

MEASURING THE MORAL AND ECONOMIC CONSEQUENCES OF JUDICIAL DISCRETION

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Abstract Do laws shape values? We test a model of law and norms using an area of law where economic incentives are arguably not the prime drivers of social change. From 1958-2008, Democratic judges were more likely than Republicans to favor progressive free speech standards. Using the random assignment of U.S. federal court judges setting geographically-local precedent, we estimate that progressive free speech standards liberalized sexual attitudes, behaviors, crimes, and diseases. We then randomly allocated data entry workers to enter newsarticles of court decisions. Progressive decisions liberalized sexual attitudes and shifted norm perceptions, but not self-reported behavior. These results present evidence of law's expressive power – with fundamental implications for decision making in social and political settings and for the empirical predictions of theoretical models in these domains.

Keywords: Law and norms, expressive law, cultural change

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

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

“[t]he protection given speech and press was fashioned to assure unfettered interchange of ideas for the bringing about of political and social changes desired by the people ... But implicit in the history of the First Amendment is the rejection of obscenity as utterly without redeeming social importance.” (Justice Brennan in *Roth v. United States*, 354 U.S. 476 (1957)).

“If we accept the unprovable assumption that a complete education requires the reading of certain books, ... and the well nigh universal belief that good books, plays, and art lift the spirit, improve the mind, enrich the human personality, and develop character, can we then say that a state legislature may not act on the corollary assumption that commerce in obscene books, or public exhibitions focused on obscene conduct, have a tendency to exert a corrupting and debasing impact leading to antisocial behavior?” (Justice Berger in *Paris Adult Theatre I v. Slaton*, 413 U.S. 49 (1973)).

1 Introduction

Where do normative commitments come from and what are their effects? Different groups often have different views on what they think is right or just, which can lead to group conflict. From abolition of slavery, to women’s liberation, to environmentalism, 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. Laws do not shape values in neoclassical models of law and economics, where only deterrence drives the response to law (Becker 1968); yet a large body of work in psychology suggests that laws can affect people’s behaviors simply by telling them what is the right thing to do (Tyler 2006; McAdams and Nadler 2008). Whether laws shape preferences—and in which direction—is important for those conducting cost-benefit analyses of judicial decisions and for those who want to know whether the decisions accord with the democratic will of the people, especially if judicial decisions shape the democratic will. Judge Richard Posner (1998b) has lamented that, “[judicial] opinions lack the empirical support that is crucial to sound constitutional adjudication” and Justice Breyer (2006) has remarked, “I believe that a[n] interpretive approach that undervalues consequences, by undervaluing related constitutional objectives, exacts a constitutional price that is too high.”

There are three empirical challenges to knowing whether laws shape values. The first is causal inference, which we derive from random variation of legal precedent. The second is distinguishing expressive from deterrence effects, since the behavioral response is in the same direction. To overcome this challenge, we employ an area of law, namely, obscenity law, since these laws are likely to have effects other than through economic sanctions alone because of their emotional salience and controversy. 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. Twentieth century free speech theorists were concerned with democratic deliberation (Habermas 1991), but contemporary theory intends free speech to promote individuals’ ability to participate in the growth and development of culture (Balkin 2004).

The third challenge is measuring values through revealed preference, since stated values could reflect shifts in the threshold for what kinds of survey responses are deemed acceptable. To be sure, shifts in stated values are independently interesting, but to examine shifts in revealed preference, we accompany the population-level analysis with an experiment. The experiment’s relatively short time frame means that deterrence is unlikely to explain the attitudinal changes, and the null response on self-reported behaviors is consistent with the population-level behavioral response reflecting revealed preference rather than shifts in acceptable survey responses.

Our use of a paired lab and field design complements previous papers on law and norms in experimental economics that use 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). Our paper also builds on a large experimental literature using revealed preferences on social preferences and moral behavior: measuring lying aversion with a die-rolling paradigm (Kajackaite and Gneezy 2015; Abeler et al. 2016; Gino et al. 2010; Hurkens and Kartik 2009); cooperation in moral dilemma games (Henrich et al. (2010); Rand et al. 2012; Cappelen et al. 2013); bargaining over killing surplus lab mice (Falk and Szech 2013); fairness in non-market and market interactions (Smith 1962; Roth et al. 1991; Bartling et al. 2015; Hoffman et al. 1994; Ross and Ward 1995); and the imposition of negative externalities on other lab subjects (Plott 1983). Our paper has the advantage of presenting causal identification in the field.

In the U.S., a fundamental rationale in judicial decisions has been to protect what it perceives as the moral fabric of society. Isolating the effects of laws from technological or other factors that facilitated norm change is challenging. In the U.S., the three-part *Miller* test and the *Roth* test 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, as laws mobilize individuals, are disseminated to the media, and are otherwise indirectly promulgated (Akerlof et al. 1996; Cooter et al. 2008).

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 (also known as Courts of Appeals) with regional jurisdiction setting legal precedent for millions of people. Between 1958 and 2008, 175 U.S. Courts of Appeals cases addressed free speech regulations of obscenity (Sunstein et al. 2006; Kastellec 2011), yielding roughly 124 experiments across 51 years and 12 Circuits. While this number may seem small, analysis of state laws usually examine the impact of up to 50 experiments in the U.S. or 34 in the OECD. Since Courts of Appeals are only to hear cases presenting new legal issues, cases representing big legal changes occur throughout the time period. Because judicial composition of Courts of Appeals cases is unlikely to be correlated with subsequent economic outcomes other than through the decisions, the random assignment of judges creates exogenous variation in legal precedent that can be used to estimate the causal impact of court-made law on values.

When law causes what is viewed as moral to shift towards what the law values, we label this

an “expressive effect”, and when law causes what is viewed as moral to shift against what the law values, we label this as “backlash”. Some economists have begun to model moral values and behavior. Confronted with mounting evidence that people are often pro-social, economic models were expanded under a broader rubric of incorporating fairness into economics (Rabin 1993), so that people care about more than the material consequences, but also the consequences for others (Fehr and Schmidt 1999), and what people think of one’s type or intentions (McCabe et al. 2003; Falk and Fischbacher 2006; Bénabou and Tirole 2006), and the social audience for one’s decision (Andreoni and Bernheim 2009). Just knowing that an observer will think badly of the decision-maker can be sufficient to compel moral behavior (Dana et al. 2006, 2007; Cilliers et al. 2015). “Homo Kantiensis”, whose preferences are ones that are socially optimal when everyone else also holds that view, is evolutionarily stable when preferences rather than strategies are the unit of selection (Alger and Weibull 2012). The Homo Kantiensis model implies that if people have “some positive degree of morality, then, in addition to ... taxes it may be effective to remind individuals of moral aspects of our conduct vis-a-vis the environment” (Alger and Weibull 2016). Other papers have modeled the conditions under which Kantian behavior emerges (Falk and Tirole 2016). Values have been cited as a rejection of markets (Roth 2007) and deviations from optimal economic policy (Mankiw and Weinzierl 2010).

We are guided by the only theoretical model in the scientific literature that we are aware of that allows both expressive and backlash effects to occur in response to law (Bénabou and Tirole 2012). We present a simplified version below and apply it to the context of free speech law in Section 2. The model assumes three motivations for human behavior: intrinsic motivations, extrinsic motivations (i.e., deterrence), and social motivations. 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., intrinsic motivations) 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 lawmaker 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. This expressive effect, however, only arises when a sufficient number of people perform law’s 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, backlash occurs.

In our application, the court issues a sanction that also informs people that more people are conducting law’s stigmatized activity than they previously thought. Since population-level 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 identify this parameter of the model and measure expressive apart from deterrence effects.

In Section 3, we present a method to test this model using random variation in jurisprudence. This method is presented in detail because of the recommendation that when it comes to observational data, “[f]or objective causal inference, design trumps analysis” (Rubin 2008). We first verify that judges are effectively randomly assigned and we confirm the divisiveness of social issues; as previous

research has documented, Democratic judges were significantly more likely than Republican judges to vote progressively in free speech cases (Sunstein et al. 2006; Songer and Haire 1992). Since many biographical characteristics, such as religion and race, strongly predict judicial decisions on social issues, we then employ LASSO to select predictive biographical characteristics to serve as instrumental variables (Belloni et al. 2012; Chen et al. 2014b). We cite additional evidence on information transmission of Courts of Appeals decisions, and present evidence that newspaper articles about Courts of Appeals obscenity decisions increase in the Circuits and years with decisions. We do not imply that law only affects individuals through newspaper reports—there is a wide range of media affected by free speech laws, and moreover, subsequent judges follow precedent. Rather, we present evidence on newspapers and we use newsarticles in our experiment because these data represent, arguably, the first instance when the law may affect some listener, actor, or thought leader, who then transmits the information.

In Section 4, we estimate the subsequent impact of free speech precedent on self-reported sexual attitudes and behaviors and on government statistics of crime and disease, which are the secondary consequences perceived to follow from progressive free speech decisions, according to judges and policymakers. These are the outcomes that motivated the U.S. federal courts for at least a half-century. Breakdown of moral standards (*Fort Wayne Books v. Indiana*, 489 U.S. 46 (1989)) and secondary effects, such as sexual violence (*Amatel v. Reno*, 156 F.3d 192 (D.C. Cir. 1998)), child sexual abuse (*Ginsberg v. New York*, 390 U.S. 629 (1968)), disease and drugs (50 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 occurred for gonorrhea and syphilis, and one reason may be sorting among sexual partners based on their disease status, a mechanism that has been formally modeled in the economics literature (Kremer 1996).

As to what range of impact estimates are reasonable, we are guided by the only set of empirical results in the causal inference literature that we are aware of. These papers attribute 3.2% of rapes and 2.5% of sex crimes and child sex abuses from 2000–2008 to the expansion of internet broadband (possibly due to consumption of obscene content, according to the authors) (Bhuller et al. 2013), 7% of the probability of giving birth from 1980–1991 to portrayals of intimate relations on television (La Ferrara et al. 2012), 10% of divorce and separation to broadcast of images critical of traditional values (Chong and Ferrara 2009), 8% of the number of situations where domestic violence toward women was deemed acceptable, 15% of preferences for sons, and 52% of the likelihood of pregnancy to the introduction of cable television (Jensen and Oster 2009), 7% of the number of social groups

to television and radio (Olken 2009), and 25-30% of the likelihood that women had ever used oral contraception to the U.S. Supreme Court decision *Griswold v. Connecticut*, which struck down contraception bans and altered state laws (Bailey 2010). It is important to note that these media are a subset of all mediums affected by free speech jurisprudence. Our paper presents the advantage of being based on randomization.

Whether these effects seem large or small depends on one’s perspective. Structural estimates attribute 50% of the sexual revolution to individuals’ moral views on sexual rights, shaped by laws and doctrine (Fernandez-Villaverde et al. 2014). Some studies document some mechanisms by which laws can affirm moral standards or 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). According to recent summaries of the literature, the majority of laboratory experiments find support for secondary effects. Bhuller et al. (2013) and Baron and Straus (1984) find that this translates into higher crime rates in the field. 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)). 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, 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.

The interpretation of all of these studies—and ours—is subject to the usual caveats in the literature—causal effects are sufficient, but not necessary conditions for an outcome (Deaton 2010).¹ As we see it, the objective of the study is to communicate the information embodied in a data set on law and norms, with explicit recognition of the model uncertainty present in the analysis (Leamer 1978; Sala-i Martin, Doppelhofer and Miller 2004; Hansen 2007; Cohen-Cole, Durlauf, Fagan and Nagin 2009). This leads us to present many estimates of the model and leave the reader to take a weighted average corresponding to their preferences, or interpret the range of point estimates as partial or set identification. We subject our baseline 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

¹A defendant who shares the same first initial as a judge receives 8% longer sentences, but this effect only explains 0.03% variance (Chen and Prescott 2016). See also Deaton’s NYU “Debates in Development” lecture on the topic, where he describes causal effects as Insufficient but Non-redundant parts of a condition which is Unnecessary but Sufficient (INUS).

distributed lag structure, and vary the biographical characteristics used to isolate exogenous shifts in free speech precedent. We include controls for Circuit-specific time trends and composition of the pool of judges available to be assigned to panels; drop 1 Circuit at a time; vary the lag structure; use wild bootstrap and simulations that randomly assign legal variation to another Circuit. As Barrios, Diamond, Imbens, and Kolesar (2012) write, “if the covariate of interest is randomly assigned at the cluster level, only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals,” which implies similar standard errors across the different methods of accounting for clustering at the Circuit or Circuit-year level. We also present tests of randomization, assess additional identification concerns, and present robustness to accounting for the potential for litigants to pursue an appeal in response to prior years’ outcomes by using the random assignment of judges in the lower courts (District Courts) from which an appeal arises as an additional source of exogenous variation.

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 indeed affects attitudes in the absence of deterrence. We present equations that map the experimental and population-level estimates to put bounds on the total indirect effects of the law for individuals not directly exposed to free speech precedent. We report two experiments in Section 5 and the third in Chen and Yeh (2014a), all of which show, across a total of 1,345 subjects, the expressive effects of law.

Data entry workers were assigned to transcribe newspaper summaries of free speech decisions that were randomized to be progressive or conservative. We then asked the same set of attitude and behavior questions as in the population-level analysis. The first experiment recruited 197 workers from around the world. It found that 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. The second experiment restricted workers to being from the U.S. and surveyed attitudes and an important parameter for the model—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. In terms of beliefs, these workers exposed to progressive laws reported believing a lower percentage of people having extramarital sex than those exposed to conservative laws, verifying the information multiplier proposed by Bénabou and Tirole (2012).

The theoretical model of Bénabou and Tirole (2012), with simplifying assumptions that we present in Section 2, also suggests whether law has expressive or backlash effects depends on the underlying social norms. When law’s stigmatized activities were relatively scarce, these activities became normalized when conservative free speech decisions caused people to update their beliefs that the stigmatized activities were more common than previously thought. This update in perceived prevalence, in turn, caused more people to do the stigmatized activity, which eventually becomes destigmatized. In our data, a large number of free speech decisions occurred 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 judicial decisions led to backlash—an increase in the perceived morality of homosexual sex and an increase in the incidence of non-marital sexual behaviors. In the aftermath of the moral revolution, law had expressive effects. This result may be due to sampling variation, but supporting evidence is found experimentally in Chen and Yeh (2014a). In communities where law’s stigmatized activity is rare, progressive free speech precedent caused data entry workers to backlash, and also lowered their subjective well-being. The opposite occurred for individuals in communities where law’s stigmatized activity is more common.

Besides the effects of law on attitudes, several additional pieces of evidence are consistent with expressive rather than deterrence effects as the primary channel to explain the findings. First, the role of material penalties is unlikely to be significant in the short time frame of our experiments. Second, backlash effects would not be explained by deterrence. Third, 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 result also is consistent with the effects found in the arrest data reflecting actual changes in underlying behavior, rather than simply changes in law enforcement aggressively making arrests in response to court decisions. Fourth, we also collected data on state-level sales of pornographic magazines as pornography media providers were often parties in free speech litigation. Magazine circulation did not respond to free speech decisions, though we cannot rule out a possible change in content or change in other channels for obscene content that reach the public.

Our study focuses on the more basic and timeless question of whether laws influence conceptions of rights, albeit in a very particular setting across three experiments and the field. A leading theorist of free speech articulated the primary value of guaranteeing free speech to be individual self-realization, from which follows liberty, autonomy, self-fulfillment, and human development (Redish 1982). If progressive free speech precedent liberalized sexual attitudes, behavior, crime, and disease, and increased subjective well-being in communities where law’s stigmatized activity is prevalent, then these effects would be consistent with these goals, at least for some communities.

2 Theory

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

the contemporary goal of free speech is to promote each individual’s ability to participate in the growth and development of culture.

More broadly, an open question in international law is whether custom can be shifted in the direction intended by formal institutions (Aldashev et al. 2012). Since 1973, the legal standard defining obscenity in the U.S. has been the three-part *Miller* test set out in the Supreme Court decision *Miller v. California*, 413 U.S. 15 (1973). The *Miller* test defines material as obscene if “the average person, applying contemporary community standards” would find that the material (1) “appeals to the prurient interest”; (2) has “patently offensive” depictions of sexual conduct; and (3) “lacks serious literary, educational, artistic, political, or scientific value.” Before the *Miller* test, the *Roth* test allowed banning obscenity when the average person, applying contemporary community standards, would consider the dominant theme of the material, taken as a whole, appeals to prurient interests.

Moral harms and their “secondary effects” (i.e., sexual violence, disease and drugs) were discussed in the Supreme Court decisions *Young v. Adult Mini Theatres, Inc.* 427 U.S. 50 (1976) and *Renton v. Playtime Theatres, Inc.* 475 U.S. 41 (1986) regarding obscene speech. Anti-pornography advocates assert that regulation is necessary to communicate social values and protect human welfare. For example, Radin (1996) argues that the failure to regulate pornography would lead to the commodification of the body and endanger women, and the link between commodification and gender violence can be formalized in a model of incomplete contracts (Chen 2004). Though stressing that morality is not the focus (MacKinnon 1987), 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.

It is widely presumed that law affects moral values and behavior simply through its expressive power (Cooter 1988; McAdams 2000; Posner 1998a, 2000; Kahan 1997; Sunstein 1996). Laws are theorized to influence the population through moralizing language designed to affect social norms and ultimately judgment and decision-making, whether 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 (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).

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

Based on the theory of expressive law, scholars in a wide range of legal areas have made nor-

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

Sexual norms have changed dramatically during the time period of our study. As noted by Fernandez-Villaverde et al. (2014), in 1958, 35% of U.S. women engaged in premarital sex by the age of 19 compared to 75% today. They estimate however, if individuals' moral views had not changed, a little over 50% of U.S. women would have had premarital sex by the age of 19 today. Changes in moral views include: In 1968, only 15% of women had a permissive attitude towards premarital sex, but this increased to 45% by 1983. In 1957, 57% of Americans believed that adults who preferred to be single were "immoral", but today, it is no longer considered a moral issue and more than 50% of adults are single. Bearing children out-of-wedlock was once extremely rare, but today more than half of births to women under 30 occur outside of marriage (Klinenberg 2012). The American Association of Retired Persons (AARP), founded in 1958, provided retirees with advice on sexting in 2009 (Leshnoff 2011). Fernandez-Villaverde et al. (2014) report that between 1710 and 1750, 69% of all criminal cases in New Haven were for premarital sex, which was punished by fines, jail, and public flogging. Five times in the last 25 years, the South Korean Constitutional Court has decided on the legality of a law that makes adultery a crime, and in the past six years alone, 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: can formal institutions shift custom? What causes a rights revolution and what role does the law play? Historical studies of the advent of the sexual revolution document backlash by conservatives to stop the Supreme Court from encroaching on state rights to control pornography during the 1950s and 1960s. From 1959 to 1966, bans on three books with explicit erotic content were challenged and overturned. Prior to this time, a patchwork of regulations, local customs, and vigilante actions governed what could and could not be published. For example, the United States Customs Service banned James Joyce's *Ulysses* by refusing to allow it to be imported into the United States. Different cities and organizations had their own rules for allowable content. The Warren Court greatly expanded civil liberties and in *Memoirs v. Massachusetts* and other cases curtailed the ability of municipalities to regulate the content of literature, plays, and movies. For six years, it reversed summarily—without further opinion—scores of obscenity rulings by lower state and federal courts, culminating in the 1969 decision² that held that people could view whatever they wished in the privacy of their own homes.

