bailey, Martha J., 2010, "Momma's Got the Pill": How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing, *The American Economic Review* 100, 98–129.

Baliga, Sandeep, Eran Hanany, and Peter Klibanoff, 2013, Polarization and ambiguity, *The American Economic Review* 103, 3071–3083.

Baron, Larry, and Murray A. Straus, 1984, Sexual Stratification, Pornography, and Rape in the United States, in Neil M. Malamuth, and Edward Donnerstein, eds., *Pornography and Sexual Aggression*, 185–209 (Academic Press, Orlando, Florida).

Barondes, Royce De Rohan, 2010, Federal District Judge Gender and Reversals, in *5th Annual Conference on Empirical Legal Studies Paper*, Working Paper.

Barrios, Thomas, Rebecca Diamond, Guido W. Imbens, and Michal Kolesár, 2012, Clustering, Spatial Correlations and Randomization Inference, *Journal of the American Statistical Association* 107, 578–591.

Barth, Thomas, 1968, Perception and Acceptance of Supreme Court Decisions at the State and Local Level, *Journal of Public Law* 17, 308–350.

Becker, Gary S., 1968, Crime and Punishment: An Economic Approach, *Journal of Political Economy* 76, 169–217.

Belloni, Alex, Daniel L. Chen, Victor Chernozhukov, and Chris Hansen, 2012, Sparse Models and Methods for Optimal Instruments with an Application to Eminent Domain, *Econometrica* 80, 2369–2429.

Bem, Daryl J., 1972, Self-Perception Theory, in Leonard Berkowitz, ed., *Advances in Experimental Social Psychology*, volume 6, 1–62 (Academic Press, New York).

Ben-Veniste, Richard, 1971, Pornography and Sex Crime: The Danish Experience, *Technical Reports of the Commission on Obscenity and Pornography* 7, 245–261.

Bénabou, Roland, and Jean Tirole, 2012, Laws and Norms, Discussion Paper series 6290, Institute for the Study of Labor (IZA), Bonn, Germany.

Berdejó, Carlos, and Daniel L. Chen, 2014, Priming Ideology? Electoral Cycles without Electoral Incentives among U.S. Judges, Working paper, ETH Zurich.

Berliner, Dana, 2003, *Public Power, Private Gain: A Five-Year State By State Report Examining the Abuses of Eminent Domain*.

Bernheim, B. Douglas, 1994, A Theory of Conformity, *The Journal of Political Economy* 102, 841–877.

Bhuller, Manudeep, Tarjei Havnes, Edwin Leuven, and Magne Mogstad, 2013, Broadband Internet: An Information Superhighway to Sex Crime?, *Review of Economic Studies* 80, 1237–1266.

Bird, Susan Willett, 1975, The Assignment of Cases to Federal District Court Judges, *Stanford Law Review* 27, 475–487.

Bokor, Matt, 1980, Jacksonville Porn-Free, Officials Say, Associated Press - Domestic News.

Bowles, Samuel, and Sandra Polania-Reyes, 2012, Economic Incentives and Social Preferences: Substitutes or Complements?, *Journal of Economic Literature* 50, 368–425.

Boyce, Bret, 2008, Obscenity and Community Standards, *Yale Journal of International Law* 33, 299–368.

Brekke, Kjell Arne, Snorre Kverndokk, and Karinen Nyborg, 2003, An Economic Model of Moral Motivation, *Journal of Public Economics* 87, 1967–1983.

Breyer, Stephen, 2006, *Active Liberty: Interpreting Our Democratic Constitution* (Vintage Books).

Brown, Jane D., and S.F. Newcomer, 1991, Television Viewing and Adolescents' Sexual Behavior, *Journal of Homosexuality* 21, 77–92.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, 2008, Bootstrap-Based Improvements for Inference with Clustered Errors, *The Review of Economics and Statistics* 90, 414–427.

Card, David, and Laura Giuliano, 2011, Peer Effects and Multiple Equilibria in the Risky Behavior of Friends, NBER Working Papers 17088, National Bureau of Economic Research.

Chang, Tom, and Antoinette Schoar, 2013, Judge Specific Differences in Chapter 11 and Firm Outcomes, Revise and Resubmit at Journal of Finance.

Chen, Daniel, Yosh Halberstam, and Alan C. L. Yu, 2015a, Perceived masculinity predicts court outcomes at the United States Supreme Court, Technical report, ETH Zurich, Mimeo.

Chen, Daniel L., 2004, Gender Violence and the Price of Virginity: Theory and Evidence of Incomplete Marriage Contracts, Working paper, University of Chicago, Mimeo.

Chen, Daniel L., 2006, Islamic Resurgence and Social Violence During the Indonesian Financial Crisis, in Mark Gradstein, and Kai A. Konrad, eds., *Institutions and Norms in Economic Development*, chapter 8, 179–199 (MIT Press).

Chen, Daniel L., 2010, Club Goods and Group Identity: Evidence from Islamic Resurgence during the Indonesian Financial Crisis, *The Journal of Political Economy* 118, 300–354.

Chen, Daniel L., 2011, Markets and Morality: How Does Competition Affect Moral Judgment?, Working paper, Duke Law School.

Chen, Daniel L., 2013, The Deterrent Effect of the Death Penalty? Evidence from British Commutations During World War I, Working paper, ETH Zurich.

Chen, Daniel L., 2015, Can Markets Stimulate Rights? On the Alienability of Legal Claims, *RAND Journal of Economics* 46, 23–65.

Chen, Daniel L., Jens Frankenreiter, and Susan Yeh, 2014a, Measuring the Effects of Legal Precedent in U.S. Federal Courts, Working paper, ETH Zurich.

Chen, Daniel L., and John J. Horton, 2014, Are Online Labor Markets Spot Markets for Tasks?: A Field Experiment on the Behavioral Response to Wage Cuts, *Management Information Systems Quarterly* (Revised and Resubmitted).

Chen, Daniel L., Vardges Levonyan, S. Eric Reinhart, and Glen B. Taksler, 2014b, Mandatory Disclosure: Theory and Evidence from Industry-Physician Relationships, Working paper, Mimeo.

Chen, Daniel L., Vardges Levonyan, and Susan Yeh, 2014c, Do Policies Affect Preferences? Evidence from Random Variation in Abortion Jurisprudence, Working paper, ETH Zurich.

Chen, Daniel L., and Jo Lind, 2007, Religion, Welfare Politics, and Church-State Separation, *Journal of Ecumenical Studies* 42, 42–52.

Chen, Daniel L., and Jo Thori Lind, 2014, The Political Economy of Beliefs: Why Fiscal and Social Conservatives and Liberals Come Hand-in-Hand, Working paper.

Chen, Daniel L., Moti Michaeli, and Daniel Spiro, 2015b, Ideological Perfectionism on Judicial Panels, Working paper, ETH Zurich.

Chen, Daniel L., Tobias J. Moskowitz, and Kelly Shue, 2015c, Decision-Making Under the Gambler's Fallacy: Evidence from Asylum Judges, Loan Officers, and Baseball Umpires, Working paper, ETH Zurich.

Chen, Daniel L., and Martin Schonger, 2013, Social Preferences or Sacred Values? Theory and Evidence of Deontological Motivations, Working paper, ETH Zurich, Mimeo.

Chen, Daniel L., and Martin Schonger, 2015, A Theory of Experiments: Invariance of Equilibrium to the Strategy Method of Elicitation and Implications for Social Preferences, Working paper, ETH Zurich.

Chen, Daniel L., and Jasmin Sethi, 2011, Insiders and Outsiders: Does Forbidding Sexual Harassment Exacerbate Gender Inequality?, Working paper, University of Chicago.

Chen, Daniel L., and Holger Spamann, 2014, This Morning's Breakfast, Last Night's Game: Detecting Extraneous Factors in Judging, Working paper, ETH Zurich.

Chen, Daniel L., and Susan Yeh, 2013, Distinguishing Between Custom and Law: Empirical Examples of Endogeneity in Property and First Amendment Precedents, *William & Mary Bill of Rights Journal* 21, 1081–1105.

