Laws, Beliefs, and Backlash[1]

Brian Wheaton[2]
UCLA, Anderson School of Management
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Abstract: Do laws affect the beliefs and attitudes held by the public? I set up a model wherein families care about their children's beliefs, which are shaped by a combination of parental actions and the law set by society. These straightforward assumptions are sufficient to generate systematic backlash against laws – individuals move in the opposite direction of the law in an attempt to preserve the values which are important to them and are placed under threat by the law. Next, I turn to survey data from the ANES to test the implications of the model. I first focus in-depth on one specific case: the state Equal Rights Amendments (ERAs), which aimed to legislate gender equality. Using a dynamic difference-in-differences identification strategy, I find robust evidence that ERA passage leads to a sharp backlash amongst men in particular, who exhibit sharply more negative attitudes toward male/female equality. This shift translates into a significant increase in Republican vote share, worsened material outcomes for women, and increased marital strife. I also test and confirm the other implications of the model – such as the fact that the backlash is passed on to the next generation and that it endures more strongly in ideologically-homogeneous communities. Next, I provide evidence against a variety of alternative mechanisms. And finally, stepping back from the ERAs, I show that virtually every major U.S. social policy law of the past half-century has induced significant backlash. Taken as a whole, these findings suggest that aggressive pushes for social change through legislation may come at a significant cost.

1 Introduction

The literature on law and economics has increasingly distinguished between the *functional* role of laws and the *expressive* role of laws. That is, most laws serve dual purposes: they provide civil or criminal penalties which incentivize compliance (functional), but they also provide a signal of society's goals, norms, and standards for acceptable behavior (expressive). Laws vary quite broadly in the extent to which they exhibit each of these two roles. Deeply-buried legal

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[2] Contact: brian.wheaton@anderson.ucla.edu

clauses on the precise conditions under which certain tax credits apply may provide relatively little in terms of signaling norms, but they come with a well-defined incentive (i.e., the threat of audit) not to deviate from the law. On the other hand, a gay marriage law – in addition to legally allowing marriage for gay people – may plausibly influence the attitudes and beliefs of heterosexual individuals who are not otherwise functionally bound by the law nor face any threat of legal penalty from it. Indeed, a statement such as this can be made for many social policy laws.

But if social policy laws do have an effect on attitudes and beliefs, what effect will they have? A straightforward and sensible conjecture would be that, by legislating better conditions or enhanced treatment for a certain group of individuals, public attitudes toward that group would also become more positive. However, it is also possible that legislating better conditions or enhanced treatment for a group could lead to backlash – that is, to attitudes toward the group becoming more *negative*. In a mechanism not dissimilar from a social version of crowd-out, individuals may push back against the law as they seek to preserve their preferred norms. Furthermore, if these expressive effects of the law do indeed tend to push in the direction of backlash, then in cases where the functional effects of the law are minimal (in terms of bettering the circumstances of the group in question), the backlash may actually *overwhelm* any direct improvements produced by the law. This is a fundamentally empirical question, and distinguishing between the aforementioned hypotheses is the subject of this paper.

To guide this effort, I begin by constructing a model of the effect of social policy laws on beliefs and attitudes expressed by the public. In this model, each individual has preferences over a continuous political spectrum. Broadly speaking, they may be conservative, moderate, or liberal, and this is represented by their bliss point. Individuals prefer to take actions as close as possible to their bliss point. Actions may represent the attitudes they express to others, the votes they cast, or a range of other ideologically-coded activities. Individuals also prefer people around them – such as their children, friends, and neighbors – to have and maintain an ideology close to their own. That is, liberals prefer to live in a society that is and will continue to be liberal; conservatives prefer to live in a society that is and will continue to be conservative.

Individuals' ideology is shaped by a weighted average of their priors, the actions taken by people around them, and the law. I show that these assumptions are sufficient to generate systematic backlash against laws.

Intuitively, a law that clashes with an individual's ideological preferences places the persistence of that ideology under threat. Society will begin to move away from the individual's preferred ideology and toward the law. Consequently, individuals find it optimal to move in the opposite ideological direction of the law in an attempt to push back, influence those around them, and preserve values which are important to them to the extent they can. For example, a conservative facing a newly-implemented liberal law will find it optimal to express *more* conservatism he/she they would under a conservative law in order to temper the influence of the liberal law on their family and friends. And a liberal facing a newly-implemented liberal law is able to reduce his/her expressions of liberalism and rely, in part, on the law to influence people around them. The model also yields the additional prediction that this backlash against the law will persist most strongly and successfully in ideologically-homogeneous communities, and a version of the model focusing specifically on parents' desire to transmit their ideology to their children delivers the added implication that the backlash should be passed on to the next generation.