The last ruling led the U.S. Congress to fund the President's Commission on Obscenity and Pornography. Yet, the 1970 Commission's findings that there was "no evidence to date that expo-

²*Stanley v. Georgia* (394 U.S. 557)

sure to explicit sexual materials plays a significant role in the causation of delinquent or criminal behavior among youths or adults”, “no evidence that exposure to explicit sexual materials adversely affects character or moral attitudes regarding sex and sexual conduct”, and conclusion that “legislation prohibiting the sale, exhibition, or distribution of sexual materials to consenting adults should be repealed” were roundly rejected and criticized by Congress. In the immediate aftermath, opposing groups authored minority reports that dissented with the Commission’s view, which was subsequently cited by the U.S. Supreme Court in later conservative decisions. When Chief Justice Warren was to be replaced by Justice Fortas, a conservative group led by Senator Thurmond organized the “Fortas Obscene Film Festival,” (it featured transvestites) which not only led to the resignation of Justice Fortas but also the nomination of Justice Burger instead, who by 1973 issued the *Miller* test which repudiated the “utterly without redeeming social value” standard from *Memoirs* in favor of the markedly less liberal “lacks serious literary, artistic, political, or scientific value” (Boyce 2008). Group conflict arises over social preferences and sacred values (Bowles and Polania-Reyes 2012; Chen et al. 2006; 2010; 2013; 2015). The 2016 Republican Party platform has declared, “Current laws on all forms of pornography and obscenity need to be vigorously enforced” and that “Pornography, with its harmful effects, especially on children, has become a public health crisis that is destroying the life of millions”.

Little is known about when law causes what is viewed as moral to shift towards or against what the law values. Little is known about what regulations on obscene speech actually do and whether the rationale put forward by policy makers and scholars of female empowerment concerned about the commodification of women and the potential deleterious secondary effects (i.e., sexual violence, child sexual abuse, disease and drugs) are empirically justified.

Several studies have linked major court rulings with subsequent changes in public opinion where the case originates and suggested that media plays a prominent role (Hoekstra 2000).³ Information entrepreneurs, such as community organizations raising awareness can also act as a catalyst. For example, Weinrib (2012) documents how, in response to major Courts of Appeals 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).

Several U.S. Supreme Court decisions and the Warren Court’s great expansion of civil liberties coincided with the onset of the sexual revolution. Contraceptive use has been linked to the sexual revolution (Akerlof et al. 1996). Free speech jurisprudence affects the government’s ability to regulate mail, magazines, books, television, movies, internet, and phone calls - all of the media channels analyzed by previous authors on the cultural impact of these channels (La Ferrara et al. 2012; Chong and Ferrara 2009; Olken 2009; Jensen and Oster 2009; Bhuller et al. 2013; Bailey 2010).

Our study also relates to contemporary debates over same-sex marriage and discrimination, an area of significant social change in recent years. Though we emphasize that our legal cases are about obscenity as defined in its historical context and not gay rights per se, of the 175 free speech

³See, 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.

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.⁴ Though this paper answers a question different from that addressed in the usual research on law and norms, it has the advantage of a relatively clear source variation that allows identification of any effects in a paired lab and field setting. It also differs from previous papers analyzing the causal effects of law by examining its expressive effects.

2.2 Model Set-Up We present a simplified version of the only theoretical model in the scientific literature that we are aware of that allows both expressive and backlash effects to occur (Bénabou and Tirole 2012). We define “expressive effects” as occurring when the law shifts moral attitudes and behavior in a direction the law intended, and “backlash effects” as when the law does the opposite.

The theoretical framework is intended to guide discussions on the use of law in social change, and assist in understanding when laws have expressive as opposed to backlash effects. The model builds 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).

Two sets of multipliers are key. 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 reasoning motivating behavior, see Brekke et al. (2003); Andreoni (1989); Chen et al. (2015a); 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).

Individuals maximize the following utility function:

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

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

⁴http://www.nytimes.com/2012/05/21/nyregion/Some-Gay-Rights-Advocates-Question-Rutgers-Sentencing.html?_r=1&hp

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

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

The judge set the costs and $\sigma_j \bar{a}$ represents the systematic component of judge j 's decision-making that leads her to value the public good $\bar{a}$ more or less than other judges. λ is the shadow cost of resources used as incentives like enforcement costs. With exogenous variation in y in our empirical framework due to random assignment of judges with different σ_j , we focus on the behavior of the agent.

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

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

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, and this assumption is buttressed by the General Social Survey. In the aforementioned countries, India, Russia, and the U.S., exercising free speech rights related to obscenity corresponds to $a = 0$ and abstaining from free speech corresponds to $a = 1$. $e > 0$ captures judicial concerns that exercising free speech leads to some harm.

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

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

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

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

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

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

where we define:

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

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

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

A sufficient condition for a fixed point is if $1 + \mu\Delta'(v) > 0$, in which case $[\underline{v}, v^*]$ share of the population exercise free speech (or whatever is the exercise that judges were concerned about). Note that the action need not be observable by others for the model to apply. $E(x | a)_s$ could also capture one's own perception of intrinsic motivations in a self-signaling 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, respondents 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 this GSS question because μ is the weight that respondents put on the morality of an action.

2.4 Social Multiplier To understand this sufficient condition, note that $v^* + \mu\Delta(v^*)$ is the marginal benefit of exercising free speech for people at the cutoff. The marginal benefit is the sum of intrinsic motivation and social motivation. c is the marginal cost. The intuition for the sufficient condition is as follows. If $1 + \mu\Delta'(v) > 0$, then as the cut-off increases, the marginal benefit will eventually equal the marginal cost c , which is constant, and that cut-off will be a fixed point. The more people who exercise free speech, the more honor associated with abstaining from free speech, which means the less others will exercise free speech. While $1 + \mu\Delta'(v) > 0$ is a sufficient condition for a fixed point, it is not a necessary condition, which is explained in more detail in Bénabou and Tirole (2012). In particular, $\Delta'(v) < 0$ is possible, when a small perturbation leads to rapid social changes as society moves from one steady state to another amid a moral revolution.

See Figure 1 for a distribution of intrinsic motivations. Under Jewitt's (2004) lemma, the shape of Δ mirrors the density of v . Δ initially decreases, then increases. Intuitively, this is because adding a small mass around the cut-off will shift one truncated mean more than the other. When v^* is small (most people choose $a = 1$), raising v^* increases $E(v_a | 0)$ more than $E(v_a | 1)$, as $E(v_a | 0)$ includes very few points on the left tail of the v -distribution. Slightly increasing the support of the truncated distribution to the right adds a large share of individuals with high v 's. In contrast, $E(v_a | 1)$ is less affected. In words, the 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. The more people who exercise the targeted free speech, the more normalized it becomes, so the more others will exercise the targeted free speech as well: $\Delta'(v) < 0$. Multiple equilibria can arise if complementarity is strong enough or μ is large enough. When $1 + \mu\Delta'(v)$ is negative, there may be unstable equilibria.

2.5 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 signaling 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 progressive communities.

To summarize, explicit sanctions indicate that the policymaker sees a problem. The judge has information about v^* and issues a sanction when she believes v^* is too high. Upon observing the decision, community leaders and individuals update their beliefs about the underlying distribution.

When exercise of free speech is common, v^* is on the right side of the distribution, so free speech decisions have expressive effects. The model embeds conflicting qualitative discussions in the literature on laws having backlash or expressive effects: (1) laws have expressive effects when v^* is high (the density of v is falling) and (2) laws have backlash effects when v^* is low (the density of v is increasing). Since laws can have either expressive or backlash effects, the theoretical ambiguity motivates an empirical analysis.

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.

2.6 Testing the Empirical Predictions The most robust prediction of the model, which sets it in contrast with a world where deterrence is the sole channel through which law affects behavior, is that normative views are likely to differ in Circuit-years that issue progressive free speech jurisprudence. To test this, we will simply compare the outcomes in Circuit-years with progressive and conservative free speech jurisprudence and perform robustness checks to confirm that the difference seems to be due to free speech jurisprudence.

The expression $\Delta(v)$ is operationalized in questionnaires like the General Social Survey (GSS), where people are asked about the morality of particular actions. By reporting what is their perceived morality of an action, respondents report the difference in the social perception of someone who chooses $a = 1$ vs. the social perception of someone who chooses $a = 0$.

To link legal precedent with outcomes, we propose a constant returns to scale model linking behavior with values. Let $v^* = A\Delta(v) = \Phi(\Delta(v)) = \Delta(v)\phi(\Delta(v))$, where cut-off v^* is behavior in society, and $\Delta(v)$ is values. We parameterize $\phi(\Delta(v)) = e^{\rho Law_{ct}} A_{ct}$.

Consider a general dynamic equation for behavior (and dropping the superscript on v):

$$(8) \quad v_{ct} = A_{ct} + \alpha_1 v_{ct-1} + \dots + \alpha_n v_{ct-n} + \rho_0 Law_{ct} + \rho_1 Law_{ct-1} + \dots + \rho_n Law_{ct-n} + \varepsilon_{ct}$$

allowing behavior to depend on n lags of past behavior and adding an error term.

We assume that A_{ct} evolves according to:

$$(9) \quad \Delta A_{ct} = g_c + \gamma_0 Law_{ct} + \dots + \gamma_n Law_{ct-n}$$

This allows both current and lagged jurisprudence to affect the growth rate of A .

Substituting ΔA_{ct} into a first differenced version of v_{ct} yields a dynamic panel estimation equation of the form:

$$(10) \quad \Delta v_{ct} = g_c + \alpha_1 \Delta v_{ct-1} + \dots + \alpha_n \Delta v_{ct-n} + \gamma_0 Law_{ct} + \dots + \gamma_n Law_{ct-n} + \rho_0 \Delta Law_{ct} + \rho_1 \Delta Law_{ct-1} + \dots + \rho_n \Delta Law_{ct-n} + \Delta \varepsilon_{ct}$$

The “level effects” of law on behavior appear through ρ . The “growth effects” of law appear through γ . Rewriting the ΔLaw terms as Law terms yields:

$$(11) \quad \Delta v_{ct} = g_c + \alpha_1 \Delta v_{ct-1} + \dots + \alpha_n \Delta v_{ct-n} + (\gamma_0 + \rho_0) Law_{ct} + (\gamma_1 + \rho_1 - \rho_0) Law_{ct-1} + \dots + (\gamma_n + \rho_n - \rho_{n-1}) Law_{ct-n} - \rho_n Law_{ct-n-1} + \Delta \varepsilon_{ct}$$

Relabeling the coefficients on Law yields:

$$(12) \quad \Delta v_{ct} = g_c + \alpha_1 \Delta v_{ct-1} + \dots + \alpha_n \Delta v_{ct-n} + \sum_{j=0}^{n+1} \beta_j Law_{cj} + \Delta \varepsilon_{ct}$$

To find the growth effect, consider $\Delta v_{ct-j} = \Delta v$ and $Law_{cj} = Law$. Solving yields:

$$(13) \quad \Delta v_c = \frac{g_c}{1 - \alpha_1 - \dots - \alpha_n} + \frac{\sum_{j=0}^{n+1} \beta_j}{1 - \alpha_1 - \dots - \alpha_n} Law_c$$

so that the growth effect of jurisprudence is simply $\frac{\sum_{j=0}^{n+1} \beta_j}{1 - \alpha_1 - \dots - \alpha_n}$, which is identical to $\frac{\sum_{j=0}^n \gamma_j}{1 - \alpha_1 - \dots - \alpha_n}$ since the ρ terms all cancel.

As we find variation in Law that is randomly assigned, we focus on $\alpha_j = 0$ for all j :

$$(14) \quad g_{ct} = \theta_c + \theta_t + \sum_{n=0}^L \beta_{t-n} Law_{ct-n} + \Delta \varepsilon_{ct}$$

where θ_c are Circuit fixed effects, θ_t are time fixed effects, and Law_{ct} is a vector of annual jurisprudence with up to L lags included. This equation captures the growth effect of law, e.g., the effect of law on features, such as institutions that influence moral revolutions.

The growth equation allows separate identification of level effects and growth effects through the examination of β_j . In particular, both effects influence the growth rate in the initial period. The difference is that the level effect eventually reverses itself. For example, a jurisprudential shock may affect community activism, but after a few periods, activism returns to normal. By contrast, the growth effect appears during the jurisprudential shock and is not reversed. A failure to innovate in one period leaves the Circuit permanently further behind. The growth effect is identified as the summation of the jurisprudential effects over time. Following the convention in the growth literature, we are interested in the distributed lag effect and test for joint significance of the lags.

Another way to think about our research design is that laws are not likely to have an immediate impact. Individuals may need time to adjust to a new legal regime; alternatively, the effects of a law change may fade as expectations adjust. To estimate the delayed effects of the law, we estimate a distributed lag specification.

Our data on audits of behavior can be interpreted as a growth equation because the disease data is reported as incidence, the number of new cases. Our data on values and behaviors, however, are repeated-cross sections, so we will estimate the growth equation in levels. This means that a level effect is inferred from a persistent set of lags and a growth effect from a set of lags that grow over time. In contrast, convergence in values and behavior is inferred from lags that fade over time.

In sum, the model of law and norms makes possible competing views of the effect of law on values. Free speech jurisprudence updates individuals on the distribution of actions, which affects their views of the morality of the targeted free speech.

3 Design of Field Study

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. This endogeneity is suggested—and then embodied—in the following sample of over a century of legal doctrine that instructs future judges how to decide:

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.” This was 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*).

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. The regulation must also satisfy the necessary Constitutional criteria such as not being overbroad or vague. 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. Each state has 1-4 District Courts and each Circuit Court presides over 3-9 states. There are a total of 12 Circuits, which decide *issues of law*; they take facts as given from District Courts and have no juries. (There is also a Federal Circuit, which mostly handles intellectual property cases.) 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). 98% of their decisions are final. In the remaining 2% that are appealed to the Supreme Court, 30% are affirmed. State officials regularly update a set of guidelines to identify actions and regulations that may result in costly litigation after Courts of Appeals decisions (Frost and Lindquist 2010; Pollak 2001).

In deciding issues of law, Courts of Appeals provide new interpretations or distinctions of pre-existing precedents or statutes. These new distinctions expand or contract the space under which an actor is found liable (Gennaioli and Shleifer 2007). For example, *Young v. American Mini Theatres, Inc.*, 427 U.S. 50 (U.S. 1976) declared constitutional a city ordinance that prohibited adult movie theaters from being located within 1000 feet of any two other “regulated uses” (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.

Each Courts of Appeals 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. 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. Some judges take a reduced caseload if retired or visiting, but all are randomly assigned by a computer algorithm. From discussions with government officials at the Courts of Appeals, it appears that randomization occurs. More importantly, in Chen and Sethi (2012), they formally test for randomization by showing that case characteristics as determined by District Courts are not correlated with the characteristics of the Courts of Appeals judges assigned to the case.

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). Historians have also documented that judges rely on personal values influenced by historical forces (Klarman 2004). The courts are polarized by group identity, personal experience, and legal philosophy. For example, Courts of Appeals judges behave more partisan before Presidential elections, wins and losses of sports games affect judicial decisions, ideological perfectionism affects decisions, decisions on recent cases affect the next decision, and explanatory power of extraneous factors persist after employing the best prediction models of judges' decisions (Barry et al. 2016; Berdejó and Chen 2014; Chen 2016; Chen and Eigel 2016; Chen et al. 2015a; 2014; 2015b; 2016d; 2016c).

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. This also means that the composition of judges in any one legal area is not correlated with the composition of judges in another legal area because of random assignment. 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.

Our identification strategy also exploits the random assignment of District Court judges. The demographic characteristics of District judges are correlated with whether the District judge is reversed by the Courts of Appeals (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 control for the presence of a Courts of Appeals case. To be sure, we are not identifying the causal effects of the presence of a Courts of Appeals case. The assignment of a District judge can affect the Circuit panel's decision. We want to identify a portion of the law that is not coming from other social trends or areas of law, and this portion comes from the random assignment of federal judges.

Our causal inference therefore comes from the random assignment of judges who interpret the facts and the law differently. This exogenous variation in establishment of precedent across different

regions allows us to identify the causal effects of common law precedent. Our specification should be invariant to the number of lags and leads. The use of leads serves as a check of the identification strategy, namely, whether the assignment of judges to obscenity cases may be endogenous to other factors that correlate with socio-economic outcomes.

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) collected data on all Courts of Appeals free speech decisions pertaining to obscenity from 1958–2004. 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),⁵ *Eckstein v. Melson* (selling magazines and books with explicit sexual imagery),⁶ and *Penthouse v. McAuliffe* (showing a *Penthouse* movie).⁷ More recently, cases in the 1990s and 2000s involve downloading images from the Internet⁸ and making lewd phone calls.⁹

To be sure, these cases may seem narrow, but legal precedent develops through elaborate analogies—at the conceptual level. For example, *Roe v. Wade* extended the right of privacy under the 14th Amendment’s Due Process Clause, which was previously interpreted as precluding government interference in *freedom of contract*¹⁰; *Roe v. Wade* interpreted Due Process as precluding government interference in a *woman’s decision to have an abortion*. Thus the impact of cases include the conceptual innovation, and are not restricted to future instances of identical fact patterns.

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, 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

⁵259 F.2d 54 (3d Cir. 1958).

⁶18 F.3d 1181 (4th Cir. 1994).

⁷702 F.2d 925 (11th Cir. 1983).

⁸*United States v. Thomas*, 74 F.3d 701 (6th Cir. 1996).

⁹*United States v. Landham*, 251 F.3d 1072 (6th Cir. 2001).