Chen, Daniel L., and Susan Yeh, 2014a, The Construction of Morals, *Journal of Economic Behavior and Organization* 104, 84–105.
Chen, Daniel L., and Susan Yeh, 2014b, Growth Under the Shadow of Expropriation? The Economic Impacts of Eminent Domain, Working paper, ETH Zurich and George Mason University.
Cialdini, Robert B., 1984, *Influence: How and Why People Agree to Things* (Quill New York).
Cooter, Robert, 1988, Expressive Law and Economics, *The Journal of Legal Studies* 27, 585–607.
Cooter, Robert D., Michal Feldman, and Yuval Feldman, 2008, The Misperception of Norms: The Psychology of Bias and the Economics of Equilibrium, *Review of Law and Economics* 4, 889–911.
Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora, 2013, Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment, *The Quarterly Journal of Economics* 128, 531–580.
Croson, Rachel, 2009, Experimental Law and Economics, *Annual Review of Law and Social Science* 5, 25–44.
Dahl, Gordon, and Stefano DellaVigna, 2009, Does Movie Violence Increase Violent Crime?, *The Quarterly Journal of Economics* 124, 677–734.
Dal Bó, Pedro, Andrew Foster, and Louis Putterman, 2010, Institutions and Behavior: Experimental Evidence on the Effects of Democracy, *American Economic Review* 100, 2205–2229.
Dennis, Donna I., 2007, Obscenity Law and its Consequences in Mid-Nineteenth-Century America, *Columbia Journal of Gender and Law* 16, 43–96.
Donnerstein, Edward I., and Daniel G. Linz, 1986, Mass Media Sexual Violence and Male Viewers: Current Theory and Research., *American Behavioral Scientist* 29, 601–618.
Dworkin, Andrea, and Catharine A. MacKinnon, 1988, *Pornography and Civil Rights: A New Day for Women's Equality* (Organizing Against Pornography).
Eckes, Alfred E., 2000, Backlash Against Globalization?, *Global Economic Quarterly* 1, 117–136.
Edelman, Benjamin, 2009, Markets: Red Light States: Who Buys Online Adult Entertainment?, *The Journal of Economic Perspectives* 23, 209–220.
Ellickson, Robert C., 1998, Law and Economics Discovers Social Norms, *The Journal of Legal Studies* 27, 537–552.
Feld, Barry C., 2003, Race, Politics, and Juvenile Justice: The Warren Court and the Conservative "Backlash", *Minnesota Law Review* 87, 1447–2173.
Fernandez-Villaverde, Jesus, Jeremy Greenwood, and Nezih Guner, 2014, From Shame to Game in One Hundred Years: An Economic Model of the Rise in Premarital Sex and its De-Stigmatization, *Journal of the European Economic Association* 12, 25–61.
Fish, A.N.J., D.V.I. Fairweather, J.D. Oriel, and G.L. Ridgway, 1989, Chlamydia trachomatis infection in a gynaecology clinic population: identification of high-risk groups and the value of contact tracing, *European Journal of Obstetrics & Gynecology and Reproductive Biology* 31, 67–74.
Fitzpatrick, Brian T, 2010, An Empirical Study of Class Action Settlements and Their Fee Awards, *Journal of Empirical Legal Studies* 7, 811–846.
Fox, Dov, and Christopher L. Griffin, Jr., 2009, Disability-Selective Abortion and the Americans with Disabilities Act, *Utah Law Review* 2009, 845–905.
Funk, Patricia, 2007, Is There An Expressive Function of Law? An Empirical Analysis of Voting Laws with Symbolic Fines, *American Law and Economics Review* 9, 135–159.
Galbiati, Roberto, and Pietro Vertova, 2008, Obligations and cooperative behaviour in public good games, *Games and Economic Behavior* 64, 146–170.
Gennaioli, Nicola, and Andrei Shleifer, 2007, The Evolution of Common Law, *The Journal of Political Economy* 115, 43–68.
Gentzkow, Matthew, and Jessie M. Shapiro, 2008, Preschool Television Viewing and Adolescent Test Scores: Historical Evidence from the Coleman Study, *The Quarterly Journal of Economics* 123, 279–323.
Goldman, Sheldon, 1999, *Picking Federal Judges: Lower Court Selection from Roosevelt Through Reagan* (Yale University Press).
Haidt, Jonathan, 2001, The Emotional Dog and its Rational Tail: A Social Intuitionist Approach to Moral Judgment, *Psychological Review* 108, 814–834.
Haire, Susan B., Donald R. Songer, and Stefanie A. Lindquist, 2003, Appellate Court Supervision in the Federal Judiciary: A Hierarchical Perspective, *Law & Society Review* 37, 143–168.
Hall, Matthew, 2010, Randomness Reconsidered: Modeling Random Judicial Assignment in the U.S. Courts of Appeals, *Journal of Empirical Legal Studies* 7, 574–589.
Harrison, Glenn W., and John A. List, 2004, Field Experiments, *Journal of Economic Literature* 42, 1009–1055.

Hoekstra, Valerie J., 2000, The Supreme Court and Local Public Opinion, *The American Political Science Review* 94, 89–100.

Hoekstra, Valerie J., 2003, *Public Reaction to Supreme Court Decisions* (Cambridge University Press).

Holmes, King K., Ruth Levine, and Marcia Weaver, 2004, Effectiveness of Condoms in Preventing Sexually Transmitted Infections, *Bulletin of the World Health Organization* 82, 454–461.

Jordan, Samuel P., 2007, Early Panel Announcement, Settlement, and Adjudication, *Brigham Young University Law Review* 2007, 55–107.

Kahan, Dan M., 1997, Between Economics and Sociology: The New Path of Deterrence, *Michigan Law Review* 95, 2477–2497.

Kalven, Harry Jr., 1960, The Metaphysics of the Law of Obscenity, *The Supreme Court Review* 1960, 1–45.

Kaplow, Louis, and Steven Shavell, 2007, Moral Rules, the Moral Sentiments, and Behavior: Toward a Theory of an Optimal Moral System, *Journal of Political Economy* 115, 494–514.

Kastellec, Jonathan P., 2011, Panel Composition and Voting on the US Courts of Appeals over Time, *Political Research Quarterly* 64, 377–391.

Kastellec, Jonathan P., 2013, Racial Diversity and Judicial Influence on Appellate Courts, *American Journal of Political Science* 57, 167–183.

Keele, Denise M., Robert W. Malmsheimer, Donald W. Floyd, and Lianjun Zhang, 2009, An Analysis of Ideological Effects in Published Versus Unpublished Judicial Opinions, *Journal of Empirical Legal Studies* 6, 213–239.

Kendall, Todd D., 2007, Pornography, Rape, and the Internet.

Klarman, Michael, 2004, *From Jim Crow to Civil Rights: The Supreme Court and the Struggle for Racial Equality* (Oxford University Press, New York).

Klarman, Michael J., 2005, Brown and Lawrence (and Goodridge), *Michigan Law Review* 104, 431–489.

Klick, Jonathan, and Thomas Stratmann, 2003, The Effect of Abortion Legalization on Sexual Behavior: Evidence from Sexually Transmitted Diseases, *The Journal of Legal Studies* 32, 407–433.

Klinenberg, Eric, 2012, *Going Solo: The Extraordinary Rise and Surprising Appeal of Living Alone* (The Penguin Press).

Kobylka, Joseph F., 1991, *The Politics of Obscenity: Group Litigation in a Time of Legal Change* (Greenwood Press).

Kremer, Michael, 1996, Integrating Behavioral Choice into Epidemiological Models of AIDS, *The Quarterly Journal of Economics* 111, 549–573.

Kretzschmar, Mirjam, Yvonne T. H. P. van Duynhoven, and Anton J. Severijnen, 1996, Modeling Prevention Strategies for Gonorrhea and Chlamydia Using Stochastic Network Simulations, *American Journal of Epidemiology* 144, 306–317.

Krieger, Linda Hamilton, 2000, Foreword–Backlash Against the ADA: Interdisciplinary Perspectives and Implications for Social Justice Strategies, *Berkeley Journal of Employment and Labor Law* 21, 1–18.

Law, David S., 2005, Strategic Judicial Lawmaking: Ideology, Publication, and Asylum Law in the Ninth Circuit, *University of Cincinnati Law Review* 73, 817–866.

Lessig, Lawrence, 1998, The New Chicago School, *The Journal of Legal Studies* 27, 661–691.

Lockhart, William B., and Robert C. McClure, 1960, Censorship of Obscenity: The Developing Constitutional Standards, *Minnesota Law Review* 45, 5–122.

Lopez, Edward J., R. Todd Jewell, and Noel D. Campbell, 2009, Pass a Law, Any Law, Fast! State Legislative Responses to the *Kelo* Backlash, *Review of Law and Economics* 5, 101–135.

MacKinnon, Catharine A., 1987, Not a Moral Issue, in *Feminism Unmodified*, 146–162 (Harvard University Press, Cambridge, MA).

Magrath, C. Peter, 1966, The Obscenity Cases: Grapes of Roth, *The Supreme Court Review* 1966, 7–77.

Mann, Christopher B., 2010, Is There Backlash to Social Pressure? A Large-Scale Field Experiment on Voter Mobilization, *Political Behavior* 32, 387–407.

Martino, Stephen C., Rebecca L. Collins, Marc N. Elliott, Amy Strachman, David E. Kanouse, and Sandra H. Berry, 2006, Exposure to Degrading Versus Nondegrading Music Lyrics and Sexual Behavior Among Youth, *Pediatrics* 118, e430–e441.

McAdams, Richard, 2000, An Attitudinal Theory of Expressive Law, *Oregon Law Review* 79, 339–390.

McAdams, Richard H., and Janice Nadler, 2008, Coordinating in the Shadow of the Law: Two Contextualized Tests of the Focal Point Theory of Legal Compliance, *Law & Society Review* 42, 865–898.