With these theoretical results in mind, I move to the data – focusing first on the state Equal Rights Amendments of the 1970s, which aimed to legislate equality between men and women along various dimensions. The 1970s featured a very public and often-contentious debate as to whether an Equal Rights Amendment (ERA) should be added to the U.S. Constitution and the constitutions of the individual states. These proposals involved adding language to their respective constitutions declaring men and women to be fundamentally equal and subject to equal rights and treatment. The ERA was highly expressive in nature; that is, even its advocates conceded that the legal consequences of the ERA were not known with certainty, and its symbolism was often touted as amongst its most important functions (Mansbridge 1986). The ERA was one of the most salient and visible issues of the 1970s, with GSS data from the late 1970s/early 1980s revealing that 88.4% of individuals had heard of the ERA and 82.2%

understood what it was. While the attempt at adding a Federal Equal Rights Amendment to the U.S. Constitution eventually failed, roughly half of U.S. states eventually managed to successfully pass state-level Equal Rights Amendments by ballot initiative.

I leverage the staggered introduction of these state ERAs using a difference-in-differences strategy to identify the effect of a law declaring men and women equal on views about whether men and women are indeed equal – and a variety of other related outcome variables. Using individual-level survey data from the American National Election Study (ANES), I find evidence of a polarization effect, whereby women in states that pass an ERA become marginally more likely to believe in women's equality but men instead react by becoming sharply and significantly less likely to believe in said equality. The two key threats to identification in this setting – migration and policy endogeneity – are unlikely to play a major role given the sign of the effect, as they would entail men who oppose male/female equality moving disproportionately to ERA states and states on a more socially-conservative trajectory being more likely to adopt the ERA, a socially-liberal law. Still, in order to deal with any potential endogeneity, I perform a variety of robustness checks. In particular, I focus on individuals in border counties: comparing the evolution of views on female equality along one side of the border between two states to those along the other side of the border, before and after one of those two states introduces an ERA. I run specifications including state-specific time trends. I conduct permutation tests and a wild bootstrap-t procedure as alternative robust methods of generating standard errors within-sample. I restrict the sample to the closest ERA referenda. And I present evidence from dynamic difference-in-differences specifications that pre-trends are non-existent and the effects do not fade out over time.

In addition to the primary result of backlash, I also find considerable evidence in support of other testable implications of the model. Backlash occurs on both sides of the political spectrum. Persistence of backlash is stronger in ideologically-homogeneous communities. Laws are found to play a unique role in generating backlash; more bottom-up components of the women's movement – such as female entry into the labor force, which I study using a shift-share design, and female election to political office, which I study using a close-election RD design – do not

generate backlash. Furthermore, consistent with the version of the model focusing on intergenerational transmission of ideology between parents and children, backlash is significantly stronger amongst men with children, and backlash is successfully passed on to the next generation, albeit with reduced intensity.

Next, I provide evidence against alternative mechanisms. First, I provide evidence – using data on second-order beliefs – that the backlash does not merely represent a re-definition of what gender equality is understood to mean by survey respondents. Second, I show that the backlash is not a consequence of the campaign leading up to the law but rather a consequence of the law itself. Third, I find no evidence that persuasion effects – with ERA opponents ramping up their efforts to convince people – are responsible for the backlash, nor do I find any evidence that the media more broadly contributed to the backlash; if anything, it appears to have mitigated it. Fourth, I discuss why an explanation hinging on policy mood – whereby liberal laws may simply tend to be passed shortly prior to conservative shifts in public-opinion – is inconsistent with the results. Fifth, I find no evidence that the backlash is the result of fears on the part of men about increased labor-market competition from women. Sixth, I find evidence against the hypothesis that the backlash merely represents (potentially-irrational) anger at government on the part of those who disagreed with the ERA. Seventh and last, I provide evidence as to why a desire to merely influence the law – without any role for transmitting one's ideological preferences to one's peers or children – is unlikely to be responsible for the backlash.

Finally, I show that backlash is not merely an idiosyncratic consequence of the Equal Rights Amendments. Using survey data from the ANES, the GSS, and Gallup, I present evidence from dynamic difference-in-differences regressions that virtually every major social policy law of the past half-century has induced sharp and significant backlash with no pre-trends. The Civil Rights Acts of the 1960s, the legalization of abortion in the 1970s, the relaxation of gun control beginning in the 1980s, the Defense-of-Marriage Acts of the 1990s, the legalization of marijuana beginning in the 2000s, the legalization of gay marriage in the 2010s, and more – across various categories of social policy and across the ideological spectrum, backlash has time and time again been the consequence. These findings suggest that an important trade-off exists between the

direct, functional consequences of a law and the backlash it induces amongst the public. More succinctly, aggressive pushes for social change through legislation may face a significant cost in terms of countervailing cultural backlash.