¹⁰See, e.g., *Allgeyer v. Louisiana* (1897).

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

decided. The salience and timing of this spike is consistent with people paying attention to these precedents.

We also collect all District Court obscenity cases, yielding 2,960 cases from 1957–2008. We collected administrative data on these cases from the Administrative Office of the U.S. Courts (AOC) and PACER filings on District Court cases. Sixteen years of Public Access to Court Electronic Records are available on open source sites for 33 Districts. We used PACER data to obtain judge identities that are missing in the AOC data.

3.2.2 Judicial Biographies We compiled information on judges’ characteristics from the Appeals Court Attribute Data, District Court Attribute Data,¹¹ Federal Judicial Center, and our own data collection. The final dataset includes information on vital statistics. Variables include: geographic history, education, occupational history, governmental positions, military service, religion, race, gender, and political affiliations. Raw data on religion come from Goldman (1999).¹² Judges whose religions remained missing or unknown were coded as having no publicly known religious affiliation. We collected religion data as political and social issues divide along religious lines in the U.S., so it is reasonable to hypothesize that judges from different religions come to different conclusions in court cases. 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 16.8 judges available for assignment to panels. Some judges assigned to cases come from District Courts or specialized courts. In robustness checks, we omit these judges. In expectation, there are 0.46 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. 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

¹¹<http://www.cas.sc.edu/poli/juri/attributes.html>

¹²Sisk’s data are available at <http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm>.

availability.

3.2.3 Attitudes and Behaviors We use the General Social Survey (GSS) to measure sexual attitudes and behaviors.¹³ The GSS is an individual-level survey that was conducted annually from 1973–1994 (except for 1979, 1981, and 1992), and biannually from 1994–2004. For each year, the GSS randomly selects a cross-sectional sample of residents of the United States who are at least 18 years old. The survey provides information on the demographic characteristics of the respondents and their attitudes towards various situations and societal phenomena. The GSS provides responses from approximately 1,500 respondents for each survey year between 1973–1992, and approximately 2,900 respondents per survey year from 1994–2004, for a total of 44,897 sample individuals between 1973–2004. This is the same dataset that Fernandez-Villaverde et al. (2014) use to attribute 50% of the sexual revolution to individuals’ moral views. Our data ends in 2004 because state-identifiers are not available in the public access version of the data.

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. $\Delta(v) = E(v_a | v_a > v) - E(v_a | v_a < v)$. 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.¹⁴ 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)

¹³<http://publicdata.norc.umd.edu/gssbeta/index.html>

¹⁴Some of the data were available here: <http://bpp.wharton.upenn.edu/jwolfers/data/DeathPenalty/StatePanel.dta>. We extend this series using earlier and later volumes of U.S. government statistical yearbooks.

of sexually transmitted diseases—chlamydia, syphilis, and gonorrhea—for each state from 1984–2008 from the Centers for Disease Control and Prevention¹⁵ 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.¹⁶ Unweighted regressions are reported in robustness checks, but the literature typically uses weights.

3.3 Empirical Strategy It has been argued that the ideal research design randomly assigns individuals or firms to different legal rules to help resolve uncertainty about consequential impacts of law (Abramowicz et al. 2011). Thankfully, individuals are not randomly assigned to different systems of justice, nor are judges randomizing their decisions in the interest of legal science, but the random assignment of law-making judges provides a close approximation.

A randomized control trial is effectively created through the random assignment of judges who interpret the facts and the law differently. Consider the following thought experiment to illustrate intuitions. 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 also high. Random variation in the assignment of Courts of Appeals 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).

The effect of free speech jurisprudence is then obtained by comparing the means of the outcomes of interest after progressive free speech precedent and conservative free speech precedent. Note that this difference is not an estimate of the comparison between progressive free speech precedent and no precedent. The values and behavior can be different than what they would have been if there was no precedent whatsoever. What we are trying to estimate is the effect of progressive free speech precedent, rather than conservative free speech precedent, *when there is a precedent*.

Denoting $\Delta(v)_{ict}$ as the moral views of individual i and Law_{ct} as a dummy equal to 1 if the

¹⁵U.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.

¹⁶<http://www.census.gov/popest/states/>

decision was progressive, this estimate is simply:

$$(1) \quad E[\Delta(v)_{ict}|Law_{ct} = 1] - E[\Delta(v)_{ict}|Law_{ct} = 0] \equiv \beta_1.$$

We expect $\beta_1 \neq 0$ if laws have effects other than through deterrence alone. Since we are interested in effects over time, we specify a distributed lag. We extend our specification to include the presence of a decision, $1[M_{ct-n} > 0]$. We focus on four years of lags and one lead ($n = -1$ to 4) and vary the lag structure for robustness. M is the number of cases, which is typically 0 or 1 (so typically $Law_{c(t-n)}$ is 1 (100% progressive) or 0 (100% conservative)). Since most of our data is yearly, we take the average law measurements in each Circuit-year, which also preserves the structure of our randomization (the moment condition for causal inference is the easiest to interpret). We also considered weighting our estimates by the number of cases in a Circuit-year, where weights are the geometric mean of $M_{c(t-n)} + 1$ over the distributed lag. The statistical significance of the results increases, so we present more conservative estimates without weighting.

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

Analogizing to coin flips, β_1 captures the effect of the heads-or-tails coin flip (progressive vs. conservative precedent), $\beta_1 + \beta_2$ captures the effect of the heads coin flip and the presence of the coin (progressive precedent vs. no decision), and β_2 captures the effect of the tails coin flip and the presence of the coin (conservative precedent vs. no decision). 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 examine several outcomes: self-reported values, self-reported behavior, and audits of behavior. We expect progressive laws to increase progressive values and behavior if free speech law has expressive effects.

In principle, we have up to 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 the following features.

However, as we see it, the objective of the study is not to identify a best model of the law and norms process, but to communicate the information embodied in a data set on law and norms. If the objective of the exercise is to communicate a single estimate of the effect of laws (and an associated measure of the uncertainty of the estimate), then it has been argued this should be done with explicit recognition of the model uncertainty present in the analysis (Cohen-Cole, Durlauf, Fagan and Nagin 2009). The conceptual arguments (Leamer 1978; Draper 1995) and statistical issues are detailed elsewhere (Sala-i Martin, Doppelhofer and Miller 2004; Hansen 2007; Hjort and Claeskens 2003). This leads us to present many estimates of the model and leave the reader to take a weighted average corresponding to their preferences, though to be sure, the reader can also limit attention to the baseline model. Controls we consider are:

- Circuit-fixed effects, time-fixed effects, and Circuit-specific time trends to allow different Circuits to be on different trajectories with respect to outcomes. These controls should probably not be dropped for the standard reasons alluded to in the prior thought experiment, and indeed the results are most malleable to their exclusion. The malleability suggests a reference as to how stable we might expect our inferences to be, and whether we should evaluate the sign and significance—rather than point estimates—of the effect across multiple specifications due to model uncertainty, or interpret the range of point estimates as form of partial or set identification;
- 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 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;
- Time-varying Circuit-level controls, such as the characteristics of the pool of judges available to be assigned in Circuit c and time $t - n$, and lagged community standards that we include 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.

We also present a specification with four years of leads and one lag ($n = -4$ to 1).

For standard errors, as Barrios, Diamond, Imbens, and Kolesar (2012) write, “if the covariate of interest is randomly assigned at the cluster level, only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals,” so we expect to see similar results whether clustering standard errors at the Circuit or Circuit-year level. Barrios et al. (2012) show that random assignment of treatment addresses serial and spatial correlation across treatment units. We check our results using the standard approach with U.S. data, 50 state clusters; randomization inference that assigns the legal variation to another Circuit; and wild bootstrap.¹⁷

3.3.1 Instrumental Variable Law_{ct} and ϵ_{ict} may be correlated due to uncontrolled-for social trends or other legal developments that correlate both with Law_{ct} and $\Delta(v)_{ict}$. 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 (and, as a normative matter, should) 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

¹⁷We thank our NBER discussant Bentley MacLeod for recommending for a related paper that we cluster at the Circuit level as the baseline.

drop the subscript n to ease the exposition on the instrumental variables construction.

We develop an instrumental variable for Law_{ct} using judges' biographical characteristics. We use biographical characteristics because the number of free speech cases yields sharp demographic effects but not judge-specific effects (there are roughly 180 life-time appointed judges and only 175 cases). Let N_{ct} be the number of judges assigned to free speech panels who are Democratic. 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). Figure 5 illustrates the identification strategy. The jagged line displays $\frac{N_{ct}}{M_{ct}}$ and the smooth line displays $\mathbf{E}(\frac{N_{ct}}{M_{ct}})$ in each of the 12 Circuits. The smooth lines indicate the underlying variation in judge-specific characteristics within Circuits over time. The jagged line indicates the random year-to-year variation in Democrats per seat. We estimate how outcomes respond to idiosyncratic variation in $\frac{N_{ct}}{M_{ct}}$.

More formally, let $p_{ct} = \frac{N_{ct}}{M_{ct}} * \mathbf{1}[M_{ct-n} > 0]$, i.e., defined to be 0 when $\mathbf{1}[M_{ct-n} > 0] = 0$. Then: $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{Pr}[M_{ct} > 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} > 0] + \mathbf{Pr}[M_{ct} = 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} = 0] = 0$. Next, $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}[\mathbf{E}(p_{ct})\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}(p_{ct})\mathbf{E}(\varepsilon_{ict}) = \mathbf{E}[p_{ct}\varepsilon_{ict}]$. Thus, p_{ct} and $p_{ct} - \mathbf{E}(p_{ct})$ both serve as valid instruments. Notably, as Table 2 and Figure 7B show, $\mathbf{E}(p_{ct})$ is uncorrelated with Law_{ct} . The precise collection of instruments is $p_{c(t-n)}$ for $n = -1$ up to 4.

This draft presents estimates using the following identification assumption (moment condition) for causal interpretation: $\mathbf{E}[\frac{N_{ct}}{M_{ct}}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0]] = 0$. Early drafts obtained similar results using $\mathbf{E}[N_{ct}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0], M_{ct}] = 0$, which looks at the number of progressive decisions controlling for the number of decisions, and $\mathbf{E}[N_{ct}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0], Q_{ct}] = 0$, which controls for the size of the court docket and checks if progressive vs. conservative decisions had opposite-signed effects.

Returning to our thought experiment, if a Circuit-year has a higher fraction of Republicans assigned, the precedent that year will be that much more conservative. We are interested in the subsequent effects of that precedent on values and behavior. We are able to do so because the identity of a judge on a case does not directly affect outcomes except through the precedent. The court decision is taken as precedent by subsequent courts. Also, judge identity do not predict stock prices at the time of resolution controlling for the manner in which the case was resolved and judge identity do not predict stock prices at the moment that judges are revealed (Badawi and Chen 2014).

It is also worth noting that for our legal domain, allowing vs. disallowing free speech exercise is the materially relevant legal doctrine. A very interesting feature of the institutional setting, however, is that it is possible to assess this hypothesis in conjunction with another. If there are other aspects of free speech doctrine that are sensitive to judges' biographical characteristics, and if these other aspects of free speech doctrine affect societal outcomes, we should observe correlations between 2SLS residuals and Circuit-year biographical characteristics not used in the first stage. They are not, which suggests that the allowing vs. disallowing free speech dimension of these cases is the primary channel through which free speech jurisprudence has an effect or that other aspects of free speech jurisprudence are not polarized along judicial demographic characteristics.

An early draft of Chen and Sethi (2011) compared the effects of sexual harassment law with gender discrimination law (since these areas of law are closely related) in a specification that included both areas of law and instruments for each. The data on these laws was collected by Sunstein et al. (2006). The addition of another area of law did not affect the coefficient on the other area of law, which assuage concerns of leakage.

3.3.2 Counterfactuals Dummying for the presence of a case also permits the identification of additional counterfactuals. β_{1n} captures the effect of progressive precedent where the counterfactual is a conservative precedent, $\beta_{1n} + \beta_{2n}$ captures the effect of progressive precedent where the counterfactual is no precedent, and β_{2n} 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 (again, suppressing the subscript n to ease the exposition on the instrumental variables construction). The discussion that follows is paraphrased from Chen and Yeh (2014b) and presented for the reviewer for clarity, though it can be omitted.

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. We end up using the first definition due to data availability, but we present the second definition in case of future data availability.

In the first definition, $w_{ct} = \frac{K_{1t} * \left(\frac{L_{1t}}{K_{1t}}\right) + \dots + K_{6t} * \left(\frac{L_{6t}}{K_{6t}}\right)}{K_{1t} + \dots + K_{6t}}$, 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, though in reality it goes from 5 to 13 depending on the District. The intuition is that assigning District judges who are disproportionately appealed, for whatever reason, leads to the presence of a case in the Circuit, $\mathbf{1}[M_{ct} > 0]$. Note that the instrument is simply the weighted average across all Districts within the Circuit and the construction of this instrument rests on the assumption of at least one District case per Circuit-year (else, it also makes necessary a dummying-out strategy for missing values).

In the second definition, $\tilde{w}_{ct} = K_{1t} * \left(\frac{L_{1t}}{K_{1t}} - E\left(\frac{L_{1t}}{K_{1t}}\right)\right) + \dots + K_{6t} * \left(\frac{L_{6t}}{K_{6t}} - E\left(\frac{L_{6t}}{K_{6t}}\right)\right)$ where *i* denotes District *courthouse* or District *Court*. The second definition addresses the issue that the location of

free speech controversies, K_{it} , may be endogenous. An alternative definition would have generated a collection of District courthouse level instruments, but 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: $E\left[\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct} \middle| K_i\right] = E\left[E\left[\left(\frac{L_i}{K_i} - E\left(\frac{L_i}{K_i}\right)\right) * \epsilon_{ct} \middle| \epsilon_{ct}, K_i\right] \middle| K_i\right]$. 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 $K_1, \dots, K_6$. 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.

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 and is fully developed in Chen et al. (2014b). This identification assumption allows omitting $\mathbf{1}[M_{ct} > 0]$ and the need to instrument for it altogether, which can be useful in rapid impact analyses. To be sure, this also renders only one estimable counterfactual, rather than three.

3.3.3 Randomization

Courts of Appeals A few scholars argue that certain Circuits have not used random assignment (Hall 2010; Chilton and Levy 2015). However, these variations tend to be due to judges taking sick leave or being on vacation. These decisions are determined far in advance. Other variations from random assignment include: remanded cases from the Supreme Court are returned to the original panel; en banc cases that are heard by the entire pool of judges (or a significant fraction in the Ninth Circuit); judges with conflict of interests opt out after random assignment, which is extremely rare. We do not use remanded or en banc cases, which are also relatively infrequent. 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—like the identification strategy of papers that use the patent officer assignment or disability application reviewer assignment, which are not explicitly random (Maestas et al. 2013; Galasso et al. 2015)—assumes that idiosyncratic deviations from random assignment are ignorable. Even a gold-standard random process — the roll of a die — has a deterministic element.

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

Interviews Chen and Sethi (2011) 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 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.

Orthogonality with pre-trial characteristics 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 Courts of Appeals judges in 415 gender discrimination cases.

Omnibus test for Courts of Appeals 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. 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 the Courts of Appeals, judges are revealed very late, after litigants file their briefs, sometimes only a few days before the hearing, if there is a hearing, which gives little opportunity and incentive for settlement upon learning the identity of the panel. Most of the litigation costs are sunk by that point. 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., $s_1, s_2, s_3 \dots s_n$. 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 $s_1, s_2, s_3 \dots s_n$.
5. Repeating steps 1-4 and calculate p_i for each unit.

We use the following statistics:

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

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

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

cases during certain time periods (e.g., to achieve specialization).

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

With a truly random process, the collection of all unit p-values should be uniformly distributed. The 1001th random string should have a summary statistic that is equally likely to be anywhere from 1 to 1000. A visual examination suggests that the empirical distributions for our p-values approach the CDF of a uniform distribution. 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, so this would also have to be available in the future.

District Courts District Courts assign one judge to a case randomly or rotationally (Taha 2009; Bird 1975). Cases being returned on remand from the Courts of Appeals are not randomly assigned. We do not use remanded cases in our dataset. For example, one District told us that random assignment occurs within 24 hours of a case filing, which is handled in the order of its arrival. Waldfogel (1995) reports that one District Court uses three separate randomization wheels 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.¹⁸ 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

¹⁸<http://www.mnd.uscourts.gov/cmecf/Order-for-Assignment-of-Cases.pdf>

in that Division. We confirm the method of random assignment by contacting all of the District Courts.

Wheels 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 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.

Consolidation 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 test for 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.

3.3.4 Interpretation In common law, hard cases precede easy cases. Cases that reach the Courts of Appeals are the more challenging and legally innovative cases. According to one Courts of Appeals 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). 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 Courts of Appeals decisions.

The 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. 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, $\sum_{n=0}^{\infty} \beta_{1n} = \sum_{n=0}^{\infty} TOT_{ct}^n = \sum_{n=0}^{\infty} LATE_{ct}^n$.

Indirect inference Table IV provides indirect inference that is consistent with the effect of hard free speech cases being largely through cases subsequently *not* litigated and published in the Courts of Appeals. 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 Courts of Appeals 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).

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. $LATE + \text{effect on always-takers} = TOT$ (Treatment on Treated) of the Circuit = $(TOT_{\text{direct}} + TOT_{\text{indirect of individuals}}) * P(\text{individual exposure in treated circuit})$. The experiment estimates TOT_{direct} for individuals. The unknown parameters in the equation are TOT_{indirect} and $P(\text{individual exposure in treated circuit})$. 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.¹⁹ 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.

¹⁹Matt Bokor, "Jacksonville Porn-Free, Officials Say," Associated Press, Domestic News, Dec. 16, 1980.

Court decisions influence preferences among people in the community where the case originates (Hoekstra 2003).

Expressive externalities To buttress the plausibility that Courts of Appeals decisions could eventually reach community leaders in a locality, we use a sample of newspapers and their mentions of Courts of Appeals decisions. We collated articles from the major newspaper for the city in which each Circuit Court resides. These are: *The Boston Globe*, *New York Times*, *Philadelphia Inquirer*, *Richmond Times Dispatch*, *Times-Picayune*, *Cincinnati Post*, *Chicago Tribune*, *St. Louis Post-Dispatch*, *San Francisco Chronicle*, *Denver Post*, *Atlanta Journal and Constitution*, and *The Washington Post*. We collected data from 1979 to 2008 from NewsBank using the search term: (obscen*) w/100 (judgment OR "court ruling") AND Circuit AND NOT "Supreme Court".