Mead, Julia C., 2005, Village Can Shut X-Rated Store, The New York Times.

Mechanic, David, 2001, The Managed Care Backlash: Perceptions and Rhetoric in Health Care Policy and the Potential for Health Care Reform, *The Milbank Quarterly* 79, 35–54.

Merritt, Deborah Jones, and James J. Brudney, 2001, Stalking Secret Law: What Predicts Publication in the United States Courts of Appeals, *Vanderbilt Law Review* 54, 69–121.

Mill, John Stuart, 1859, *On Liberty*, second edition (John W. Parker and Son, London).

Mitchell, Katharyne, 2004, Geographies of Identity: Multiculturalism Unplugged, *Progress in Human Geography* 28, 641–651.

Nader, Ralph, and Alan Hirsch, 2004, Making Eminent Domain Humane, *Villanova Law Review* 49, 207–232.

Nash, Jonathan R., 2015, Examining Federal District Judges' Referrals to Magistrate Judges, in *2015 Annual Meeting of the International Society of New Institutional Economics (ISNIE)*, number 1-44.

Nelson, Kenrad E., and Carolyn M. Williams, 2007, *Infectious Disease Epidemiology: Theory and Practice* (Jones and Bartlett Publishers).

Nielsen, Laiura B., Robert L. Nelson, and Ryon Lancaster, 2010, Individual Justice or Collective Legal Mobilization? Employment Discrimination Litigation in the Post Civil Rights United States, *Journal of Empirical Legal Studies* 7, 175–201.

Orne, Martin T., 1962, On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and Their Implications, *American Psychologist* 17, 776–783.

Paolacci, Gabriele, Jesse Chandler, and Panagiotis G. Ipeirotis, 2010, Running experiments on Amazon Mechanical Turk, *Judgment and Decision Making* 5, 411–419.

Paul, Bryant., Bradley J. Shafer, and Daniel Linz, 2001, Government Regulation of "Adult" Businesses Through Zoning and Anti-Nudity Ordinances: Debunking the Legal Myth of Negative Secondary Effects, *Communication Law & Policy* 6, 355–391.

Peresie, Jennifer L., 2005, Female Judges Matter: Gender and Collegial Decisionmaking in the Federal Appellate Courts, *The Yale Law Journal* 114, 1759–1790.

Pesendorfer, Wolfgang, 1995, Design Innovation and Fashion Cycles, *The American Economic Review* 85, 771–792.

Posner, Eric A., 1998a, Symbols, Signals, and Social Norms in Politics and the Law, *The Journal of Legal Studies* 27, 765–797.

Posner, Eric A., 2000, *Law and Social Norms* (Harvard University Press, Cambridge, Mass.).

Posner, Richard A., 1998b, Against Constitutional Theory, *New York University Law Review* 73, 1–22.

Price, Joyce, 1996, Community Standards' Ruling Stands; On-line Porn Judged by Download Site, The Washington Times.

Pridemore, William Alex, and Joshua D. Freilich, 2007, The Impact of State Laws Protecting Abortion Clinics and Reproductive Rights on Crimes against Abortion Providers: Deterrence, Backlash, or Neither?, *Law and Human Behavior* 31, 611–627.

Radin, Margaret Jane, 1996, *Contested Commodities: The Trouble with Trade in Sex, Children, Body Parts, and Other Things* (Harvard University Press, Cambridge, Mass.).

Reips, Ulf-Dietrich, 2001, The Web Experimental Psychology Lab: Five years of data collection on the Internet, *Behavior Research Methods, Instruments, and Computers* 33, 201–211.

Sen, Maya, 2015, Is Justice Really Blind? Race and Appellate Review in U.S. Courts, *Journal of Legal Studies* 44, (In Press).

Shaw, Aaron D., John J. Horton, and Daniel L. Chen, 2011, Designing Incentives for Inexpert Human Raters, in *Proceedings of the ACM 2011 conference on Computer supported cooperative work*, 275–284, New York.

Smith, Adam, 1761, *The Theory of Moral Sentiments* (A. Millar).

Songer, Donald R., and Susan Haire, 1992, Integrating Alternative Approaches to the Study of Judicial Voting: Obscenity Cases in the U.S. Courts of Appeals, *American Journal of Political Science* 36, 963–982.

Steinbuch, Robert, 2009, An Empirical Analysis of Reversal Rates in the Eighth Circuit During 2008, *Loyola of Los Angeles Law Review* 43, 51–19.

Stock, James H., and Motohiro Yogo, 2005, Testing for Weak Instruments in Linear IV Regression, in Donald W.K. Andrews, and James H. Stock, eds., *Identification and Inference for Economic Models: Essays in Honor of Thomas Rothenberg*, 80–108 (Cambridge University Press, Cambridge, MA).

Stroebel, Johannes, and Arthur van Benthem, 2012, The Power of the Church - The Role of Roman Catholic Teaching in the Transmission of HIV, Technical report, New York University.

Strouse, Jeremiah S., Nancy Buerkel-Rothfuss, and Elizabeth C. Long, 1995, Gender and Family as Moderators of the Relationship Between MusicVideo Exposure and Adolescent Sexual Permissiveness, *Adolescence* 30, 505–521.

Sunstein, Cass R., 1996, On the Expressive Function of Law, *University of Pennsylvania Law Review* 144, 2021–2053.

Sunstein, Cass R., David Schkade, Lisa M. Ellman, and Andres Sawicki, 2006, *Are Judges Political?: An Empirical Analysis of the Federal Judiciary* (Brookings Institution Press).

Sunstein, Cass R., David Schkade, and Lisa Michelle Ellman, 2004, Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation, *Virginia Law Review* 90, 301–354.

Taha, Ahmed E., 2004, Publish or Paris? Evidence of How Judges Allocate Their Time, *American Law and Economics Review* 6, 1–27.

Taha, Ahmed E., 2009, Judge Shopping: Testing Whether Judges' Political Orientations Affect Case Filings, *University of Cincinnati Law Review* 20, 101–135.

Titchener, James L., 1967, Experimenter Effects in Behavioral Research, *Archives of Internal Medicine* 120, 753–755.

Tushnet, Mark V., 2009, *The Rights Revolution in the Twentieth Century* (American Historical Association).

Tyler, Tom R., 2006, *Why People Obey the Law* (Princeton University Press).

Tyran, Jean-Robert, and Lars P. Feld, 2006, Achieving Compliance when Legal Sanctions are Non-deterrent*, *The Scandinavian Journal of Economics* 108, 135–156.

Vatuk, Sylvia, 2008, Islamic Feminism in India: Indian Muslim Women Activists and the Reform of Muslim Personal Law, *Modern Asian Studies* 42, 489–518.

Wald, Patricia M., 1999, A Response to Tiller and Cross, *Columbia Law Review* 99, 235–261.

Waldfogel, Joel, 1995, The Selection Hypothesis and the Relationship between Trial and Plaintiff Victory, *The Journal of Political Economy* 103, 229–260.

Walker, Anders, 2011, A Horrible Fascination: Sex, Segregation, & the Lost Politics of Obscenity, *Washington University Law Review* 89, 1017–1064.

Weinrib, Laura M., 2012, The Sex Side of Civil Liberties: United States v. Dennett and the Changing Face of Free Speech, *Law and History Review* 30, 325–386.

Wolf, Michael A., 1995, Overtaking the Fifth Amendment: The Legislative Backlash Against Environmentalism, *Fordham Environmental Law Journal* 6, 637–660.

Zillman, Dolf, and Jennings Bryant, 1984, Effects of Massive Exposure to Pornography, in Neil M. Malamuth, and Edward Donnerstein, eds., *Pornography and Sexual Aggression*, 114–138 (Academic Press, Orlando, Florida).