2 Literature Review

My work builds on and contributes to a number of related literatures within political economy and public economics. There has been a growing effort in recent years to understand the interplay between institutions and culture. A large body of work that dates back to the foundation of cultural economics studies the effects of culture on insitutions. Alesina and Giuliano (2015) extensively summarize this literature in a survey paper. The converse relationship – the effects of institutions on culture – received less attention at first but has been the subject of a growing literature in recent years.

The theoretical literature on the expressive role of the law and its effect on cultural norms and attitudes began in legal journals, seeded by the seminal work of Sunstein (1996). Kahan (1997), Cooter (1998), and Posner (1998, 2000) followed shortly thereafter. Within economics, much of the theoretical literature on the effects of law/institutions on culture relates heavily to the broader literature on cultural transmission. Bisin and Verdier (2001) model the dynamics of cultural transmission, finding that families which perceive their cultural traits to be in the minority double-down on said traits in order to inculcate their children with them and ensure the traits persist. Tabellini (2008) models how enforcement of laws and the broader legal framework contribute to the choice of which values parents attempt to instill in their offspring and consequently the level of cooperation in society, finding the existence of a rich two-way interplay between values and institutions. Greif and Tadelis (2010) model the evolution and persistence of "crypto-morality" – situations prevalent in history wherein families adhere secretly to one morality while openly practicing another in an attempt to thwart institutional pressure for change.

The theoretical literature on the effects of institutions on culture is not limited solely to studies of cultural persistence, however. Benabou and Tirole (2011) model the interplay

between laws and norms, arguing that laws both impose material incentives and signal a society's values/norms – and that optimal incentive-setting can differ in the presence of social norms, with laws crowding-out and undermining social norms in certain cases. Acemoglu and Jackson (2017) also model the interplay between social norms and the enforcement of laws, finding amongst other things that more restrictive laws can reduce the incidence of law-breaking behavior amongst individuals who are primarily law-abiding while increasing the incidence of law-breaking amongst individuals who are primarily law-breaking. Departing slightly from the relationship between legal institutions and social preferences, Bowles and Polania-Reyes (2012) survey the related (broader) literature on the relationship between economic incentives and social preferences, finding that crowding-out of social preferences by economic incentives appears to be more common than crowding-in.

My model builds on – and owes much to – the aforementioned approaches. It also owes homage to the very broad public choice literature generally and the median voter theorem specifically in its setup of a spectrum of ideologically-coded choices faced by each agent. This literature is far too broad to review in great detail but was seeded by Black (1948) and Downs (1957). The work of Acemoglu and Robinson (2008) on the substitutability of de facto and de jure power – with reductions in de jure power of a group being ameliorated by increased investments in de facto power – is also highly relevant.

There also exists an empirical literature on the effects of institutions on culture, beliefs, and norms, chiefly focused on the very long run. An early example is Shiller et al. (1992), which focuses on the former communist-led states of Eastern Europe. Using cross-country survey data, Shiller et al. find little evidence of a so-called Homo Sovieticus unmotivated to work and innovate. Also using cross-country survey data in the post-communist context, Roland (2012) observes that, in most dimensions, attitudes about the role of government and the role of markets in transition economies is not converging with those in Western market economies, potential evidence that these preferences come from much longer-run historical factors than the communist experience. Alesina and Fuchs-Schündeln (2007) take their analysis beyond cross-country correlations and look within Germany, focusing in particular on the treatment effect of

the East German communist-led system on East Germans. They find that East Germans remain more interventionist and pro-government than West Germans but that the former appear to be converging to the West German norm.

Becker et al. (2016) exploits a regression discontinuity to examine the effects of institutions on beliefs, looking on either side of what was once the Habsburg (Austrian) Empire border. The Habsburg Empire was marked by a characteristically well-functioning bureaucracy, and Becker et al. explore whether this institutional characteristic induced a persistent increase in trust toward government, of which they find some evidence. With a narrower bandwith of 25 kilometers, Peisakhin (2010) surveyed 1675 people living in villages on either side of the former Habsburg-Russian border, finding large and statistically-significant differences in terms of various cultural outcome variables between the two groups. Lowes et al. (2017) study the persistent effects of the institutions of the highly centralized Kuba Kingdom of Central Africa on modern rule-following, finding evidence that the legacy of the Kuba Kingdom is actually that of reduced rule-following and increased cheating – potentially indicative of substitutability between formal institutions and informal culture/social norms.