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

Information transmission Information need not transmit directly from the Courts of Appeals 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*. To be sure, 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*. Further evidence of the information transmission channel of Courts of Appeals decisions include the market response to their decisions (Araiza et al. 2014).

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 2014b). 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 new statute. If upheld, this statute would reduce the number of centers operating in the state to fewer than 10.²⁰ These examples illustrate how the Courts of Appeals 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.

3.4 First Stage: The Effect of Judge Identity on Court Decisions 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).

The Republican party has traditionally been associated with conservative values, which favor restricting exercise of free speech and focus on its perceived harms. Democrats are 10 percentage points more likely to vote for a progressive verdict ($p < 0.1$). This estimate appears in Table III Panel A Column 1. The point estimate is essentially unaffected with the inclusion of Circuit and year fixed effects in Column 2 ($p < 0.01$), the inclusion of the proportion of Democrats in the Circuit pool of judges in Column 3 ($p < 0.1$), and the inclusion of both sets of controls in Column 4 ($p < 0.01$). The estimates are notably more significant with the inclusion of fixed effects but not with the proportion of Democrats in the Circuit pool. This is due to fixed regional and time factors, but not the composition of the judge pool, predicting the outcomes of cases, so including these factors as controls sharpen the estimates of the relationship. We can see that composition of the pool is uncorrelated with outcomes in Figure 7B.

In Panel B, we examine the verdict at the panel-level. An additional Democrat on one of the 175 three-judge panels increased the chances of a progressive verdict by 5 percentage points in Column 1 ($p > 0.1$), by 10 percentage points when including fixed regional and time factors in Column 2 ($p < 0.05$), by 6 percentage points when including the proportion of Democrats in the Circuit pool in Column 3 ($p > 0.1$), and by 9 percentage points when including both sets of controls in Column 4 ($p < 0.05$). Notably, the inclusion of characteristics of the judge pool does not affect the relationship (compare Column 1 to 3 and 2 to 4). The variation in the point estimates and significance when controlling for Circuit and year fixed effects indicates that time and regional factors are important

²⁰<http://www.nytimes.com/2014/07/30/us/mississippi-abortion-clinic-Federal-court-blocks-closing.html>

determinants of panel-level effects.

In Panel C, we examine these relationships at the Circuit-year level for the 124 Circuit-years with at least 1 case. The estimates indicate that an additional Democrat on a three-judge panel increased the proportion of progressive decisions by 11 percentage points in Column 1 ($p < 0.05$). The Circuit-year level estimates differ from the case level since cases are not evenly distributed across Circuit-years. For example, suppose that there are 4 cases, one case each with 0, 0.33, 0.66, or 1 proportion of judges who are Democrat, and suppose that the panel makes a progressive decision when there are 3 Democrats. If 1 Circuit-year has the case with 0 Democrats 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 = $\frac{0.33-0}{\frac{6}{9}-\frac{0}{9}} = 0.5$) 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 ($\frac{0.33-0}{\frac{6}{9}-\frac{3}{9}} = 1.5$).

Column 2 adds a dummy indicator for whether there were cases, $\mathbf{1}[M_{ct} > 0]$ and dummies out for missing values. Dummying out for missing values is a standard approach in the literature. We redefine $Law_{ct} = Law_{ct} * \mathbf{1}[M_{ct-n} > 0]$, i.e., defined to be 0 when $\mathbf{1}[M_{ct-n} > 0] = 0$, and regress it onto the instrument $p_{ct} = \frac{N_{ct}}{M_{ct}} * \mathbf{1}[M_{ct-n} > 0]$, while including $\mathbf{1}[M_{ct-n} > 0]$ as a control. The number of observations increases to the complete time-frame of 612 Circuit-years. Circuit 11 was not founded until 1981, so Circuit 11 has 6 fewer observations than the other Circuits. Circuit 11 was created by splitting it off from Circuit 5; Circuit 5's decisions before this split are considered binding precedent in Circuit 11. We account for this split in our analyses by assigning pre-1981 precedent in Circuit 5 to observations in Circuit 11.

The point estimates and F-statistics are essentially unaffected by dummying out for missing values, which indicates that the increase in sample size from dummying out for missing values is not driving results. The point estimate remains at 12 percentage points with the inclusion of Circuit and year fixed effects in Column 3 ($p < 0.01$), the inclusion of fixed effects and the proportion of Democrats in the Circuit pool of judges in Column 4 ($p < 0.01$), the inclusion of fixed effects and Circuit-specific time trends in Column 5 ($p < 0.01$), and the inclusion of all these controls in Column 6 ($p < 0.01$). The F-statistic is 10.4, which is above the weak instruments threshold.

When we weight our estimates by the number of cases in a Circuit-year, where weights are the geometric mean of $M_{c(t-n)} + 1$ over the distributed lag, the statistical significance of the results increases. We also check that our results have strong Anderson-Rubin weak instruments-robust test statistics. We did not consider the solution employed in Galasso et al. (2015), which uses the predicted estimate from the first stage as the final instrument. Doing so would greatly increase the F-statistics, and Angrist and Pischke (2008) suggests to avoid doing so in order to not obtain identification from functional form assumptions. Thus, the first stage results should be interpreted as conservative estimates in terms of statistical significance relative to these alternative methods.

Panels D reports the first stage analyzed at the GSS-level. Bertrand et al. (2004) recommend an analysis at the individual level to be able to control for individual-level covariates. The coefficients differ from those in Panel C because of number of individuals per Circuit is not constant. The point estimate remains at 18 percentage points without controls in Column 1 ($p < 0.05$), with the inclusion of $\mathbf{1}[M_{ct} > 0]$ in Column 2 ($p < 0.05$), and adding Circuit and year fixed effects in Column

TABLE III

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

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

Notes: Significant at +10%; *5%; **1%. Heteroskedasticity-robust standard errors are in parentheses and clustered at the Circuit level. Controls include fixed effects (dummy indicators for Circuit and year), expectations (expected proportions of Democratic appointees on a given panel), and trends (Circuit-specific). Proportions during Circuit-years with no cases are defined to be 0. Panel D: GSS (1973-2004) weights are sampling weights. Individual-level controls are age, gender, race, and college education. Panel E weights are population of state or reporting agency. State-level controls are percent urban, infant mortality, percent age 15-19, percent age 20-24, percent nonwhite, police employment, unemployment rate, and real per capita income.

3 ($p < 0.01$). The point estimate becomes 20 percentage points with the addition of Circuit-specific time trends and proportion of Democrats in the Circuit pool of judges in Column 4 ($p < 0.01$), individual-level covariates in Column 5 ($p < 0.01$), and weighting using GSS sampling weights in Column 6 ($p < 0.01$). The F-statistic is 12.9, which is above the weak instruments threshold.

Panel E investigates the first stage at the CDC-level in Columns 1-4, where the individual observation is a state-year, and UCR-level in Columns 5-6, where the individual observation is a reporting-agency-year. All columns include a dummy indicator for whether there were cases, $1[M_{ct} > 0]$ and dummies out for missing values. The first stage for CDC data is 11 percentage points in Column 1 ($p < 0.05$) and with the inclusion of Circuit and year fixed effects in Column 2 ($p < 0.05$). It becomes 12 percentage points with the inclusion of Circuit-specific time trends and proportion of Democrats in the Circuit pool of judges in Column 3 ($p < 0.05$) and 13 percentage points including state-population weights in Column 4 ($p < 0.01$). The first stage for UCR data is 11 percentage points in Column 5 ($p < 0.05$) and 20 percentage points with the inclusion of crime controls and population weights of the reporting agency in Column 6 ($p < 0.01$). The number of observations drop slightly due to loss of controls. The F-statistics in Columns 4 and 6 are 12.8 and 12.3, which are above the weak instruments threshold.

To check whether our linear specifications miss important aspects of the data, Figure 7A presents nonparametric local polynomial estimates of the first stage. Estimation proceeds in two steps. In the first step, we regress the proportion of decisions that were progressive on Circuit and year fixed effects and we regress the instrument on the same. Next, we take the residuals from these two regressions and use a nonparametric local polynomial estimator to characterize the relationship between the instrument and progressive decisions. The first stage effect is not due to outliers.

The figure also shows the tremendous variation across Circuits and years, which will be useful in estimating the impact of Law_{ct} . Figure 7B shows that there is no relationship between the proportion of Democrat judges in the Circuit-year and the proportion of progressive decisions, which is consistent with the stability of the first stage coefficient when controlling for proportion of Democrat judges in Table III. This finding also complements Table IV, 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. These specifications are analogous to the ones in Table III Panel C Column 6 with a small loss in data due to lags and leads of judicial assignments being outside the range of the legal data. This does not mean that Circuit Court cases have no precedent, for the reasons described above.

We also employed LASSO to instrument for Law_{ct} . It is worth noting that a large number of biographical characteristics serve as valid instruments, which results in a weak instruments problem if we used them all. Roughly thirty characteristics that enter in levels (Democrat, male, 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

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

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

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

previous U.S. attorney), judge-level interactions (e.g., minority Democrats), and panel-level interactions (e.g., fraction of judge seats assigned to Democrats multiplied by fraction of judge seats assigned to racial minorities) yielding a total of several thousand possible instruments.

There are two ways to reduce dimensionality: a priori theory and model selection. LASSO (least absolute shrinkage and selection operator) is commonly used for model selection (Belloni et al. 2012). 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. The F statistics increase to 37 for GSS values and to 104 for GSS behaviors. In our results, we report estimates using just the Democrat instrument or the instruments selected by LASSO. All estimates use the limited information maximum likelihood (LIML) estimator because of its better small sample properties. We also present estimates with instruments for $\mathbf{1}[M_{ct} > 0]$.

We also report the results of 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. Chen et al. (2014b) describes the method in more detail and analogized it to partial identification or set identification: the 2SLS results are visually presented for a variety of instruments that have a strong first stage.

The visual Hausman test can also be viewed as another form of addressing model uncertainty instead of model averaging. Another view of the visual Hausman test and presentation of model

uncertainty in the many results that follow (especially in light of model uncertainty in the calculation of standard errors), is to interpret the point estimates as representing the confidence interval. Some suggest that there are no good rules of thumb for weak instruments when standard errors are clustered, and others suggest that the use of clustering in models with many fixed effects can be problematic. Coefficients can also vary due to sampling variation. All estimates are subject to the usual caveats that causal effects are sufficient, but not necessary conditions for an outcome (Deaton 2010). To illustrate, Chen et al. (2016b) use a machine learning approach to analyze the effects of court laws and finds that, of 18 factors that predict abortion attitudes, the court variables comprise 25-30% of the sixth factor.

At present, the literature on high-dimensional instrumental variables is evolving. Chen et al. (2016a) constructs and uses the 2 billion N-grams of up to length 8 from the universe of roughly 380,000 Courts of Appeals cases dating back to 1880 and to characterize memes that are predictive of judicial decisions. Ash et al. (2016) show that judges who use law and economics phrases (in cases other than the current case) are more likely to vote for and render conservative jurisprudence. Also, for most of the paper we are only interested in average effects. Athey and Imbens (2015) propose a machine-learning method for estimating heterogenous causal effects. We explore one dimension of heterogeneity in this paper, and additional exploration is provided in Chen and Yeh (2014a). Future econometric developments may aid rapid impact analysis of U.S. federal court decisions at the moment when judges are seeking guidance.

We report one exploration in this direction. 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: $Y_{ict} = \beta_{10}Law_{c(t)} + \beta_{11}Law_{c(t-1)} + \dots + \varepsilon_{ict}$. 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 $Z_0 = \begin{bmatrix} p_{c(t)} \end{bmatrix}$ and $Z_1 = \begin{bmatrix} p_{c(t-1)} \end{bmatrix}$. Set $\hat{X} = [\hat{L}_{c(t)} \quad \hat{L}_{c(t-1)} \quad \dots \quad \hat{L}_{c(t-j)}]$ for $j = 0, 1, \dots$, 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 $\sqrt{n}(\hat{\beta} - \beta) = \hat{Q}^{-1}\frac{\hat{X}'\varepsilon}{\sqrt{n}} \cdot \frac{1}{\sqrt{n}}\hat{X}'\varepsilon = \frac{1}{\sqrt{n}}\frac{X_jz_j}{n}(\frac{z_j'z_j}{n})^{-1}z_j'\varepsilon = \hat{\Gamma}\sqrt{n}\frac{z_j'\varepsilon}{n}$. Since $\sqrt{n}\frac{z_j'\varepsilon}{n} \rightarrow N(0, \Phi_j)$, so $\sqrt{n}(\hat{\beta} - \beta) \rightarrow N(0, V)$, $V = Q^{-1}\Gamma\Phi\Gamma Q^{-1}$. The use of fitted values turns out to share similarities with the solution employed in Galasso et al. (2015).

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 the 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. The 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 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

TABLE V
THE EFFECTS OF FREE SPEECH PRECEDENTS ON SEXUAL ATTITUDES

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

Notes: 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.

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 the Appendix (Visual Hausman) 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. We interpret this figure as suggesting the 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—stated revealed preference. 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)

TABLE VI
THE EFFECTS OF FREE SPEECH PRECEDENTS ON SEXUAL BEHAVIORS

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

Notes: 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.

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.

These results are robust when defining the instrument for variation in free speech precedent using the number of judicial panels with at least one Democratic appointee and with at least a Democratic majority, while controlling for the number of free speech cases. 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. If progressive decisions correspond with higher pornography consumption, then the statistically insignificant effect on forcible rapes is consistent with Wongsurawat’s (2006) conclusion that the previously documented positive correlation between pornography consumption and forcible rapes is overstated. 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 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. To be sure, this is related to the idea that causal effects are sufficient but not necessary for an outcome (Deaton 2010).

In any event, arrest data may 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 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 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

TABLE VII
THE EFFECTS OF FREE SPEECH PRECEDENTS ON SEXUAL CRIMES

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

Notes: 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.

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. Since this is a growth specification, the decay in individual lags reported in Appendix Table XV suggests a level effect rather than a growth effect.

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, and they are available in previous drafts of the paper.

4.5 Counterfactuals Even though the differences in free speech activity seem to be aligned with the differences in judges' revealed preferences, the results we have discussed so far focus on the difference in outcomes after progressive as opposed to conservative precedent. 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 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 calcula-

TABLE VIII
THE EFFECTS OF FREE SPEECH PRECEDENTS ON SEXUAL DISEASES

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

Notes: 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.

tions are presented in lower panels of Appendix Tables II, V, VI, VII, X, XI, XII, XV, and XVIII and summarized in Table IX.

Note that the scaling 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). If the previous results seem large, the reader should focus on the scaled coefficients instead. The simulated counterfactual presented in the Appendix also emphasizes that only a small portion of variance is explained by law.

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”.

The term “supercontrol” comes from Crépon et al. (2013), which presents the results of a national experiment that randomizes both the presence of an employment training program across cities as well as the training of individuals when there was a program. This is similar to our setting. 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

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

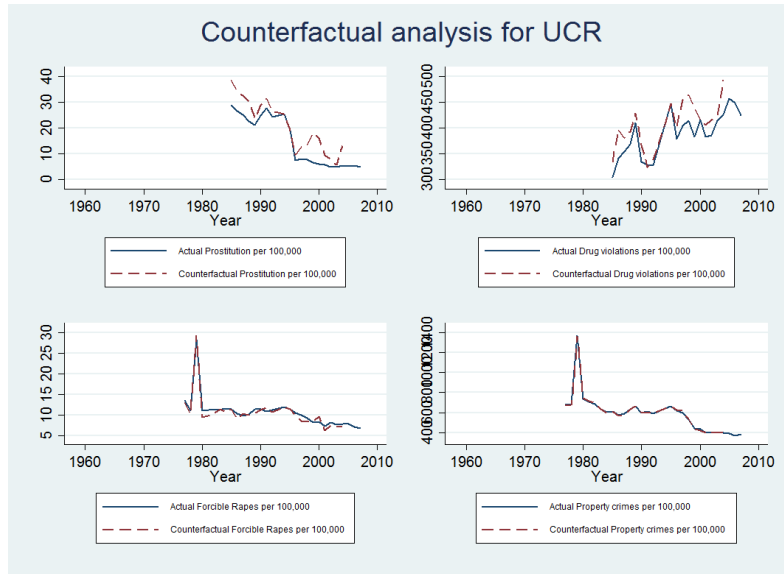
Notes: This table summarizes β_1 , $\beta_1 + \beta_2$, and β_2 for each outcome, scaled by the number of cases per year to report the typical effect per year of free speech precedent.

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, and further support an interpretation of a small impact of law. 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 visually assess the magnitudes of our estimates. 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.

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 between the effect sizes reported in Bhuller et al. (2013) and Fernandez-Villaverde et al. (2014).



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. 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 occurred 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 $a = 1$.

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.

Perhaps with AIDS in the 1980s, conservative obscenity decisions came to indicate that negative externalities from obscenity exposure were greater than before, due to the secondary effects of

disease, which would lead to expressive effects. Our population analysis and the experiment below query people’s beliefs about whether sexual materials lead to the breakdown of morals or lead to rape (not disease) and found that progressive decisions increased such beliefs. If laws have this information effect, it would still be evidence of a channel for expressive powers of law separate from its deterrence effects.

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 on the entire time frame.

To be sure, we emphasize the limited ability to conduct heterogeneity analyses especially in a manner suggested by Athey and Imbens (2015), which we leave for future research. We investigated all outcomes discussed in this paper using the same battery of robustness checks, which were reported in a longer previous draft; in the current draft, 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. (2014b) documents that abortion jurisprudence led to immediate backlash in campaign donations and abortion preferences. This paper differs from Chen et al. (2014b) 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. (2014b). In that paper, the model allows for temporal variation in backlash and expressive effects, but presents the policy-maker with a trade-off to implement social change that is large or gradual. 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, the evidence complements prior qualitative analysis of backlash effects and further suggests that laws can have effects separate from the deterrence effects of sanctions (since backlash and deterrence have opposite effects on observed behavior).

Some of other effects in the field data are also inconsistent with deterrence as the sole channel to explain the findings. 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*

TABLE X
THE EFFECTS OF FREE SPEECH PRECEDENTS OVER TIME

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

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

and *Penthouse*, from the Audit Bureau of Circulations. *Playboy* and *Penthouse* were competitors at the boundaries of community standards through the 1970s. In the 1990s, their marketing strategies had diverged. Playboy became more conservative in its depictions while Penthouse purposely pushed towards near obscene depictions. Their circulation data was 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 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. Just knowing that an observer will think badly of the decision-maker can be sufficient to affect responses (Dana et al. 2006, 2007; Cilliers et al. 2015).