Figure 1

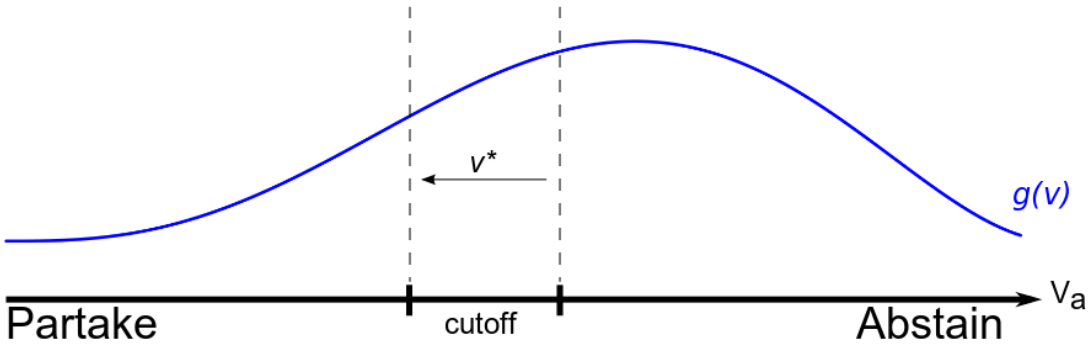


Figure 2

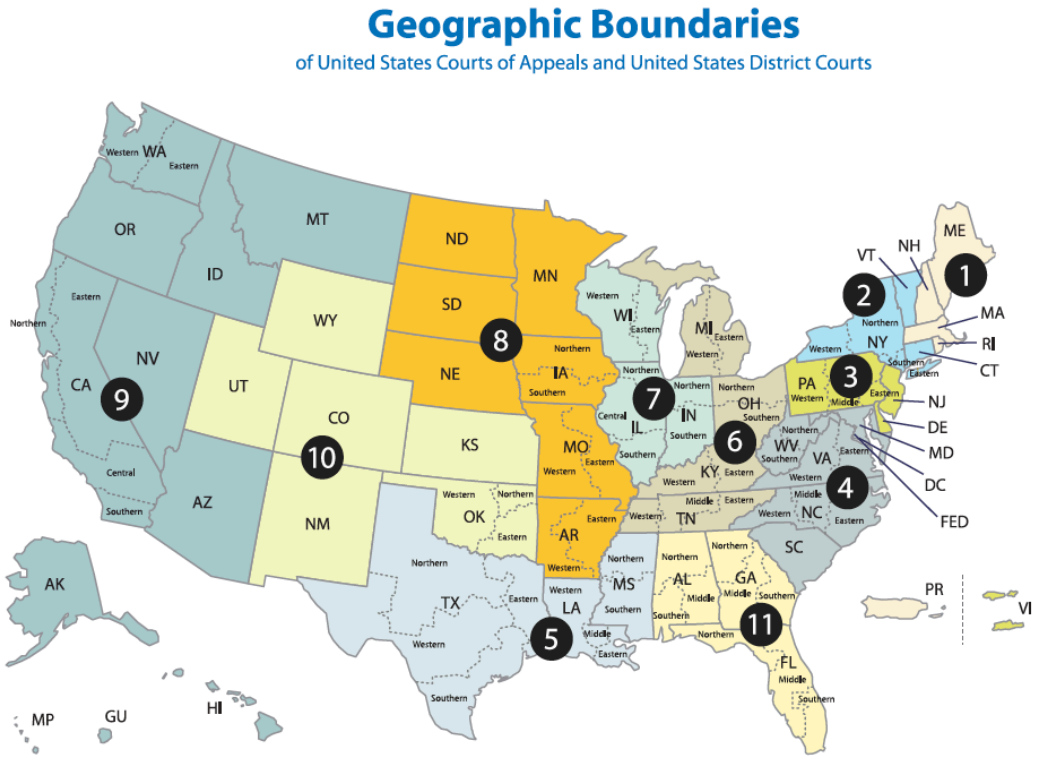
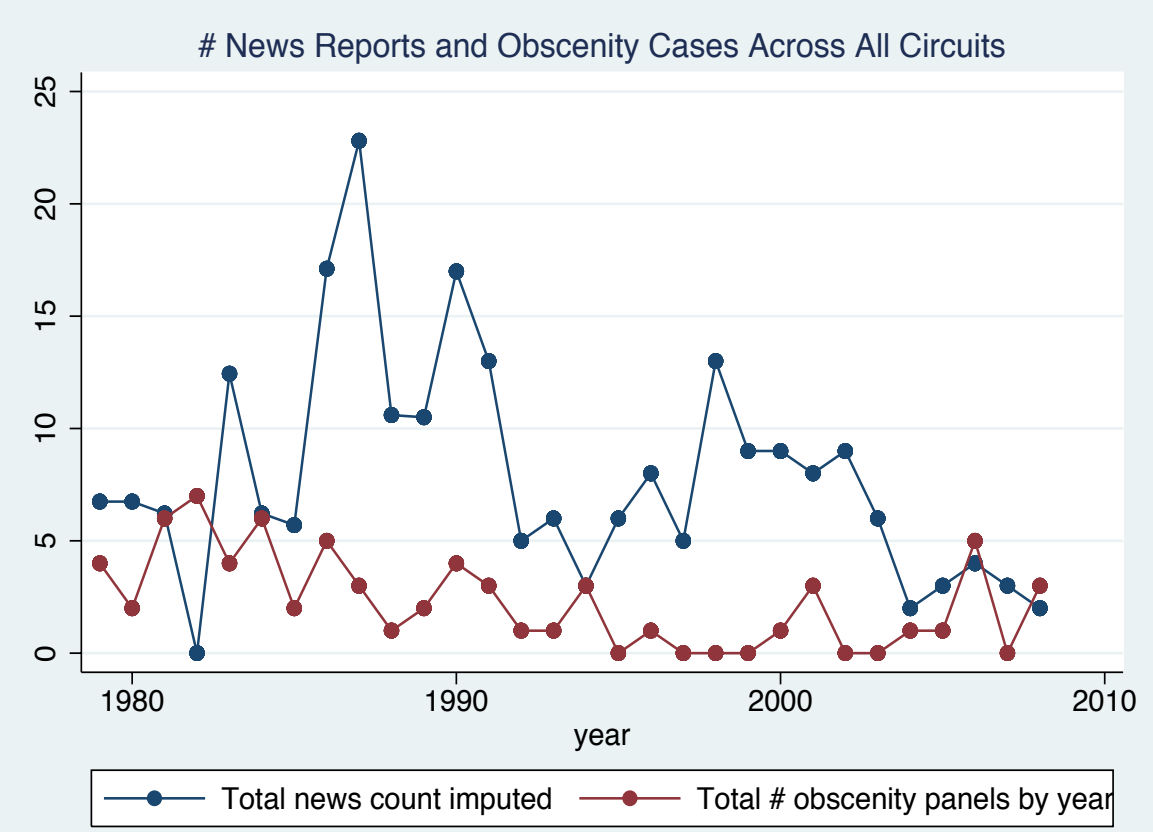


Figure 3


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

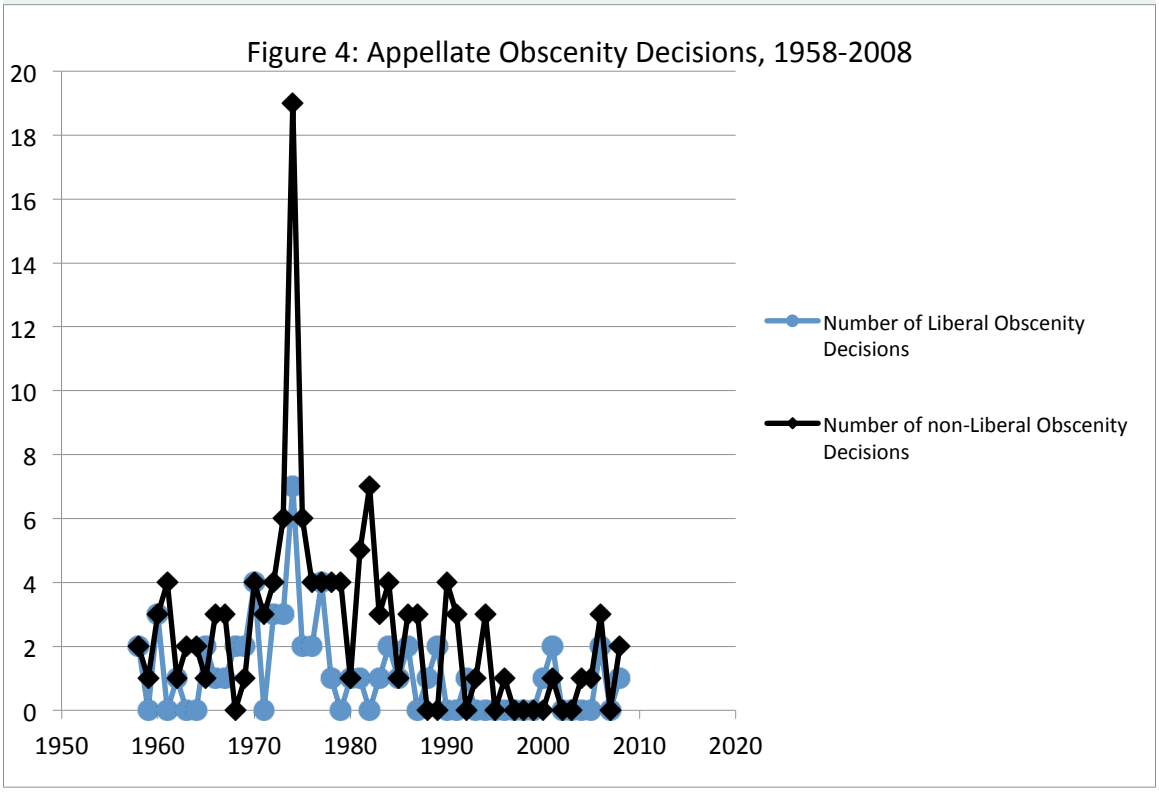

Figure 4: Appellate Obscenity Decisions, 1958-2008
0
2
4
6
8
10
12
14
16
18
20
1950
1960
1970
1980
1990
2000
2010
2020
Number of Liberal Obscenity Decisions
Number of non-Liberal Obscenity Decisions