A subset of this literature uses lab or field experiments to induce variation. Tyran and Feld (2006), Sutter, Haigner, and Kocher (2010), and Dal Bó, Foster, and Putterman (2010) explore the effect of democratic rules on behavior, the latter finding that cooperation is greater under the same rule when that rule is chosen democratically versus when it is assigned exogenously by a computer. Bursztyn, Egorov, and Fiorin (2017) run an experiment on Amazon mTurk finding that exogenous increases in participants' perceptions of Donald Trump's popularity make individuals more likely to exhibit anti-immigration views and behavior.

Fewer papers examine specific laws or examine a short/medium-run setting wherein the dynamics of change in attitudes, beliefs, or norms can be studied at a higher frequency. Gruber and Hungerman (2008), studying the repeal of the Blue Laws in the United States, is an early exception. Recent examples are Fouka (2020), who studies the German-American forced assimilation laws passed in two U.S. states in the early 1900s, and Abdelgadir and Fouka (2020), who study the 2004 French hijab ban – both of which are found to induce backlash. This

backlash, however, is of a somewhat different form than the kind I study. It concerns how groups *targeted* by a social policy law respond to that law, whereas I look beyond this realm and study how the non-targeted majority group responds as well. Ang (2019), who studies the specific case of the 1975 revision to the Voting Rights Act and finds evidence of backlash amongst the white majority, is perhaps the study which relates most closely to mine.

By studying individual laws in a short, medium, and long-run setting where the dynamics and pre-periods of legal change are clearly observable, I am able to tightly relate my empirical results to the theoretical research on the effects of laws on attitudes and norms generally – and to my model in particular. In so doing, I hope to tie together the theoretical and empirical literatures on the effects of institutions on culture, attitudes, and norms. And by extending my empirical analysis to cover the major U.S. social policy laws of the past half-century, I hope to make a substantial contribution to the literature on backlash and reveal that backlash is, in fact, a remarkably general phenomenon occurring across the spectrum of laws.

3 Model

3.1 Baseline Model

Consider a setting where, in each period t, society is populated by N individuals. Each individual has some most-preferred point $b_{i,t}$ (i.e., a bliss point), along the real line $(-\infty, \infty)$, which corresponds to the left/right political spectrum on a given issue. In other words, some people may be left-wing and some may be right-wing, and amongst left- and right-wing individuals, some may be more extreme than others. Each individual in period t takes an action, $x_{i,t}$, along the left/right spectrum. Individuals prefer to take actions as consistent as possible with their ideological bliss points. Actions may represent virtually anything ideologically-coded. For example, an individual who favors traditional gender roles will want to make statements supporting traditional gender roles, vote for the party that is more likely to ensure traditionalism in gender roles, pursue personal relationships and division-of-labor between spouses reflecting traditional gender roles, etc.

Furthermore, individuals have preferences not only over their actions but also over the

ideology of other members of society beyond themselves. They want broader society to be headed in a direction similar to their own ideological preferences. In particular, liberals prefer society to be moving in a liberal direction; conservatives prefer society to be moving in a conservative direction. These preferences can be implemented with the following utility function, $u_{i,t}$,

$$u_{i,t}(x_{i,t}) = -(x_{i,t} - b_{i,t})^2 - \sum_j \alpha_j (b_{j,t+1} - b_{i,t})^2 ,$$

where α_j denotes the importance placed on influencing the ideology of individual j relative to taking actions close to one's own preferences.

While individuals have direct control over their own actions, their control over the ideological preferences of other individuals is indirect. They are able to influence people through their own actions, but other forces also exert influence. An individual's beliefs are influenced by his/her own previously-held beliefs, by the actions they have observed other members of society taking, and by the law. In other words, ideological preferences are formed according to

$$b_{i,t+1} = \gamma_b b_{i,t} + \gamma_x \bar{x}_t + \gamma_L L ,$$

where γ_b denotes the influence of one's own previously-held ideology in determining one's future ideology, γ_x denotes the influence of society, and γ_L denotes the influence of the law. $\bar{x}_t \equiv \sum_{j=1}^{N} x_{j,t}$ denotes the average ideological position of actions taken across individuals in society.

Proposition 1: Provided $0 < \gamma_x, \gamma_L < 1$ and $\exists j$ for which $\alpha_j > 0$, the optimal action of individuals moves inversely with the law: $\partial x_{i,t}^* / \partial L < 0$. That is, backlash occurs against the law.