We check whether this mechanism explains the patterns in our data by 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 these issues.

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.²¹

²¹<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).

Because subjects are unaware of an on-going experiment, differential attrition may arise at the time treatment is revealed (Reips 2001). We minimize attrition through a commitment mechanism. In all treatment conditions, workers face an identical “lock-in” task in order to minimize differential attrition before the treatment is revealed.

The payment for each paragraph is 10 cents with workers able to receive much more in bonuses, including a 50-cent bonus for completing the survey from the GSS at the end. A paragraph takes about 100 seconds to enter so the offered payment of 10 cents per paragraph is equivalent to \$86.40 per day. At the time of the experiment, the federal minimum wage in the United States was \$58/day. In India, payment rate depends on the type of work done, although the “floor” for data entry positions appears to be about \$6.38/day.²² 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 as follows:

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

²²Payscale, Salary Snapshot for Data Entry Operator Jobs, http://www.payscale.com/research/IN/Job=Data_Entry-Operator/Salary?, accessed June 17, 2011.

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

5.2 Results

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

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

$Treatment_{it}$ is defined as 1 (for progressive), 0 (for control), or -1 (for conservative) for individual i in treatment t . X_{it} are demographic controls. We control for whether the data worker is male and, in the experiment with 197 workers from around the world (mostly from India and the U.S.), a dummy indicator for being from India. The second experiment restricted to the U.S. and had 548 workers.

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. However, other attitudes presented did not display a significant effect. At the baseline, 48% of workers said that homosexual sex is acceptable. Workers exposed to progressive decisions were 6 percentage points 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 we exclude Treatment 4, which 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

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 (0.0363)	-0.0403 (0.0280)	-0.0292 (0.0304)	0.0637+ (0.0373)	-0.0537 (0.0392)
India	-0.386** (0.0680)	0.0528 (0.0524)	-0.307** (0.0569)	-0.363** (0.0697)	-0.181* (0.0734)
Male	0.246** (0.0693)	0.0698 (0.0534)	0.135* (0.0580)	0.138+ (0.0711)	0.0631 (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 (0.0387)	-0.00403 (0.0286)	0.0187 (0.0235)	0.0419 (0.0380)	0.0335 (0.0388)
India	0.124+ (0.0724)	0.00969 (0.0535)	-0.00506 (0.0440)	-0.110 (0.0712)	-0.213** (0.0726)
Male	0.0478 (0.0738)	0.146** (0.0546)	0.149** (0.0449)	0.328** (0.0725)	-0.0173 (0.0740)
Mean Dep. Var.	0.399	0.158	0.099	0.517	0.438
Observations	197	197	197	197	197
R-squared	0.021	0.040	0.057	0.098	0.050

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

TABLE 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 (0.0190)	0.0145 (0.0156)	-0.0192 (0.0231)	0.0351+ (0.0209)	0.0425+ (0.0227)	-2.511* (0.979)
Male	0.0576 (0.0360)	0.0839** (0.0297)	0.150** (0.0439)	0.0213 (0.0398)	-0.000567 (0.0430)	-6.741** (1.861)
Mean Dep. Var.	0.803	0.124	0.392	0.739	0.655	44.532
Observations	548	548	548	548	548	548
R-squared	0.005	0.016	0.022	0.006	0.006	0.035

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

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 percentage points (out of a baseline of 74%) and the likelihood that people favor sex education in public schools by 4 percentage points (out of a baseline of 66%). The fact that the basic patterns replicate across three experiments, including Chen and Yeh (2014a), is consistent with the expressive power of law. 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 a third experiment. Chen and Yeh (2014a) uses 600 U.S. workers. In their 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. One group was asked to report their

own behaviors and another group to estimate the prevalence of the other workers' behaviors with incentive pay for accuracy. This design differs from the two experiments reported in this paper in that Chen and Yeh (2014a) (i) used monetary incentives to measure belief-updating of others' moral views (community standards), (ii) separated individual from community standards, and (iii) measured subjective utility. Exposure to progressive free speech jurisprudence caused more progressive values and increased the perceived prevalence of progressive values.

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 are consistent with a mechanism where legitimacy of law affects utility and self-identification.

5.3 Modeling Implications 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 particular 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 percentage 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 percentage 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 $LATE + \text{effect on always-takers} = TOT$ (Treatment on Treated) of the Circuit = $(TOT_{\text{direct}} + TOT_{\text{indirect}} \text{ of individuals}) * P(\text{individual exposure in treated circuit})$. The experiment estimates TOT_{direct} for individuals. Filling in parts of this equation yields: 0.3 percentage points + effect on always takers = $(3.5\% + TOT_{\text{indirect}}) * P(\text{individual exposure in treated circuit})$. Assuming that 3.5 percentage points * $P(\text{individual exposure in treated circuit})$

circuit) is the direct deterrence or expressive effects from hearing about the case, then $TOT_{\text{indirect}} * P(\text{individual exposure in treated circuit})$ captures the expressive externalities on individuals who did not hear about the case.

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. If we allow different probabilities for the direct and indirect exposures, it is reasonable to believe that $P(\text{individual direct exposure in treated circuit})$ is quite small while $P(\text{individual indirect exposure in treated circuit})$ 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, and social scientists and philosophers have long debated whether law shapes values.

We present a framework to analyze the impact of law on norms. The theoretical framework allows for both backlash and expressive effects to occur. The empirical framework allows for rapid impact analyses of U.S. Courts of Appeals decisions when judges are considering the consequences of their decisions.

We apply these frameworks to fifty years of free speech jurisprudence. Random assignment of judges to Circuit panels effectively created random variation in common law precedent.

U.S. Federal Court judges ruling on First Amendment cases regarding free speech appear to have important effects on values and behavior. Democrats assigned as judges under the Federal Court system decide free speech cases in a manner more closely linked to prioritizing individual self-expression, and they vote to protect free speech. Republicans decide cases in a manner more closely linked to a focus on secondary effects, and they vote to constrain free speech.

The effects of their decisions seem to be largely attributable to a shift in values and behavior directly relevant to the preferences of the judges. Decisions that prioritize individual self-expression appeared to increase the value and exercise of free speech rights. Decisions that focus on secondary effects appeared to reduce crime and disease. Relative to conservative free speech precedent, progressive precedent were associated with more progressive attitudes and behaviors on non-marital sexual activity, some sex-related crimes, prostitution and drug violations, and higher rates of STDs.

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.

Throughout the paper we emphasize model uncertainty. The frameworks presented are intended to be improved on in future work to aid judges in their decision making. Certainly, extrapolating the results to other countries needs caution. Our paper also contributes to a literature on the long-run consequences of institutions such as common law (Acemoglu et al. 2001; Dell 2010; La Porta et al. 1998), ongoing debates on whether institutions are mainly products of economic or social determinism (Rosenberg 1993; Klarman 2004), and whether accidents, leaders, and decisions have significant impacts on society (Banerjee and Duflo 2014). These results suggest that the identity

of the policymaker will have important effects on policy and values, and highlight an important behavioral channel for the effects of law that has received less attention in the formal literature.

Figure 1

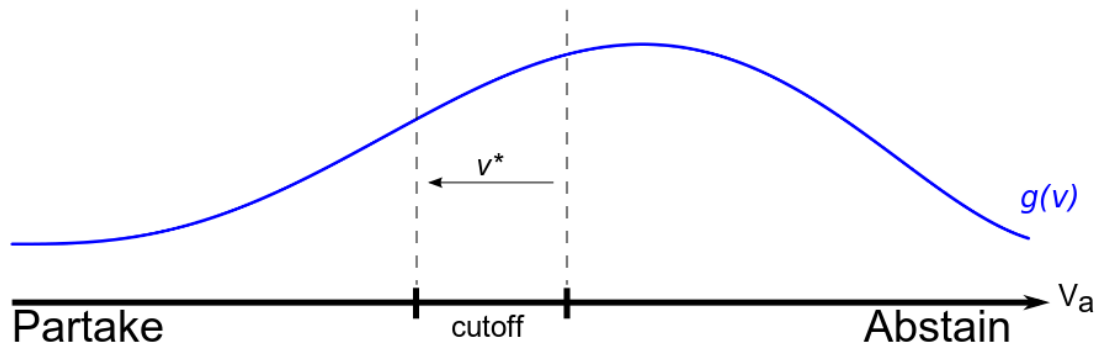


Figure 2

Geographic Boundaries of United States Courts of Appeals and United States District Courts