Figure 5A

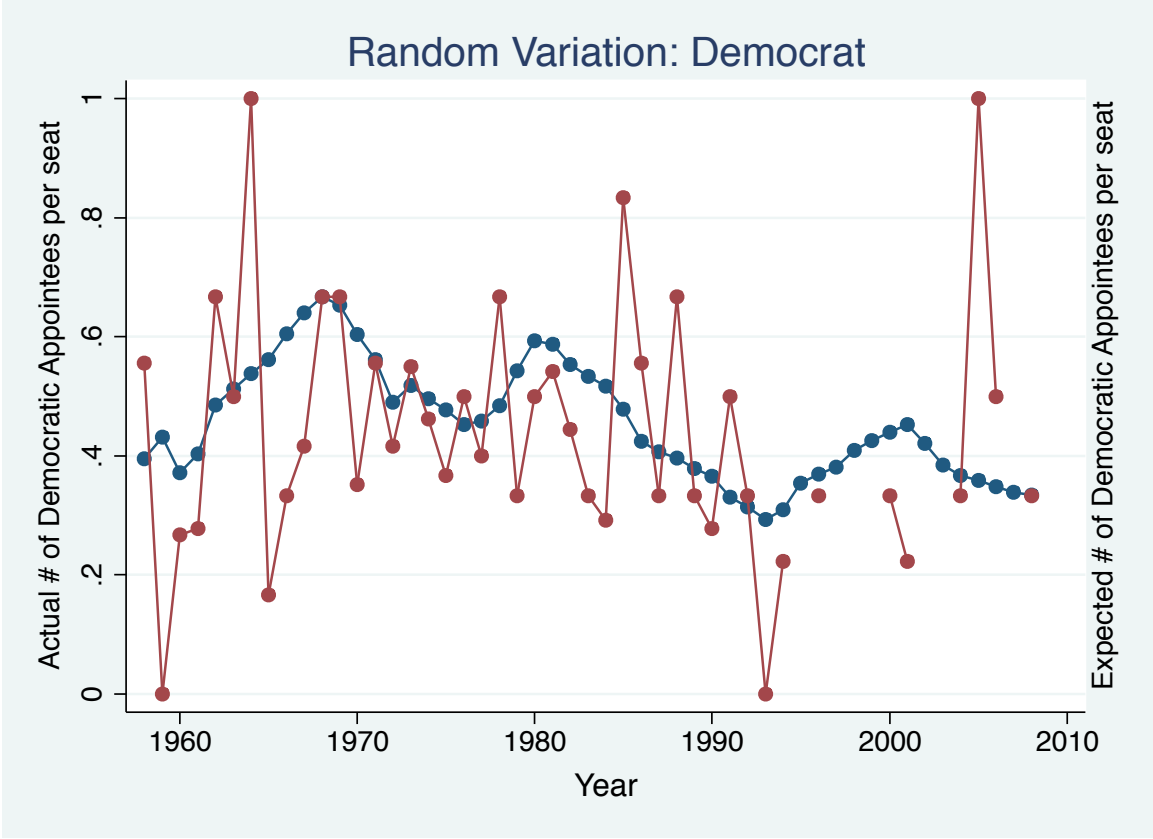


Figure 5B

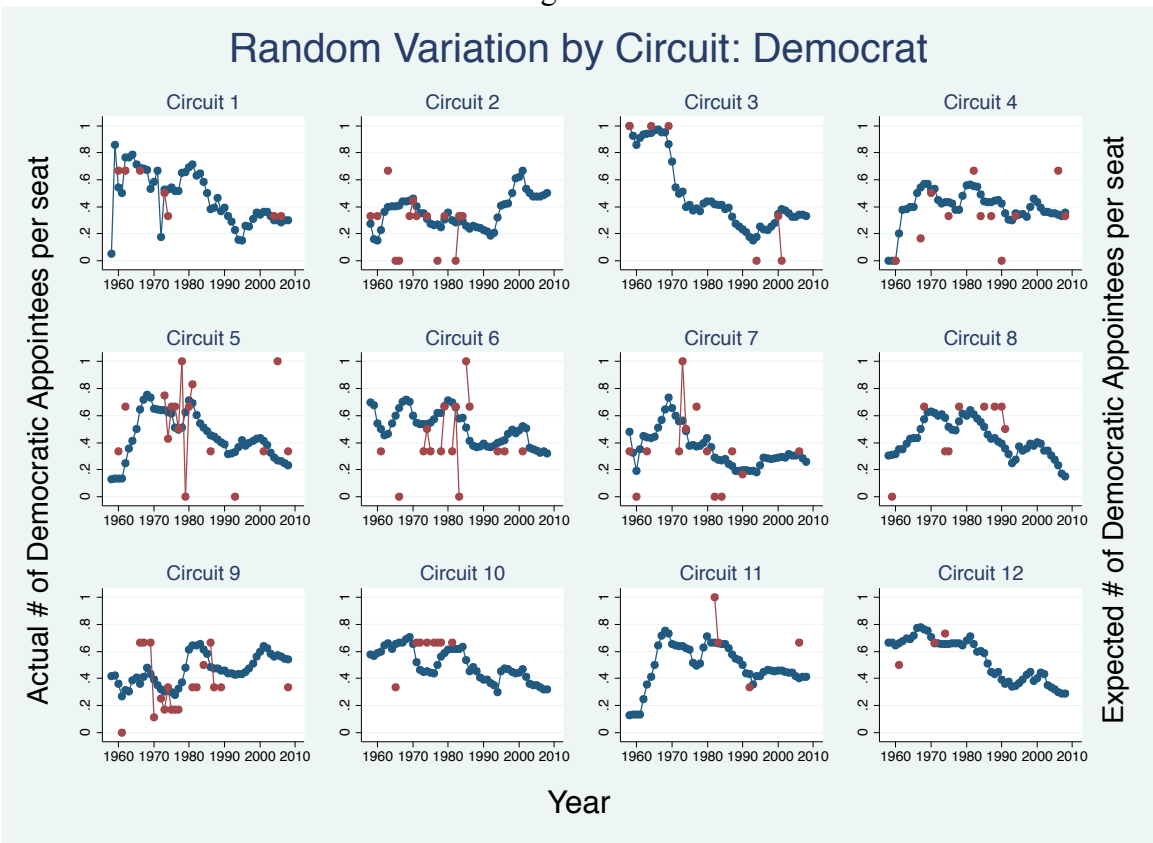
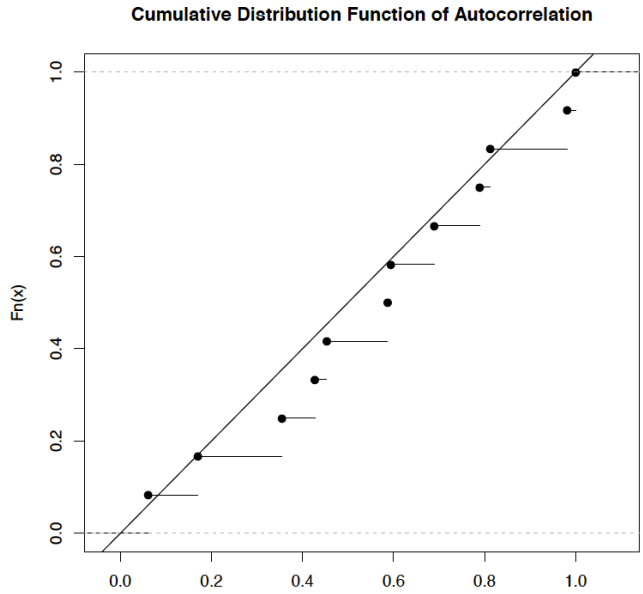
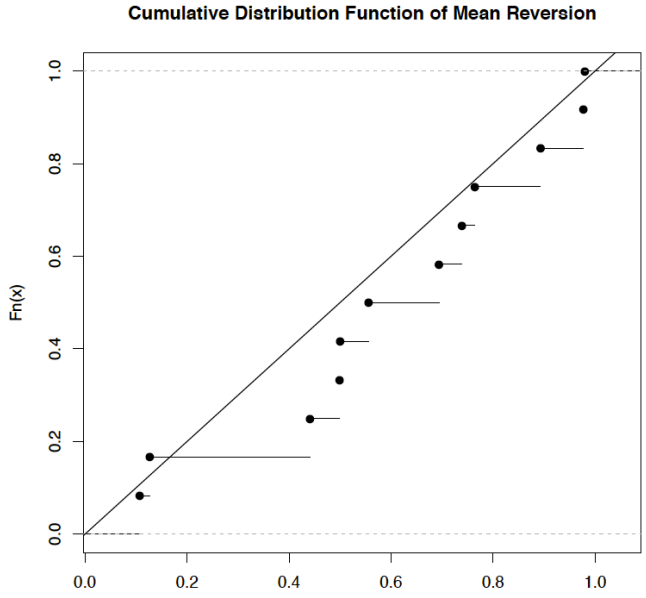