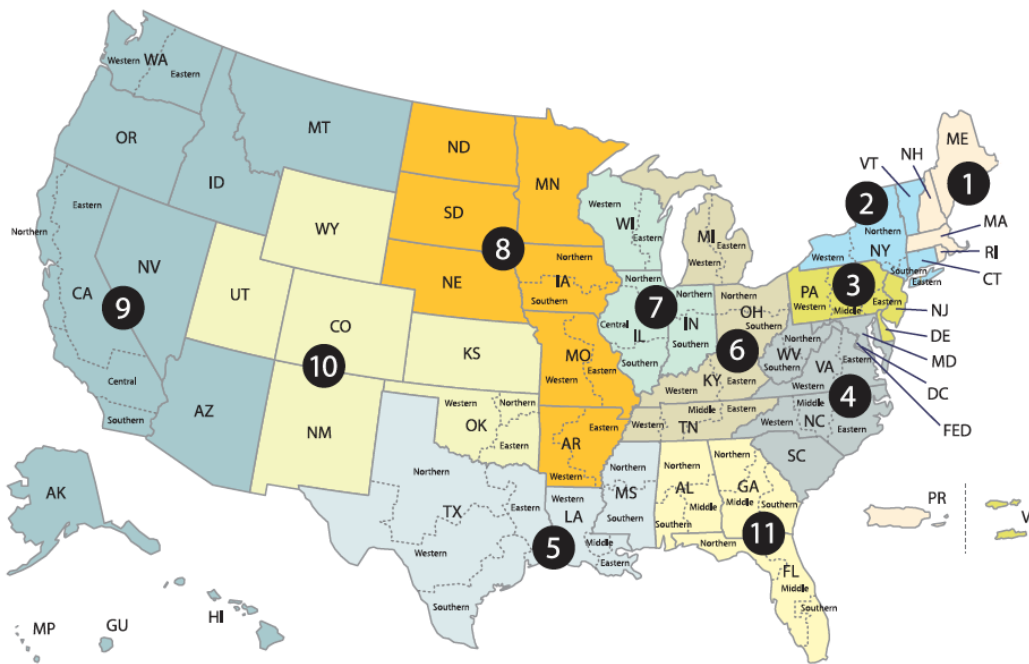


Figure 3

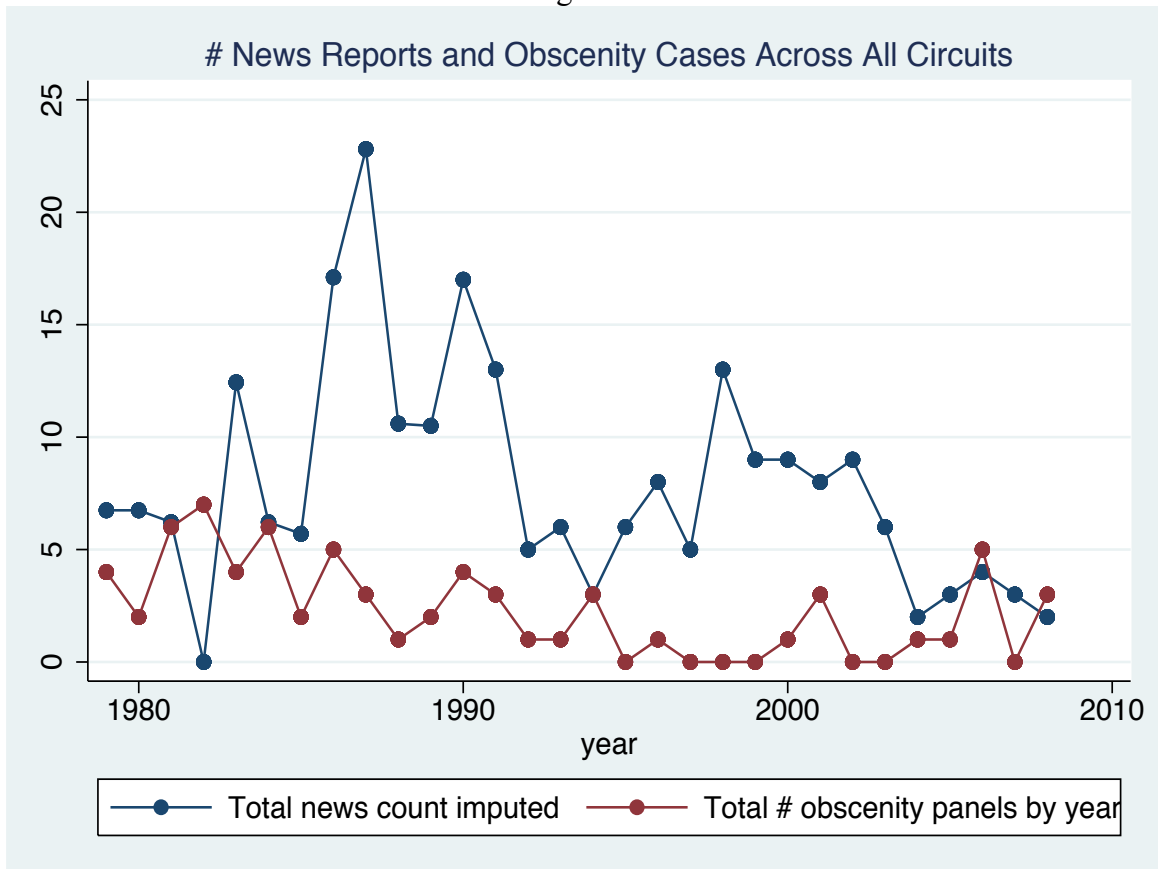


Figure 4: Appellate Obscenity Decisions, 1958-2008

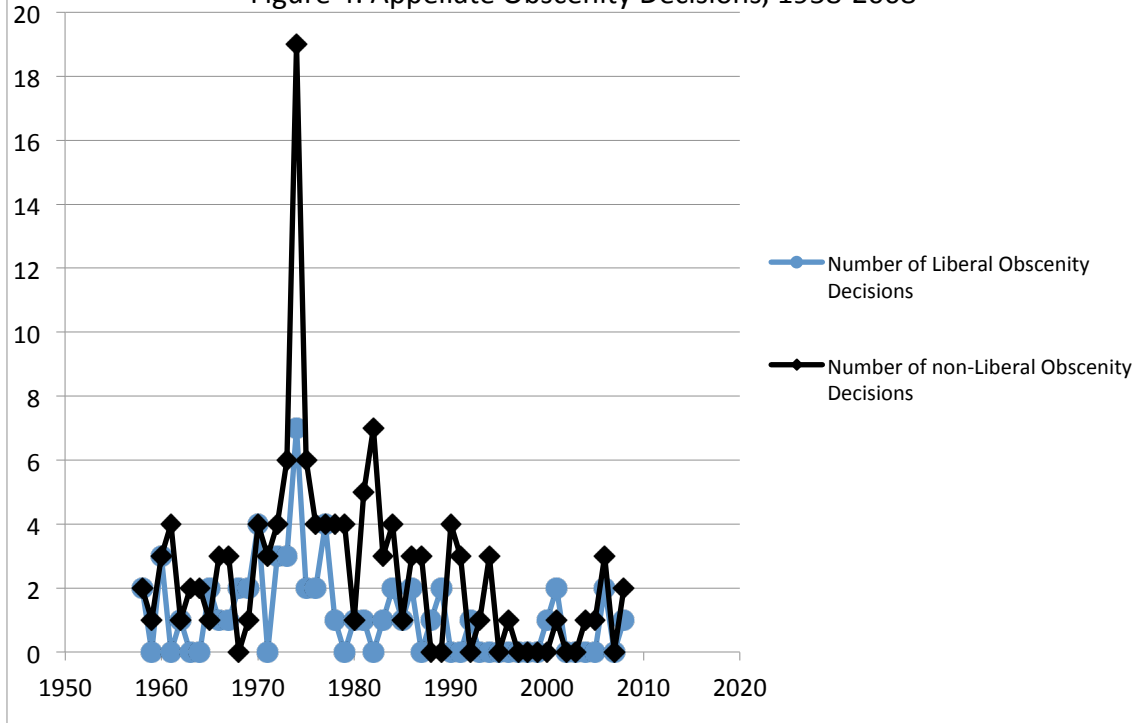


Figure 5A

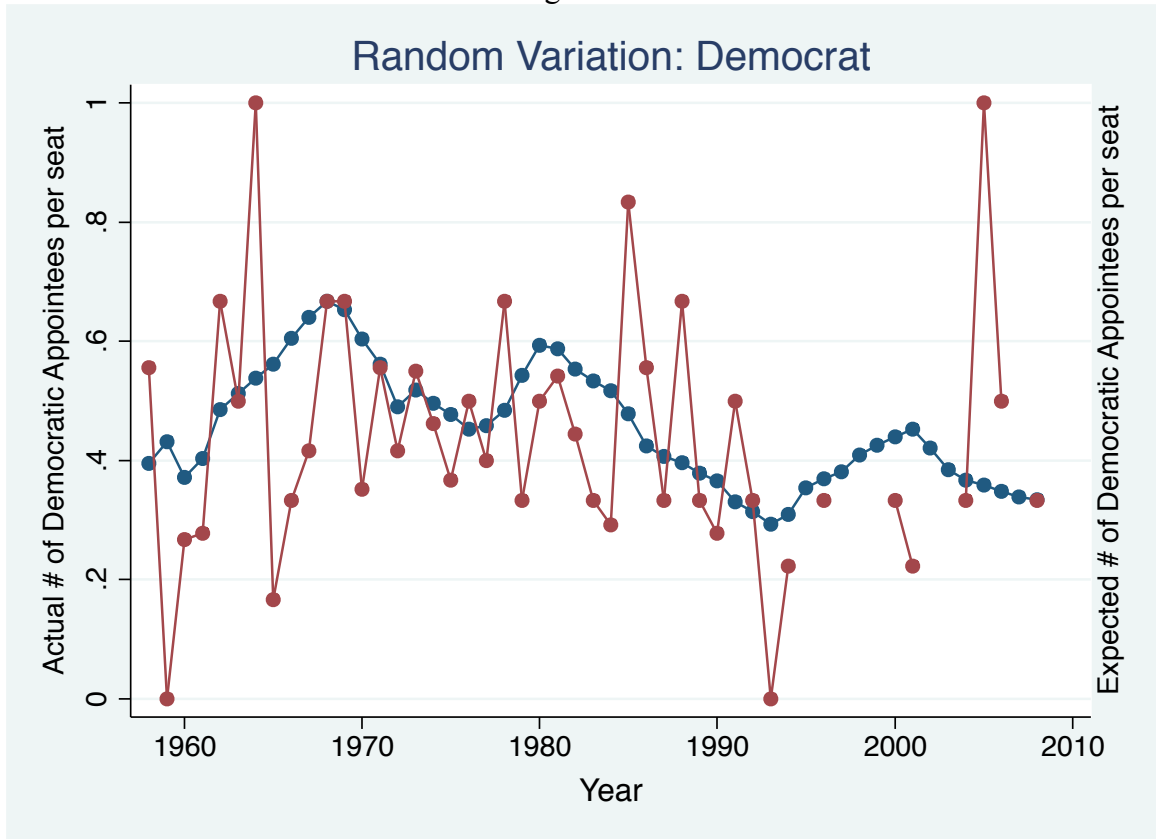


Figure 5B

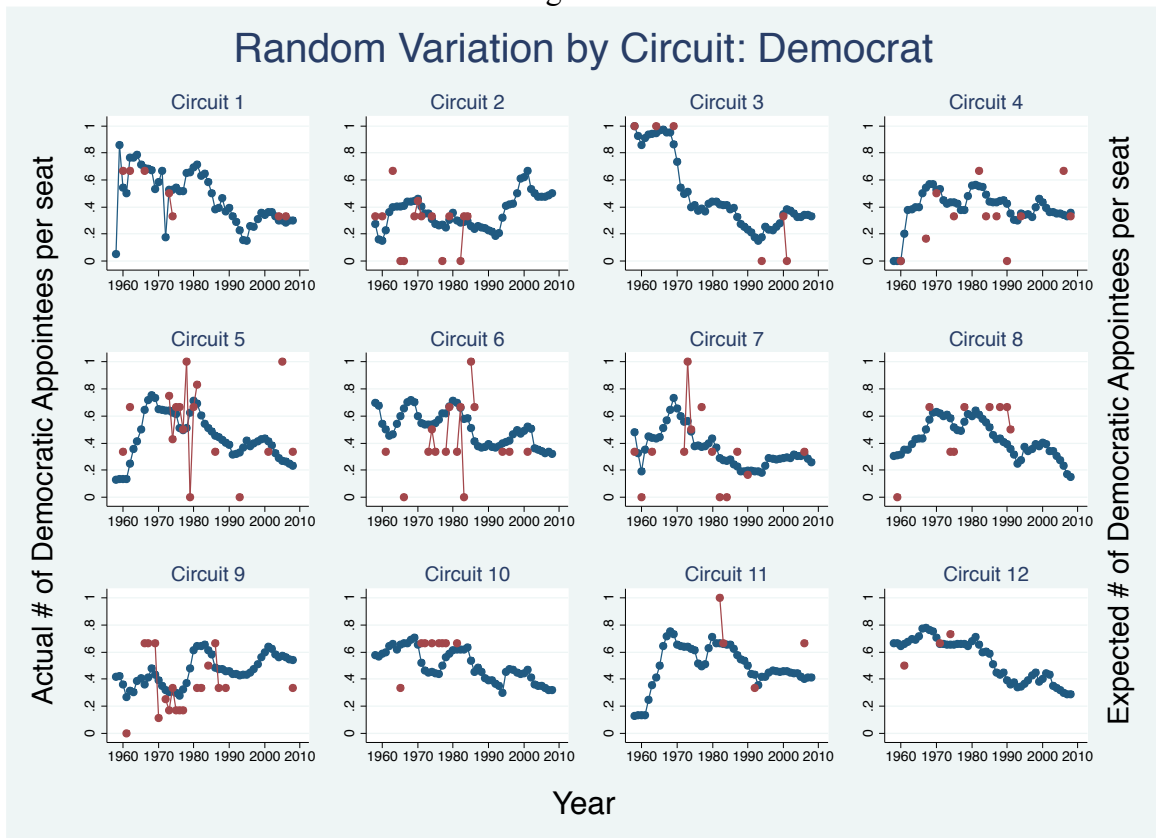


Figure 6: Randomization Check
P-Values of Democrat Appointee strings

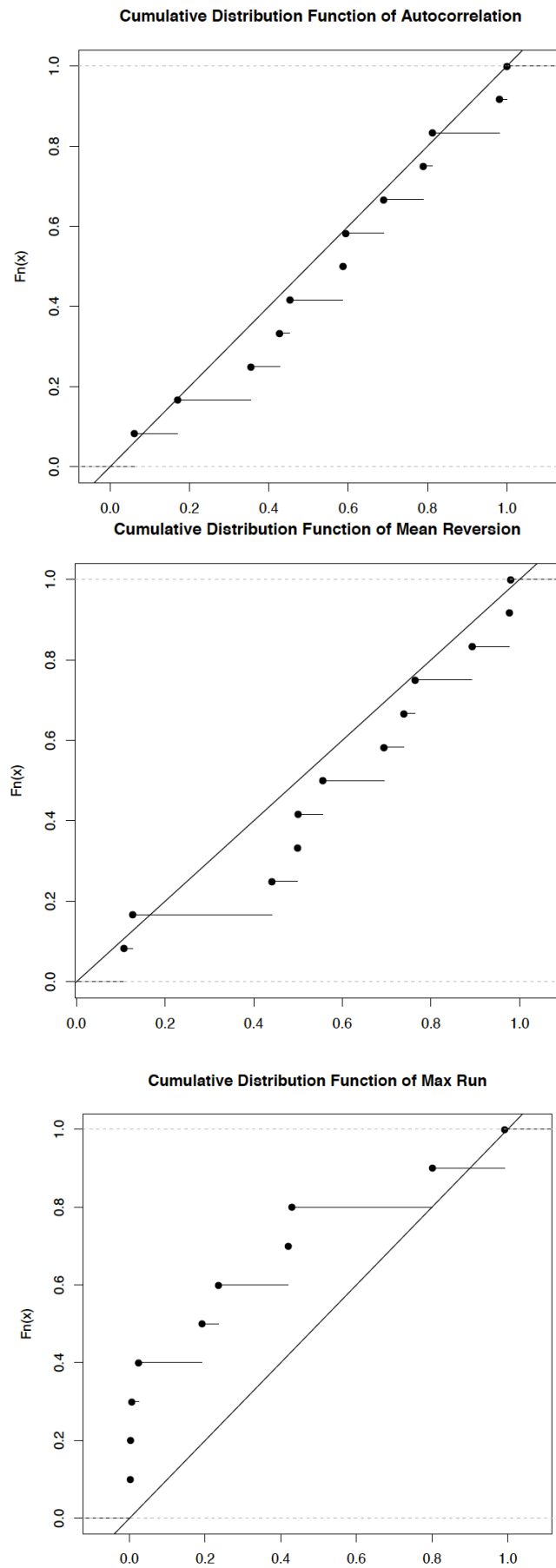


Figure 7A

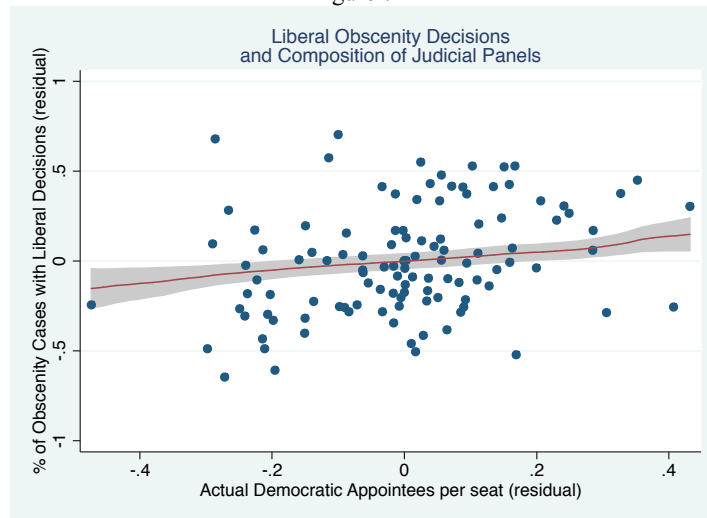
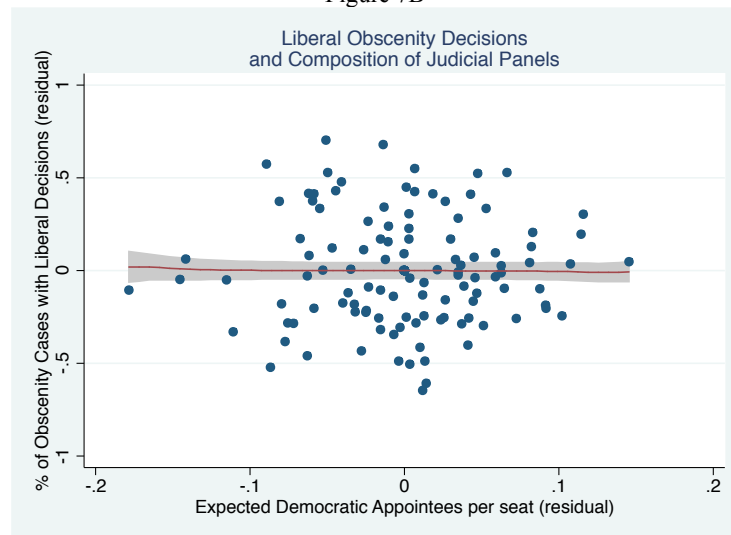


Figure 7B



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.

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For Online Publication

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

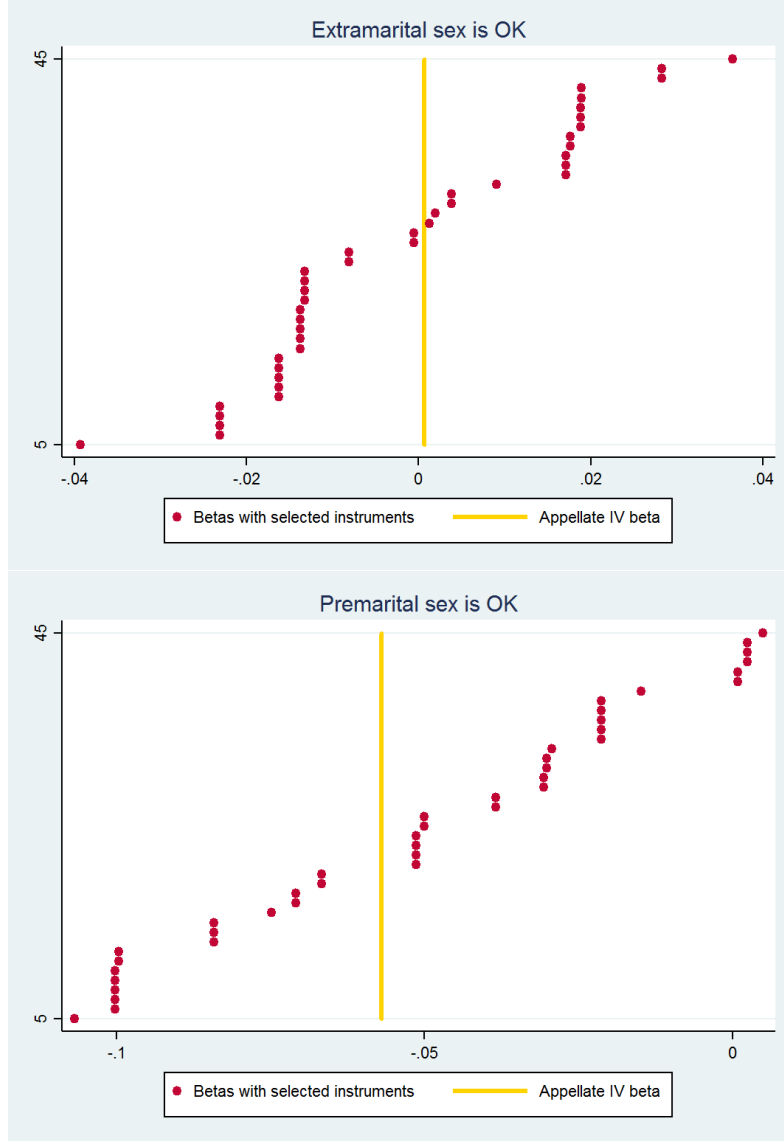
Citation	Case Name	Circuit	Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
500 F.2d 733	United States v. Hill	5	1974		0 prohibition on mailing obscene materials; transportation of obscene material	0 type of free speech expression
506 F.2d 1251	United States v. Carter	6	1974		0 prohibition on mailing obscene materials, use of common carrier to transport	0 obscene materials
505 F.2d 824	Smith v. United States	6	1974		0 interstate transportation of obscene materials	0 pornographic films
490 F.2d 76	United States v. Thevis	5	1974		0 transporting obscene material on a common carrier in interstate commerce	0 pornographic magazines
506 F.2d 511	United States v. Friedman	8	1974		0 prohibition on mailing obscene material	0 pornographic magazines
509 F.2d 368	United States v. Wornack	12	1974		0 interstate transportation of obscene matter, mailing obscene matter	0 pornographic magazines
502 F.2d 419	Huffman v. United States	12	1974		1 DC obscenity ordinance	0 pornographic magazines
503 F.2d 189	United States v. Gower	12	1974		0 DC obscenity ordinance	0 pornographic photographs and film
498 F.2d 934	United States v. Alexander	2	1974		0 prohibition on interstate transportation of obscene material	0 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 commerce	0 pornographic publications and film
515 F.2d 397	Illinois Citizens Committee for Broadcasting v. FCC	12	1974		0 broadcasting obscene material	0 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	undecar
491 F.2d 697	United States v. Harding	10	1974		1 receipt of obscene matter transported through interstate commerce	undecar
504 F.2d 1012	United States v. Wasserman	5	1974		1 prohibition on mailing obscene materials	undecar--somehow pornographic
524 F.2d 1244	United States v. Sleepoff	5	1975		0 prohibition on mailing obscene materials	"obscene advertising brochures"
514 F.2d 923	Clique v. United States	5	1975		1 prohibition on mailing obscene materials	letter containing sexually explicit language
523 F.2d 3	Walker v. Dillard	4	1975		1 VA state law criminalizing cursing at someone over the phone	Mrs. Walker swore at her neighbor over the phone
518 F.2d 20	United States v. 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	undecar
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. Lineisky	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	Pacific Foundation v. Federal Communications Commission	12	1977		1 FCC ruling	seven "patently offensive" words
562 F.2d 954	United States v. Glasman	5	1977		0 interstate transportation of obscene materials	sexually explicit films
558 F.2d 364	Amato v. Divine	7	1977		1 WI obscenity law	undecar
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 commerce	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	8	1979		0 interstate transportation of obscene materials	pornographic films and other materials
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	undecar--somehow pornographic
602 F.2d 1192	United States v. Grasi	5	1979		0 transporting obscene material on a common carrier in interstate commerce; mailing obscene material, etc.	adult movie theaters
631 F.2d 497	Entertainment Concepts II v. Maciejewski	7	1980		1 Westmont, IL city ordinances	pornographic magazines
610 F.2d 1353	Penthouse International Ltd. v. McAuliffe	5	1980		0 GA state obscenity law	adult entertainment providers raise a facial challenge to constitutionality of TX statute
648 F.2d 1020	Red Bluff Drive-In Inc. v. Vance	5	1981		1 TX obscenity statute	obscene magazines and a book
653 F.2d 381	United States v. Obscene Magazines, Book & Advertising Materials, et al.	9	1981		0 importation of obscene material	obscene words
638 F.2d 762	Reeves v. McCom	5	1981		0 Houston noise amplification ordinance prohibiting the amplification of obscene words	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. Baggett	11	1982		0 interstate transportation of obscene material with common carrier; interspersed obscenity films	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	undecar--somehow pornographic
674 F.2d 486	Turuso v. Cleveland Municipal Court	6	1982		0 OH obscenity statute	undecar--consolidated appeals
722 F.2d 1274	Janick 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	child pornography film
726 F.2d 1191	United States v. Thoma	7	1984		0 mailing child pornography for the purpose of sale	pornographic photos
747 F.2d 824	United States v. Petrov	2	1984		0 prohibition on mailing obscene material	pornographic playing cards
746 F.2d 458	United States v. Merrill	9	1984		0 mailing obscene materials	pornographic printed material
744 F.2d 1061	Olson v. Leake	4	1984		0 SC state obscenity law	sexually explicit magazines
750 F.2d 596	United States v. Various Articles of Merchandise, Seizure No. 170 & 182	9	1984		1 importation of obscene material	undecar--consolidated appeal
725 F.2d 482	J.R. Distrib. v. Ekenberry	9	1984		1 WA obscenity law	pornographic magazines
780 F.2d 1389	Upper Midwest Booksellers Assoc. v. Minneapolis	8	1985		0 Minneapolis city ordinance	pornographic pamphlets and magazines
779 F.2d 1177	Brooks v. Seiler	6	1985		1 OH state law preventing prisoners from receiving "obscene" or "inflammatory" materials	"commercial obscenity"
801 F.2d 740	Hoover v. Byrd	5	1986		0 TX obscenity statute	barren nude dancing
804 F.2d 1104	BSA, Inc. v. King County	9	1986		1 WA county ordinances	pornographic films
795 F.2d 765	United States v. Hurt	9	1986		0 mailing obscene materials	

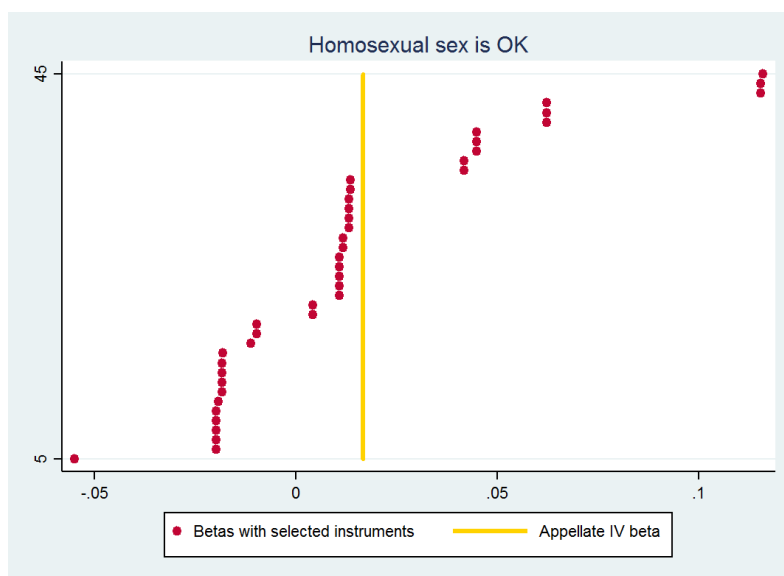
Citation	Case Name	Circuit	Year	Progressive	Type of Free Speech Regulation	Type of Free Speech Expression
803 F.2d 174	United States v. Marchant	5	1986	0	knowingly receiving child pornography	pornographic magazines featuring children
791 F.2d 463	Paduch v. Investment Entertainment, Inc.	6	1986	1	Paduch, KY obscenity ordinance	pornographic movie theaters, adult bookstores, etc.
826 F.2d 463	Moses v. County of Kenosha	7	1987	0	Kenosha County, WI obscenity ordinance	adult bookstores
819 F.2d 451	United States v. Guglielmi	4	1987	0	prohibition on mailing obscene material; use of common carrier to transport films depicting bestiality	materials sold at adult bookstores
816 F.2d 1326	Polykoff v. Collins	9	1987	0	AZ obscenity statute	a pornographic videotape
848 F.2d 923	United States v. Zangger	8	1988	1	mailing obscene material	"mainstream" pornographic materials
868 F.2d 1043	Rippinger v. Collins	9	1989	1	AZ obscenity statute	sexually explicit illustrations and photographs
867 F.2d 1188	Dworkin v. Hustler Magazine, Inc. v. King County	9	1989	1	none-Andrea Dworkin sued Hustler for libel, invasion of privacy, among other things	sexually explicit 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. Pyba	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. Ingenunson	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 communclear	
960 F.2d 134	Luke Records v. Navearro	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. Nelson	4	1994	0	federal obscenity statute	pornographic books/magazines
31 F.3d 135	United States v. Schen	3	1994	0	prohibition on mailing obscene material	sexually explicit film
74 F.3d 701	United States v. Thomas	6	1996	0	federal obscenity laws	an electronic bulletin board on which Thomas sold sexually explicit photos
230 F.3d 649	United States v. Various Articles of Merchandise, Schedule 287	3	2000	1	importation of obscene material	nude magazines from France and Germany
237 F.3d 251	United States v. Loy	3	2001	1	receiving and possessing child pornography; after conviction, Loy was pre convicted for sexually explicit films of children; prevented from viewing any pornographic images depicting child pornography	
248 F.3d 394	United States v. Fox	5	2001	0	receipt of child pornography through the internet	
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 harass 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 "lewd conduct"
466 F.3d 938	United States v. Eckhardt	11	2006	0	prohibition on making harassing 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. Blagovetch	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

A Visual Hausman test

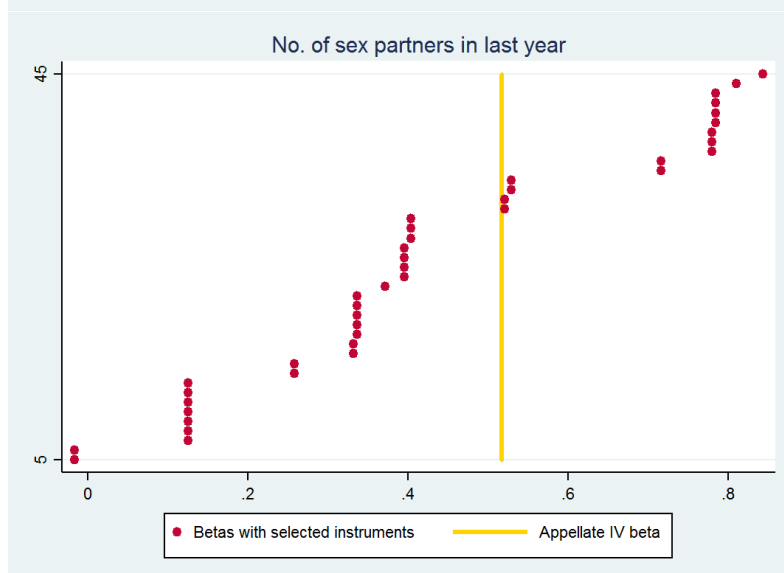
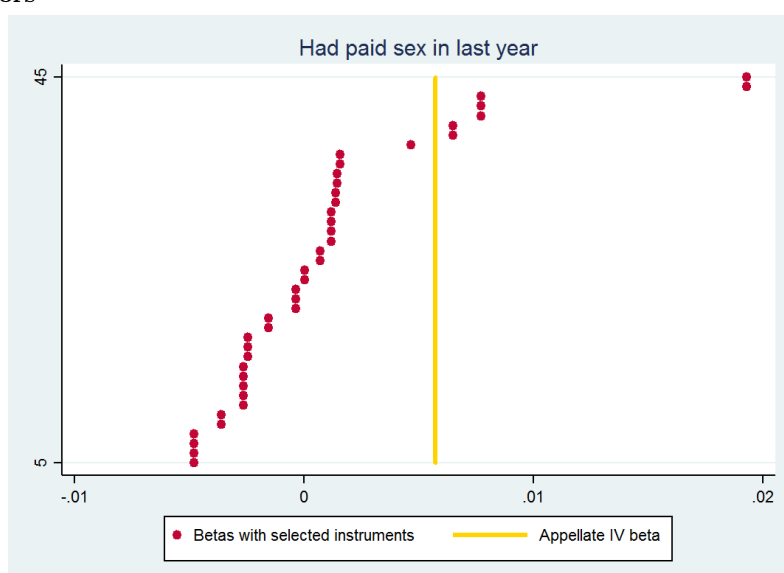
These figures present “visual Hausman” tests. We display the 2SLS estimates of $\frac{\sum_n \beta_{1n}}{n}$ using alternative instruments. The yellow line indicates the estimate from the “Appellate IV” specification where $Law_{c(t)}$ is instrumented for using the assignment of Democratic judges. The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

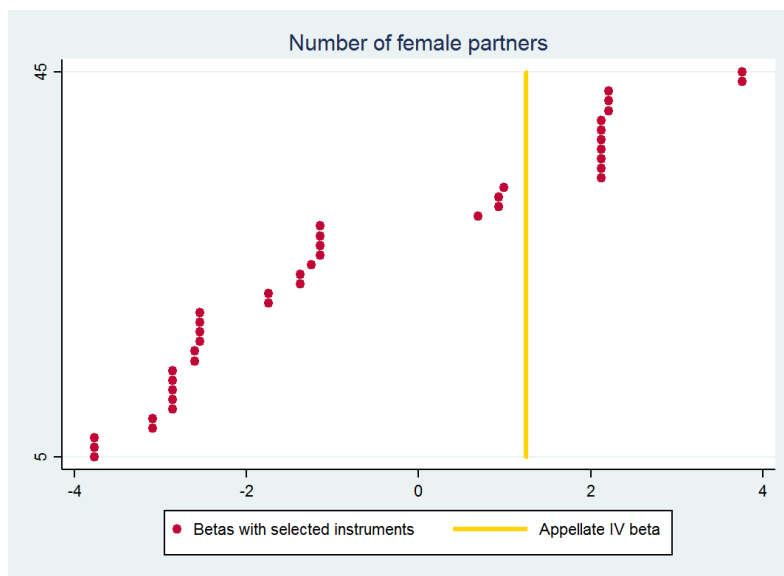
A.1 GSS attitudes



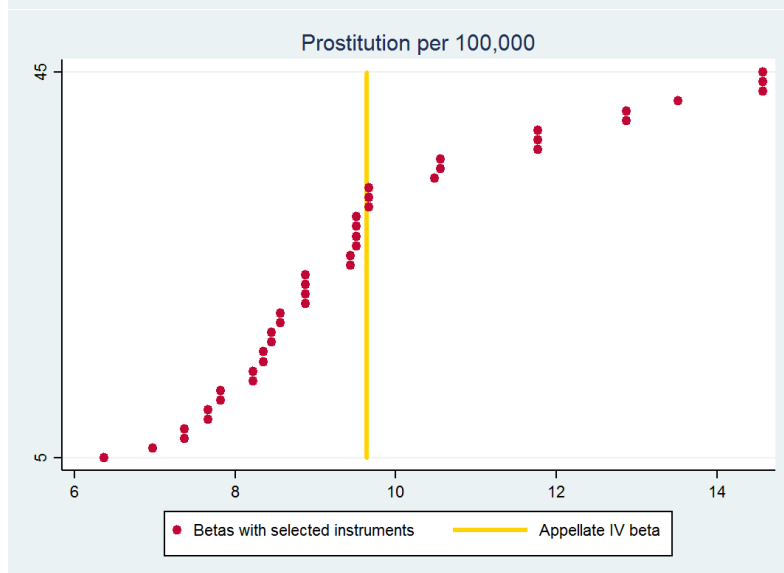
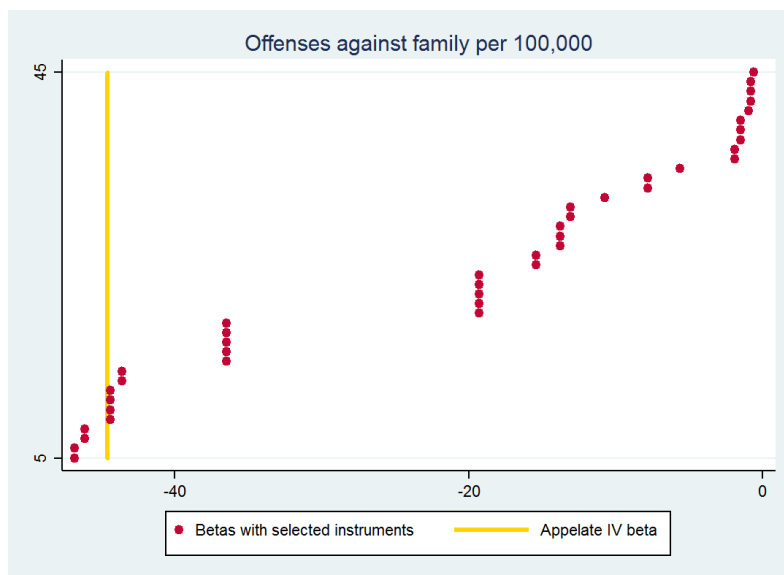