Figure 6: Randomization Check
P-Values of Democrat Appointee strings





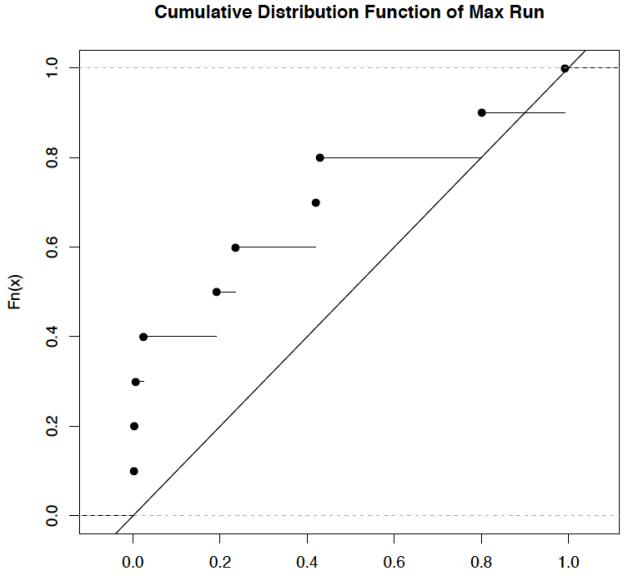


Figure 7A

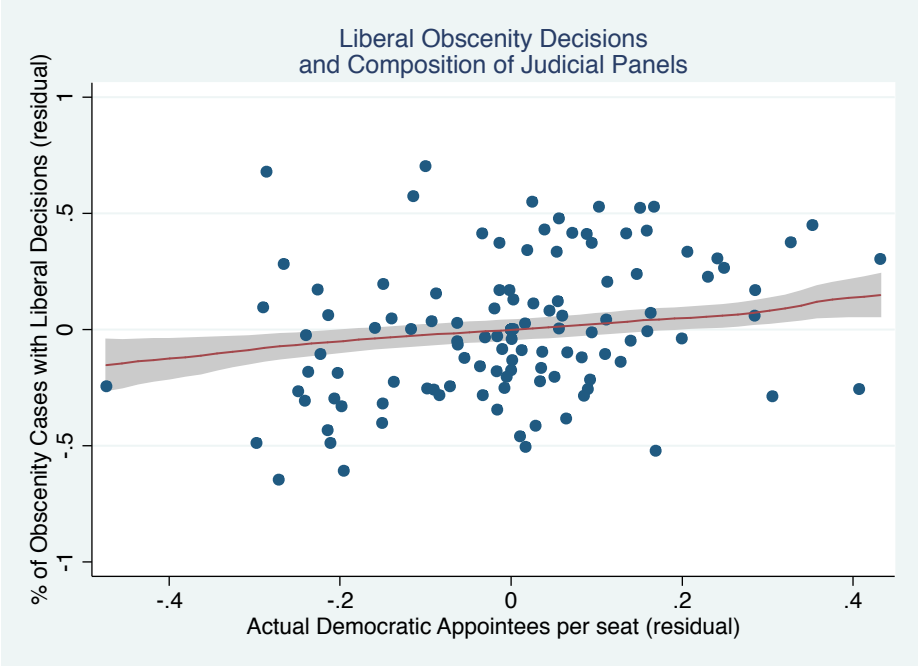
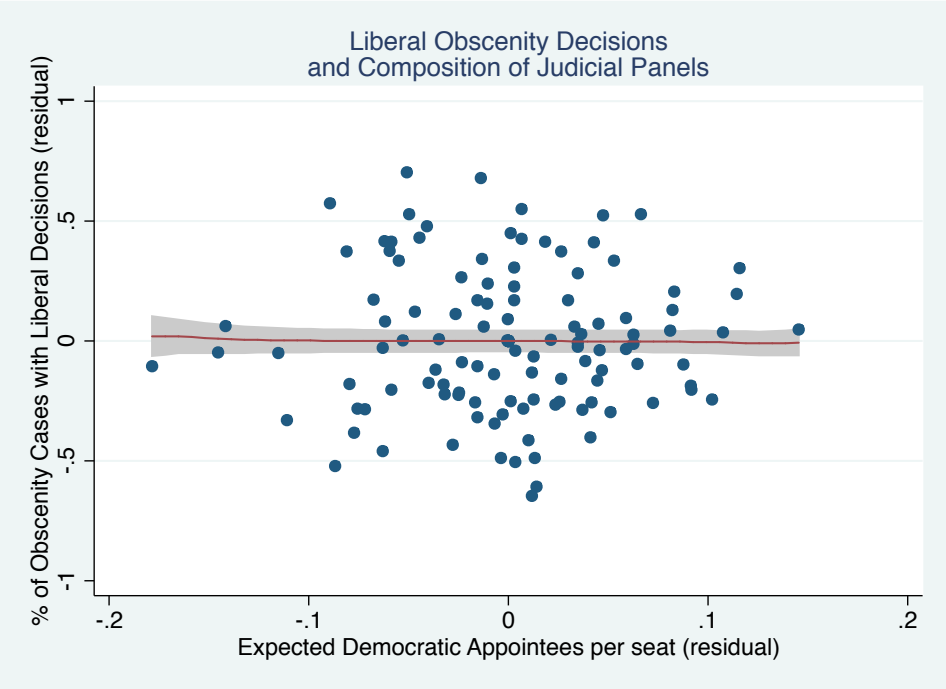


Figure 7B



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.

For Online Publication

A U.S. Legal Background: Tests Applied to Determine What is Obscene

Major doctrinal developments 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 the *Hicklin* test).

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

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

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, 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.

C Appendix 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)					
	(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	0.160	-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 (1)	P-value of lags (2)	P-value of leads (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

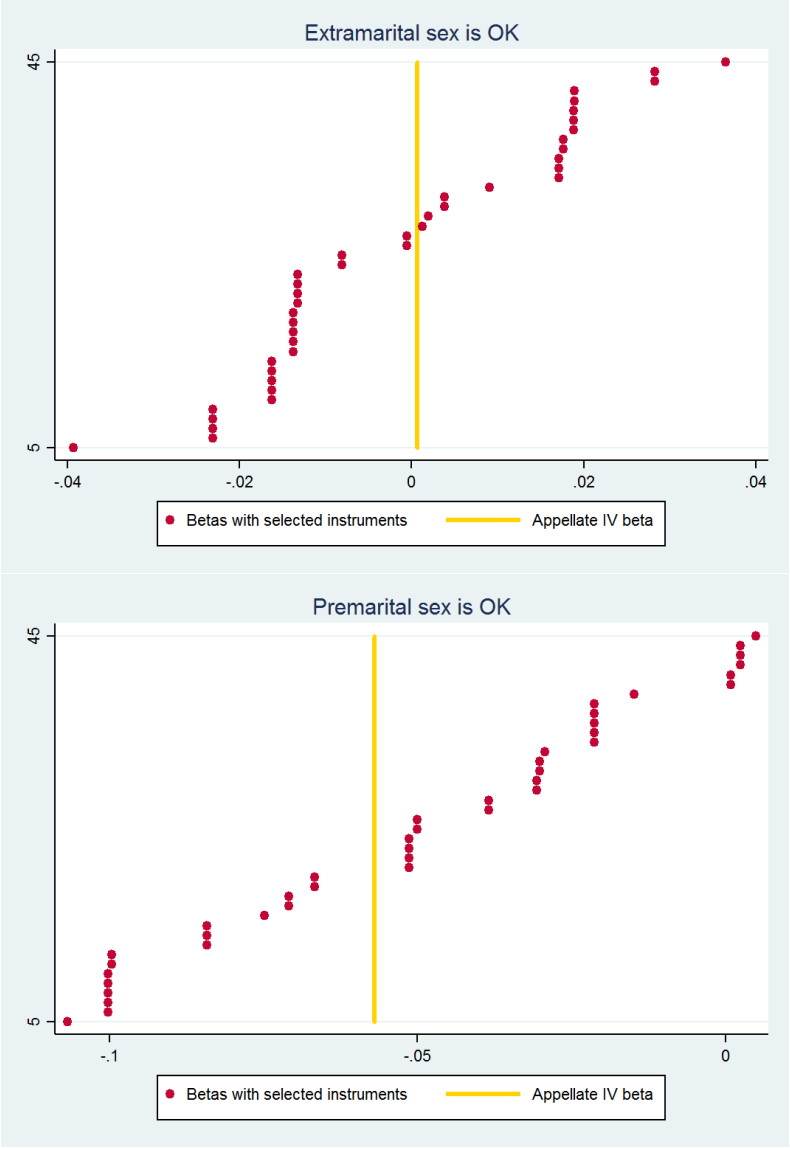
Dependent Variable	1973-1993								1980-2000							
	Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK		Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Proportion Progressive Free Speech	-0.00252	-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