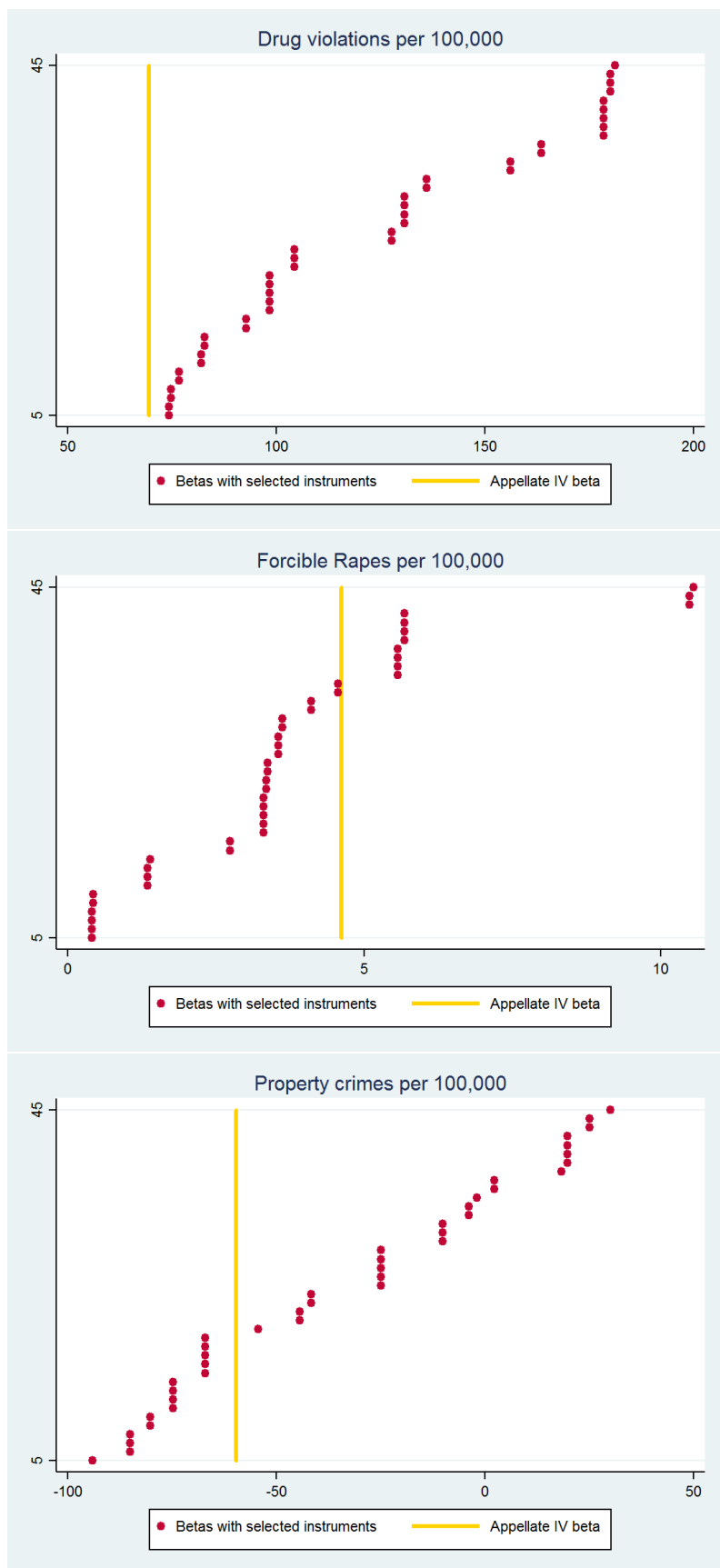
A.2 GSS behaviors



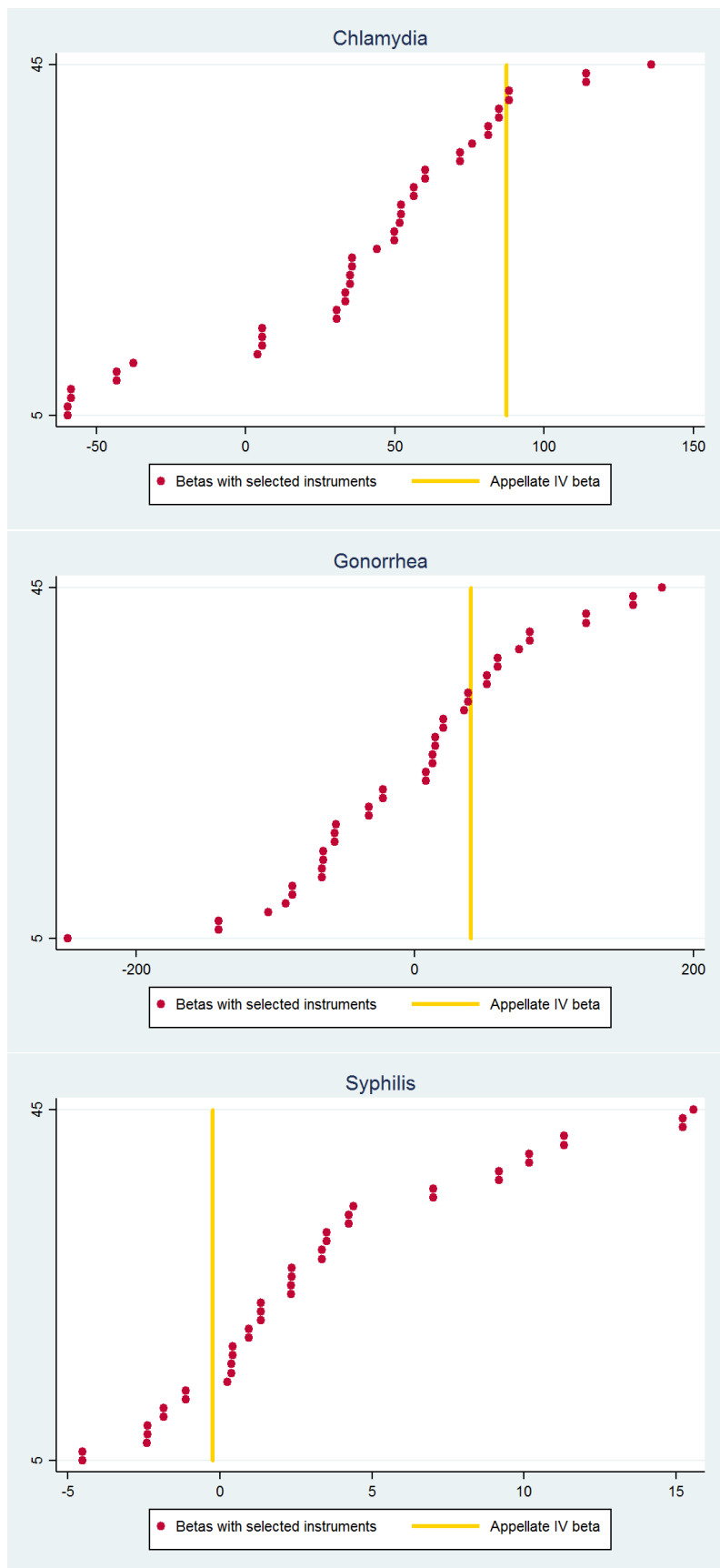


A.3 Crime data





A.4 Disease data



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

Dependent Variable	Extramartial Sex is OK					Premarital Sex is OK					Homosexual Sex is OK				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Wild BS %LE					Wild BS %LE					Wild BS %LE				
Proportion Progressive Free Speech	-0.000817 (0.000935)	0.00247 (0.0606)	-0.272 (0.486)	-0.0006585 (0.0142)	0.0188 (0.0187)	-0.0421* (0.0182)	-0.387 (0.284)	-0.0186 (0.292)	-0.0294 (0.0256)	-0.0284 (0.0278)	0.73 (0.0152)	0.0854 (0.0708)	-0.304 (0.887)	-0.0243 (0.0329)	-0.0224 (0.0341)
Appellate Decisions _{t-1}	-0.0192 (0.0147)	-0.0136 (0.0812)	-0.0501 (0.410)	-0.0179 (0.0161)	-0.0310+ (0.0159)	0.0611 (0.0358)	0.0856 (0.413)	0.00340 (0.926)	0.0644+ (0.0347)	0.0614+ (0.0365)	0.45 (0.0358)	-0.0314 (0.140)	-0.232 (0.510)	0.0125 (0.0411)	0.0137 (0.0447)
Appellate Decisions _t	0.00770 (0.0111)	-0.0547 (0.0741)	0.239 (0.670)	0.0183 (0.0193)	0.0389+ (0.0233)	-0.0613+ (0.0281)	-0.0947 (0.335)	-0.224 (0.785)	-0.0644+ (0.0351)	-0.0627+ (0.0356)	0.39 (0.0242)	-0.0133 (0.144)	-0.165 (0.909)	-0.0410 (0.0213)	-0.0369 (0.0190)
Proportion Progressive Free Speech	-0.00296 (0.0120)	0.0484 (0.138)	0.0430 (0.570)	0.0209 (0.0198)	0.0197 (0.0232)	0.00118 (0.0286)	0.00118 (0.335)	0.119 (0.515)	0.0190 (0.0309)	0.0299 (0.0328)	0.69 (0.0241)	0.126 (0.238)	-0.214 (0.909)	0.0772** (0.0213)	0.0904** (0.0190)
Appellate Decisions _{t-2}	-0.0303 (0.0137)	-0.0303 (0.0393)	-0.287 (1.447)	0.0175 (0.0150)	0.00465 (0.0289)	-0.00424 (0.0198)	-0.0823 (0.497)	0.259 (3.728)	0.0260 (0.0282)	0.0278 (0.0302)	0.85 (0.0306)	-0.114+ (0.0660)	0.454 (2.168)	-0.0361 (0.0317)	-0.0364 (0.0426)
Proportion Progressive Free Speech	0.0142 (0.0109)	0.0256+ (0.0254)	-0.0303 (0.161)	0.00224 (0.0179)	0.00661 (0.0194)	0.00468 (0.0180)	0.0491 (0.263)	0.0792 (2.421)	0.0284+ (0.0158)	0.0153 (0.0164)	0.94 (0.0147)	0.165+ (0.0859)	-0.0601 (0.462)	-0.00737 (0.0304)	-0.0151 (0.0252)
Appellate Decisions _{t-4}	18874 0.014	18874 0.012	18874 .	18874 0.013	18874 0.013	18801 0.028	18801 0.014	18801 0.015	18801 0.028	18801 0.028	18073 0.057	18073 0.052	18073 .	18073 0.057	18073 0.056
N	18874	18874	18874	18874	18874	18801	18801	18801	18801	18801	18073	18073	18073	18073	18073
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV
District IV	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
Aggregation Level	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 _{it} 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 _{it} 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 _{it} 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 L _{it} >0 lags	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 L _{it} >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 _{it} + L _{it} >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 _{it} 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 L _{it} >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 _{it} + L _{it} >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 any cases in that Circuit-year, 6-year lagged community standards (Circuit average response to whether sexual materials lead to a breakdown of morals), and individual level controls: age, gender, race, and college education. Instruments for proportion of progressive free speech decisions are Democratic appointees per seat assigned to Appellate free speech cases in a Circuit-year. Survey weights are provided by GSS.

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

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

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

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

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

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

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

Dependent Variable	Paid Sex					Wild BS					Number of Partners per Year										Wild BS					Number of Female Partners					Wild BS	
						%LE															%LE										%LE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)					
Proportion Progressive Free Speech	-0.00176 (0.00216)	0.00381 (0.0142)	-0.00422 (0.00364)	-0.00521* (0.00254)	-0.00279 (0.00249)	0.32 (0.126)	-0.537 (1.020)	0.439* (0.179)	0.0509 (0.201)	0.105 (0.148)	-0.281 (1.827)	-1.281 (4.829)	-1.319 (3.749)	-3.370 (2.265)	-2.207 (2.347)																	
Appellate Decisions _{t+1}	0.00600 (0.00360)	-0.00621 (0.0360)	0.0123* (0.00525)	0.00767+ (0.00414)	0.00627+ (0.00341)	0.14 (0.241)	2.025 (2.608)	-0.159 (0.280)	-0.130 (0.245)	-0.240 (0.220)	3.111 (1.805)	1.702 (24.70)	-0.657 (7.551)	5.432* (2.461)	5.176* (2.130)																	
Proportion Progressive Free Speech	-0.000137 (0.00344)	-0.00135 (0.0104)	0.00212 (0.00266)	0.00266 (0.00355)	0.00115 (0.00335)	0.72 (0.405)	1.291* (0.615)	0.994* (0.423)	1.177** (0.434)	0.861* (0.400)	3.829* (1.280)	-0.0335 (2.253)	8.222** (5.381)	6.648** (2.402)	7.772** (1.985)																	
Appellate Decisions _{t-1}	0.00632** (0.00201)	0.0218 (0.0155)	0.00731+ (0.00431)	0.00631* (0.00272)	0.00713** (0.00203)	0.15 (0.192)	-0.558 (1.068)	0.421+ (0.220)	0.0901 (0.187)	0.0954 (0.171)	3.262+ (1.526)	2.834 (3.765)	9.065+ (5.381)	4.172+ (2.402)	4.958* (1.985)																	
Proportion Progressive Free Speech	0.00499* (0.00223)	0.0205 (0.0207)	0.00525 (0.00362)	0.00531+ (0.00319)	0.00584** (0.00222)	0.08 (0.196)	-0.767 (1.223)	-0.394 (0.375)	0.0709 (0.145)	-0.131 (0.166)	0.79 (0.927)	3.416 (6.657)	8.824* (4.291)	2.078 (1.743)	4.019+ (2.132)																	
Appellate Decisions _{t-3}	-0.000925 (0.00208)	-0.00612 (0.00676)	0.00109 (0.00206)	0.000490 (0.00280)	0.000132 (0.00261)	0.88 (0.111)	0.593 (0.800)	0.104 (0.159)	0.108 (0.123)	0.0724 (0.107)	1.268 (0.938)	-1.661 (9.933)	1.004 (2.663)	2.284+ (1.262)	3.217** (1.133)																	
N	16659	16659	16659	16659	16659	15346	15346	15346	15346	15346	13833	13833	13833	13833	13833																	
R-sq	0.002	.	0.002	0.002	0.002	0.010	.	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	Y	Y	Lasso IV	Lasso IV	N	Y	Y	Lasso IV	Lasso IV																	
District IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV																	
Aggregation Level	Individual										Individual										Individual					Lasso IV		Lasso IV				
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 _{it} 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 _{it} 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 _{it} 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 Law _{it} >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 Law _{it} >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 _{it} +1 Law _{it} >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 _{it} 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 Law _{it} >0 leads	0.270	0.409	0.252	0.603	0.238	0.239	0.675	0.293	0.349	0.267	0.235	-0.013	0.049	0.496	0.299																	
P of Law _{it} +1 Law _{it} >0 leads	0.085	0.914	0.126	0.011	0.058	0.171	0.639	0.094	0.443	0.209	0.364	0.789	0.293	0.065	0.094																	

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

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

Dependent Variable	Number of Partners per Year (reported by Men)					Number of Female Partners (reported by Men)					Extramarital Sex (reported by Men)					Wild BS %LE	
						Wild BS %LE					Wild BS %LE						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)		
Proportion Progressive Free Speech	0.160	-2.660	0.749*	-0.0470	0.0501	1.466	-7.887	-5.880	-5.195	-2.703	0.32	-0.0142	-0.0747	0.00240	-0.0533	-0.0298	0.65
Appellate Decisions _{t-1}	(0.336)	(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	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	1.855+	2.653	2.266*	2.767**	2.080*	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	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	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	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	N	Y	Lasso IV	Lasso IV	Lasso IV	N	Y	Lasso IV	Lasso IV	Lasso IV		
District IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	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 _t 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 _t 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 _t 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_{t-1} >0 lags	0.237	-0.154	0.231	0.073	0.185	-1.190	-1.596	-5.435	-2.643	-3.167	-0.023	-0.027	-0.021	-0.030	-0.027		
P-value of 1 M_{t-1} >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 _t +1 M_{t-1} >0 lags	0.008	0.003	0.000	0.001	0.001	0.005	0.149	0.010	0.010	0.000	0.000	0.989	0.000	0.000	0.000		
Typical Law _t 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	0.001	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_{t-1} >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 _t +1 M_{t-1} >0 leads	0.357	0.207	0.135	0.716	0.490	0.604	0.281	0.269	0.109	0.161	0.077	0.809	0.225	0.005	0.034		

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

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

Dependent Variable	Divorced or Separated (older than 40)					Divorced or Separated (40 or younger)					Premarital Sex is OK (reported by Women)					Wild BS %LE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)		
	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	Wild BS %LE	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	-0.00836	-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)	(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+	0.48
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)	(0.0434)	(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	0.21
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)	(0.0336)	(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	0.52
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)	(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	0.49
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)	(0.0274)	(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	0.108	-0.0568	-0.0608	-0.0608	0.25	-0.00355	0.338	-0.0105	0.0276	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)	(0.0201)	(0.478)	(3.779)	(0.0192)	(0.0186)	
N	10778	10778	10778	10778	10778	6368	6368	6368	6368	6368	10693	10693	10693	10693	10693	10693	
R-sq	0.035	0.031	0.032	0.033	0.034	0.021	0.021	0.015	0.019	0.019	0.036	0.036	0.036	0.036	0.036	0.036	
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Lasso IV	Lasso IV	Y	Lasso IV	Lasso IV	Lasso IV	Lasso IV	Y	Y	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV	N	N	Lasso IV	N	Lasso IV	N	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	0.585	
Average Law _{it} 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	0.023	
P-value of Law _{it} 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	0.001	0.000	
P-value of Law _{it} 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	0.004	
Average 1 Law _{it} -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	-0.004	
P-value of 1 Law _{it} >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	0.000	
P of Law _{it} +1 Law _{it} >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	0.430	
Typical Law _{it} 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	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	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	-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	0.001	0.000	
P of 1 Law _{it} >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	0.828	
P of Law _{it} +1 Law _{it} >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	0.978	

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

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

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

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

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

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

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

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

Dependent Variable	Offenses Against Family and Children per 100,000					Community Vices per 100,000					Wild BS %LE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
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		N	Y	Y	Lasso IV	Lasso IV	
District IV	N	N	N	Lasso IV	Lasso IV		N	N	Lasso IV	Lasso IV	Lasso IV	
Aggregation Level				ORI Agency - Year	Lasso IV				ORI Agency - Year	Lasso IV	Lasso IV	

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

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

<i>Dependent Variable</i>	Drug Violations per 100,000										Forcible Rapes per 100,000										Wild BS %LE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)		
Proportion Progressive Free Speech	12.59	254.6	-74.52	144.0	105.7	2.231+	6.604	-0.923	14.60	0.838	0.99											
Appellate Decisions _{t-1}	(22.86)	(176.1)	(156.3)	(98.99)	(332.2)	(1.220)	(4.628)	(4.384)	(13.03)	(5.805)												
Proportion Progressive Free Speech	58.97	126.0+	272.4+	141.7**	62.82	-0.648	4.394	8.918	11.18	9.335	0.02											
Appellate Decisions _t	(41.18)	(68.06)	(144.5)	(48.43)	(221.5)	(0.867)	(3.218)	(8.373)	(15.11)	(7.986)												
Proportion Progressive Free Speech	10.92	37.83	-19.57	56.69	294.1	-0.105	4.935	3.665	11.92	2.979	0.58											
Appellate Decisions _{t-1}	(39.35)	(31.15)	(212.1)	(36.78)	(397.7)	(2.245)	(5.333)	(10.14)	(8.537)	(11.75)												
Proportion Progressive Free Speech	3.219	10.45	-10.53	-4.894	-69.43	-0.273	4.122	2.749	12.37	2.301	0.53											
Appellate Decisions _{t-2}	(22.50)	(50.28)	(197.1)	(43.31)	(201.2)	(0.948)	(4.242)	(5.573)	(10.11)	(6.752)												
Proportion Progressive Free Speech	30.58	67.50	36.36	65.38	127.1	0.469	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	-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																						
District IV																						
Aggregation Level																						
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 _{it} 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 _{it} 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 _{it} leads	0.594	0.148	0.633	0.146	0.750	0.097	0.154	0.833	0.262	0.885												
Average 1[M _{it} >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 _{it} >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 _{it} +1[M _{it} >0] lags	0.016	0.256	0.005	0.001	0.269	0.536	0.309	0.004	0.000	0.008												
Typical Law _{it} 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 _{it} >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 _{it} +1[M _{it} >0] leads	0.042	0.198	0.352	0.376	0.870	0.294	0.128	0.850	0.239	0.749												

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

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

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

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

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

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

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

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

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

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

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

Dependent Variable	Chlamydia Incidence					Wild BS		Gonorrhea Incidence					Wild BS		Syphilis Incidence					Wild BS	
						%LE						%LE						%LE			
	(1)	(2)	(3)	(4)	(5)		(6)	(7)	(8)	(9)	(10)		(11)	(12)	(13)	(14)	(15)	%LE			
Proportion Progressive Free Speech	-11.04 (13.64)	142.5 (137.3)	-171.7 (549.8)	80.06 (148.5)	67.48 (100.2)	0.54	2.683 (10.65)	47.26 (41.09)	-306.2 (2436.5)	70.32 (46.22)	-245.6 (1767.9)	0.17	0.327 (0.725)	-3.205 (4.190)	7.592 (16.24)	-5.412 (3.950)	3.787 (18.11)	0.90			
Appellate Decisions _{t+1}	-1.047 (14.03)	186.1* (94.12)	-71.07 (818.5)	-84.61 (374.9)	249.2* (115.2)	0.07	4.518 (10.42)	64.70 (66.96)	-30.98 (816.6)	133.6+ (71.03)	-47.10 (630.0)	0.78	-0.386 (0.922)	-2.318 (6.006)	10.55 (13.29)	-5.495 (5.245)	9.191 (11.62)	0.31			
Proportion Progressive Free Speech	14.21 (19.56)	70.15 (67.48)	446.0 (1431.5)	380.1 (247.2)	209.1 (194.4)	0.40	8.016 (11.05)	44.79 (75.12)	457.1 (2026.4)	115.3 (81.69)	391.2 (1613.1)	0.33	-1.263 (0.857)	-6.492 (7.131)	6.928 (20.97)	-8.808 (6.121)	9.680 (21.27)	0.93			
Appellate Decisions _{t-1}	34.45+ (17.44)	43.20 (207.0)	76.93 (320.3)	157.3 (158.0)	-124.4 (304.5)	0.39	20.84 (13.36)	56.46 (61.55)	365.9 (1097.6)	99.35 (87.67)	311.6 (762.3)	0.80	-0.878 (0.848)	-7.445 (6.115)	3.459 (15.45)	-9.131 (7.085)	7.140 (13.62)	0.28			
Proportion Progressive Free Speech	3.188 (16.69)	89.09 (78.89)	264.8 (1192.9)	102.2 (431.6)	-79.83 (257.6)	0.52	18.67 (12.52)	72.87+ (38.36)	-563.6 (2679.5)	118.4 (72.55)	-500.6 (2039.9)	0.82	-0.643 (0.894)	-2.442 (5.010)	0.368 (23.47)	-1.975 (5.384)	-3.938 (18.57)	0.15			
Appellate Decisions _{t-3}	14.34 (17.59)	48.46 (117.1)	-346.0 (925.0)	355.2 (329.2)	-5.852 (193.9)	0.98	12.58 (11.98)	20.34 (36.27)	615.8 (2585.3)	83.74 (89.54)	521.4 (1991.3)	0.24	-0.228 (1.238)	3.261 (5.082)	0.0919 (26.07)	6.604 (8.815)	5.797 (25.56)	0.65			
Appellate Decisions _{t-4}																					
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	N	Lasso IV	Lasso IV	Lasso IV	N	N	N	Lasso IV	Lasso IV	Lasso IV			
Aggregation Level	State - Year						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	0.946	0.875	0.859		0.731	0.477	0.473	0.621	0.577				
P of Law _{ct} +1/[M _{ct} >0] leads	0.709	0.252	0.756	0.432	0.538		0.184	0.230	0.930	0.091	0.930		0.822	0.773	0.815	0.842	0.718				

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

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

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

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

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

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

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

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

Dependent Variable	1973-1993										1980-2000					
	Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK		Paid Sex		Community Vices		Partners Per Year		Homosexual Sex is OK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Proportion Progressive Free Speech	-0.00252	-0.00526	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 ₊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 ₋₁	(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 ₋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 ₋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 ₋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 ₋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	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y							
Mean dependent variable	0.003	0.003	5.060	5.060	1.130	1.130	0.219	0.219	0.003	0.003	5.104	5.104	1.129	1.129	0.267	0.267							
Average Law _{it} effect	0.004	-0.002	7.463	-2.050	-0.724	-0.169	-0.003	-0.050	0.003	0.005	1.364	9.181	0.043	0.468	0.001	0.017							
P-value of Law _{it} lags	0.083	0.000	0.108	0.000	0.101	0.047	0.394	0.008	0.036	0.123	0.056	0.050	0.348	0.031	0.771	0.000							
P-value of 1[M _{it} >0] lags	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 _{it} >0] lags	-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 _{it} >0] lags	0.087	0.000	0.202	0.004	0.318	0.164	0.193	0.195	0.129	0.017	0.089	0.025	0.537	0.133	0.064	0.600							
P of Law _{it} +1[M _{it} >0] lags	0.831	0.000	0.186	0.007	0.094	0.127	0.182	0.001	0.095	0.450	0.002	0.181	0.313	0.000	0.000	0.000							
Typical Law _{it} effect	0.000	-0.000	0.545	-0.150	-0.026	-0.006	-0.000	-0.005	0.000	0.000	0.064	0.430	0.001	0.012	0.000	0.001							
Unconditional effect - progressive	0.000	-0.000	0.301	-0.054	-0.012	0.007	-0.000	-0.004	0.000	0.000	0.023	0.260	0.004	0.012	0.000	0.001							
Unconditional effect - conser	-0.001	0.000	-0.630	0.238	0.060	0.055	0.001	0.006	-0.000	-0.000	-0.104	-0.460	0.013	0.000	0.001	-0.000							
Unconditional effect - all	-0.001	0.000	-0.330	0.180	0.048	0.062	0.001	0.003	-0.000	-0.000	-0.079	-0.203	0.017	0.012	0.002	0.000							
P of 1[M _{it} >0] leads	0.801	0.582	0.416	0.768	0.245	0.154	0.714	0.847	0.253	0.403	0.373	0.192	0.246	0.620	0.111	0.971							
P of Law _{it} +1[M _{it} >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.