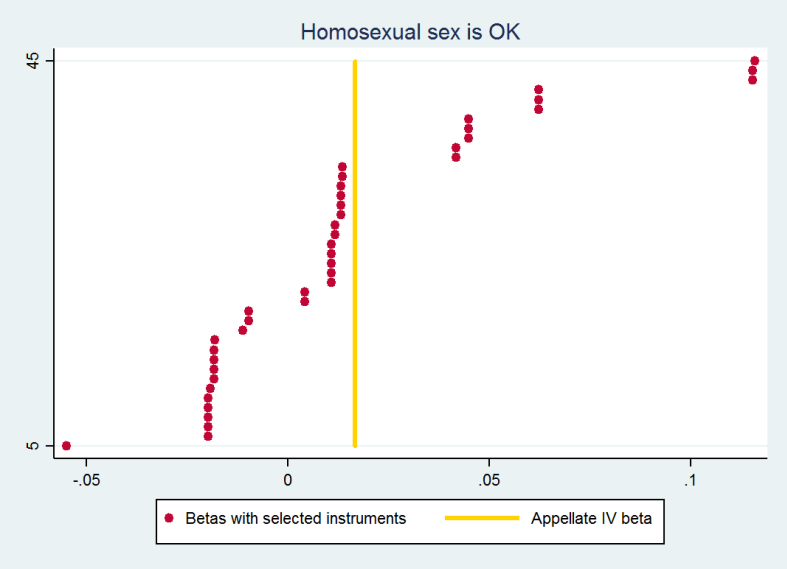
Notes: Significant at +10%, *5%, **1%. Attitudinal and behavioral data consist of individual GSS responses. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, Circuit-specific time trends, a dummy for whether 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.

D Appendix Figures

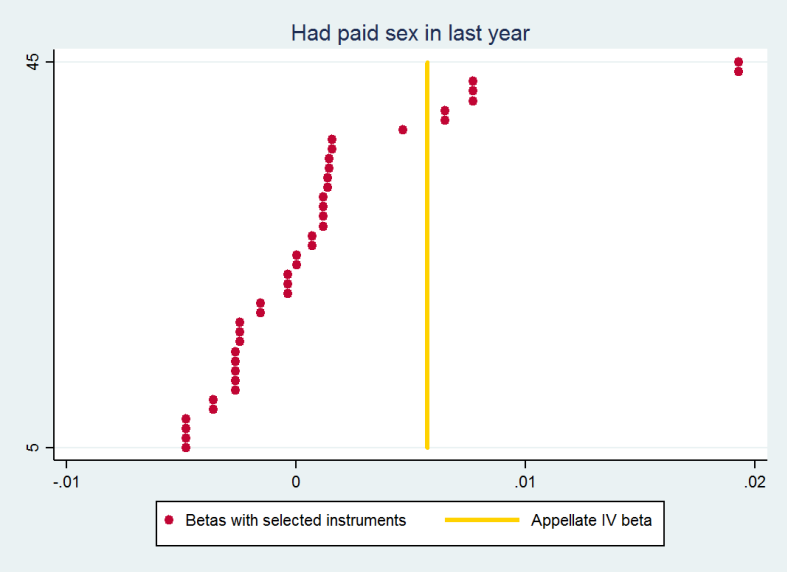
These figures present "visual Hausman" tests. We display the 2SLS estimates of $\frac{\sum_n \beta_{1n}}{n}$ using alternative instruments. The yellow line indicates the estimate from the "Appellate IV" specification where $Law_{c(t)}$ is instrumented for using the assignment of Democratic judges. The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

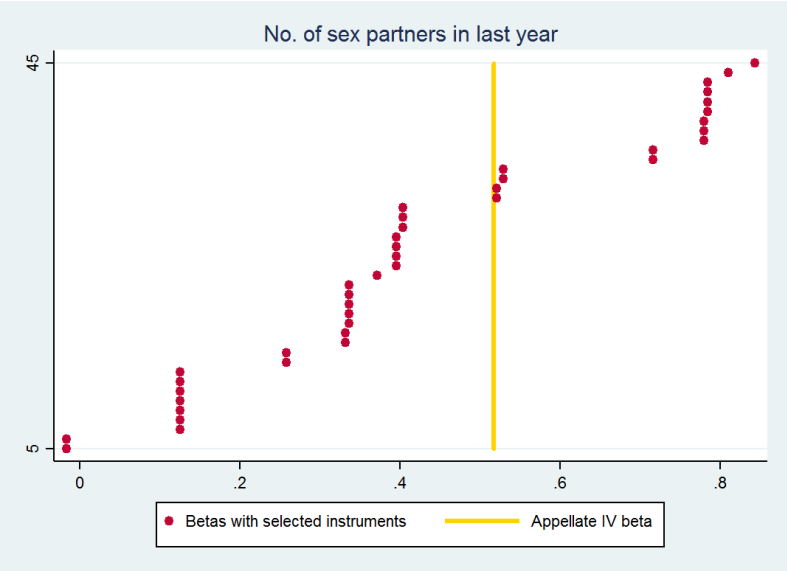
D.1 GSS attitudes

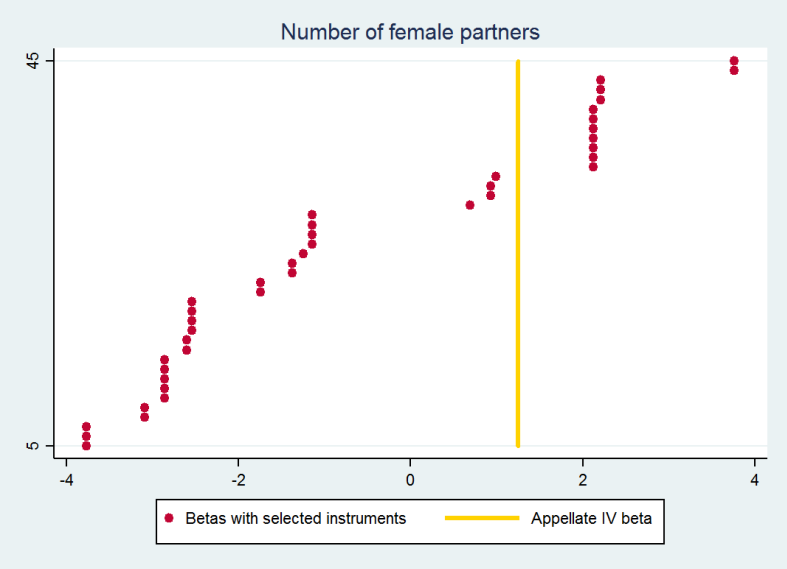




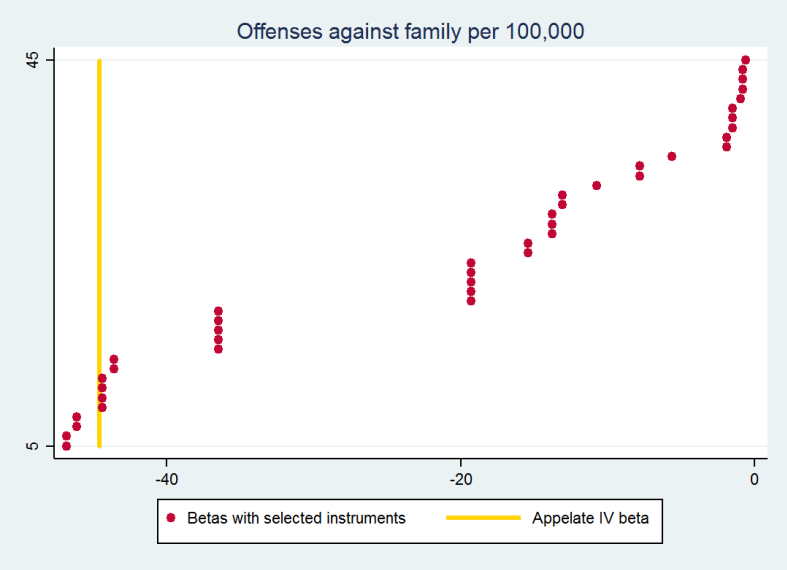
D.2 GSS behaviors

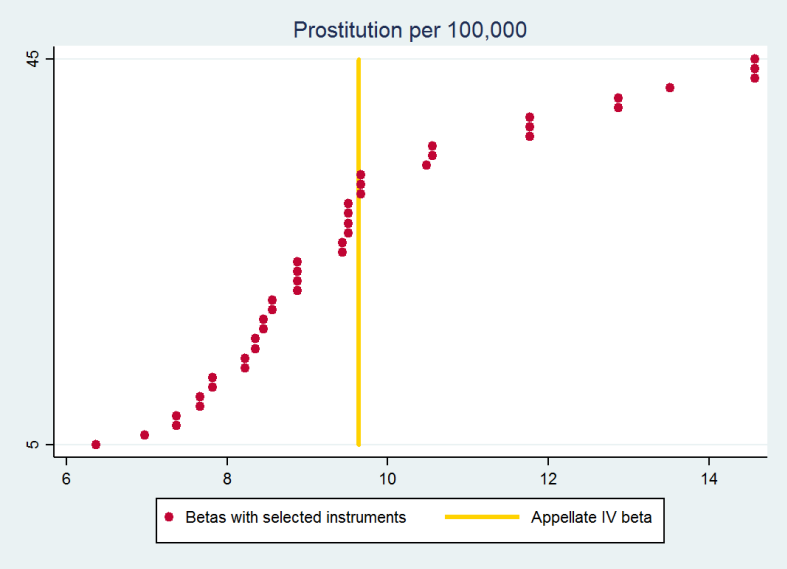


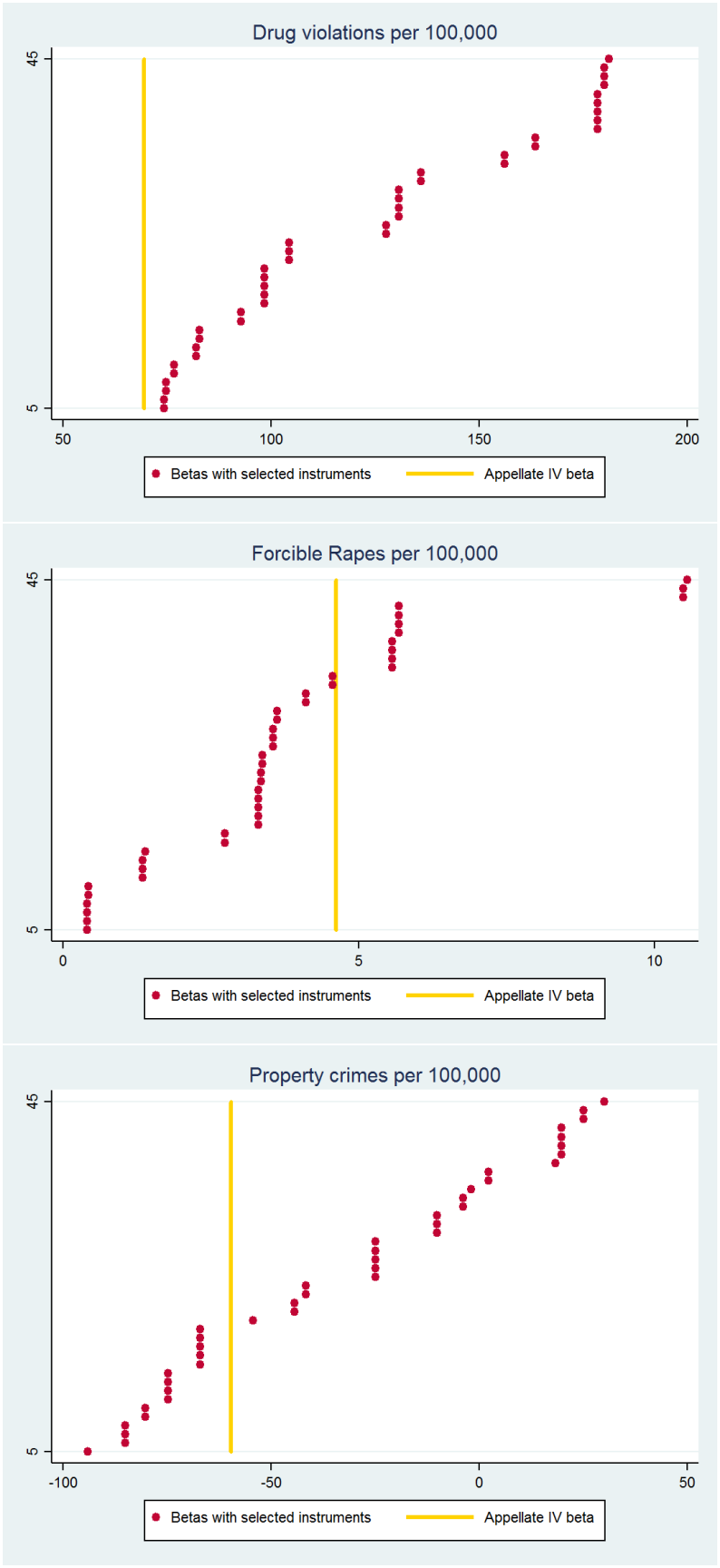




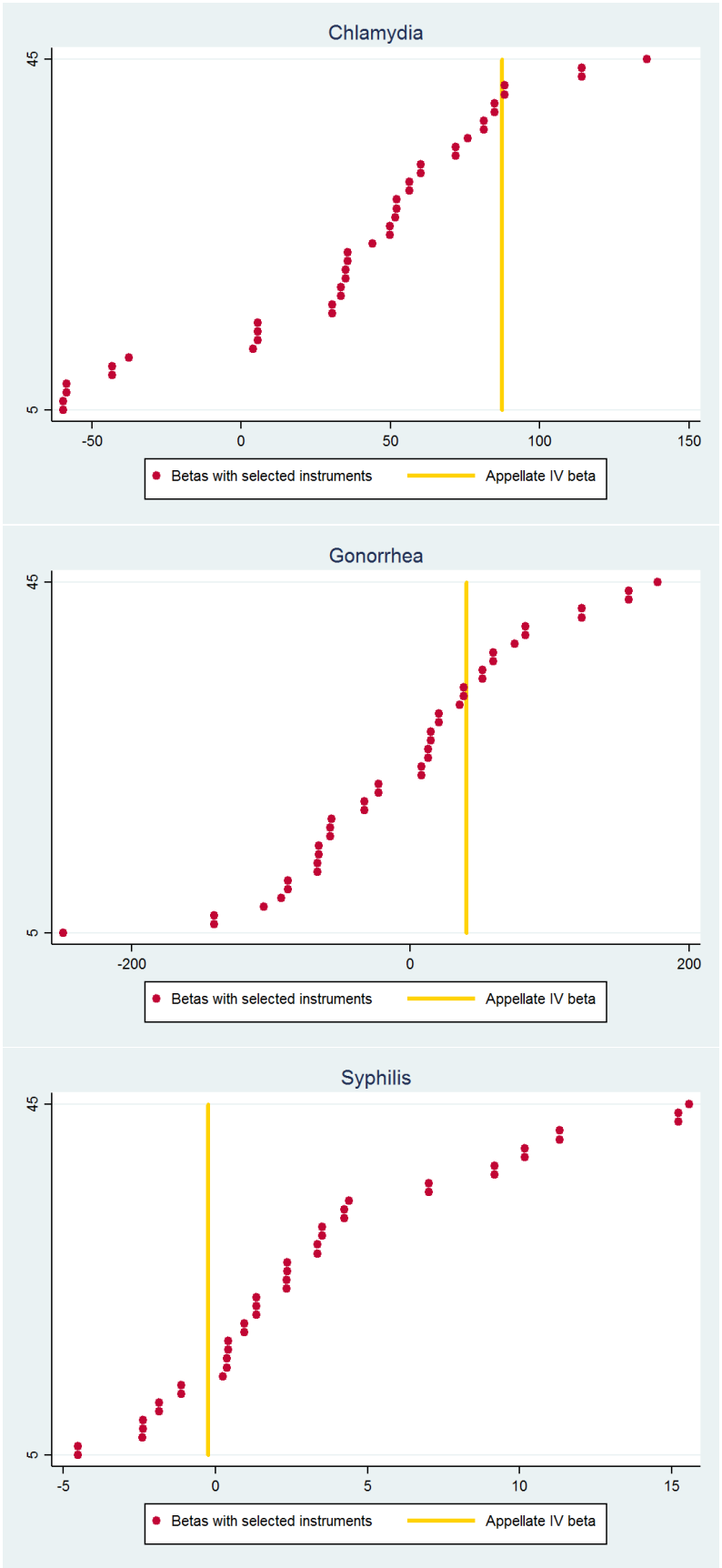
D.3 Crime data






Drug violations per 100,000
45
5
50
100
150
200
Betas with selected instruments
Appellate IV beta
Forcible Rapes per 100,000
45
5
0
5
10
Betas with selected instruments
Appellate IV beta
Property crimes per 100,000
45
5
-100
-50
0
50
Betas with selected instruments
Appellate IV beta

D.4 Disease data



E Counterfactual

This figure follows Bhuller et al. (2013) in showing the actual time trends for various crime outcomes, as well as the predicted counterfactual time trends based on the IV estimates. Going clockwise from the upper-left, the graphs report these effects for prostitution, drug violations, forcible rapes, and property crime. The scaling of the y-axis suggests the effect sizes are far closer to the effect sizes reported in Bhuller et al. (2013) than in Fernandez-Villaverde et al. (2014).