The proof of Proposition 1 (and other propositions in this section) is provided in Appendix A.1. It reveals that individuals will find it optimal to move in the *opposite* ideological direction of the law. To provide an intuitive example, suppose the law moves in a more liberal direction. Conservative individuals, wanting to counteract the influence of the law and tilt society back in a

more conservative direction, will find it optimal to double down on their conservatism.

Corollary 1: Backlash occurs throughout the political spectrum.

Proposition 1 revealed that $\partial x_{i,t}^{*} / \partial L < 0$ regardless of the specific values of any $b_{i,t}$. Backlash occurs on both sides of the political spectrum – though the intuition is subtly different for each side. As noted, if the law switches from a conservative policy to a liberal policy, conservatives will find it optimal to move rightward, doubling down to counteract the influence of the law. Conversely, liberals no longer have to take actions more liberal than their underlying preferences to counteract the influence of the law, as the law is now in line with their preferences. Thus they can relax somewhat and stop doubling-down; they too can move rightward. Proposition 1 and Corollary 1 yield some initial testable implications of the model.

3.2 Specific Cases and Numerical Examples

It is instructive to consider a few specific cases of the model. It is not a stretch of the imagination to think that individuals would focus specific attention on the ideological preferences of their own children and the factors that shape said preferences. I inserted questions related to this matter in a recent nationally-representative survey run by survey panel company Pure Profile. In one question, respondents were asked, "Suppose your state passed a social policy law. Do you believe that this would influence your children's beliefs and attitudes later in life?" 40% responded yes, and only 12% responded no – with the remainder saying maybe or not sure. In another question, respondents were asked, "If your state passed a social policy law that you disagreed with, which of the following actions would you be likely to take?" offering several options including stating their opposition to the law to family/friends, voting against the law, and writing to a congressperson. 48% of respondents said that they would explain to their children why they opposed the law. A smaller fraction – 38% - said that they would explain to their friends why they opposed the law. These data suggest that individuals do generally think that law has the ability to influence children and that they would plan respond accordingly.

Consequently, consider the simplified case where $\alpha_i = \alpha > 0$, $\alpha_j = 0\ \forall j \neq i$, $\gamma_b = 0$, and $N = 1$. This case allows for a somewhat different interpretation of the model. Here, each period t can be interpreted as a generation. Individuals care about taking actions close to their own ideological bliss point and also about inculcating their children with ideological preferences close to their own. Children's ideological preferences are shaped by parental actions and by the law.

$$u_{i,t}(x_{i,t}) = -(x_{i,t} - b_{i,t})^2 - \alpha(b_{i,t+1} - b_{i,t})^2$$

$$b_{i,t+1} = \gamma x_{i,t} + (1-\gamma)L$$

Proposition 2: Provided $0 < \gamma < 1$ and α is sufficiently large, backlash will persist beyond the initial generation and be successfully passed down to children. That is, $\partial x^*_{i,t+1} / \partial L < 0$.

Proposition 2 yields another testable implication. If, in fact, individuals are motivated by the desire to inculcate their children with ideology similar to their own, it should be the case that they (at least partially) succeed. Despite the law also exerting its influence, the backlash is passed down to the next generation.

To help visualize these concepts, Figure 2 displays a few specific cases. It shows what happens to actions over the course of generations for a family with an initial bliss point of $b_{i,0}$ = 50 when the law is initially at L = 50 as well but changes in generation 5 to L = 0 (i.e., the law moves to the left). The top-left panel varies α but holding other parameters fixed. As can be seen, backlash is the result – the family moves its actions in a more right-wing direction. The strength and persistence of this backlash varies in α, the extent to which families care about the actions of the next generation. In extreme case in which families do not care at all about the actions of the next generation (α = 0), backlash is non-existent. In the other extreme case in which families care infinitely more about the actions of the next generation relative to their own actions ($\alpha \rightarrow \infty$), backlash is extreme and completely persistent – actions remain permanently more right-wing as a result of the law moving to the left. In all intermediate cases, there is an initial backlash which is weakened over time as future generations converge to the law.

The top-right panel of Figure 2 instead varies γ while holding other parameters fixed. In the two extreme cases – $\gamma = 0$ and $\gamma = 1$ – there is no backlash whatsoever. This is because in the former case parents exert no influence on their children and consequently gain no utility from backlash. In the latter case, parents have total influence over their children and consequently need not backlash in order to pass their preferences onto them unfettered. For intermediate values, the law and parents both have some influence over their children and, consequently, the incentive for backlash exists.

The bottom-left panel of Figure 2 varies the ideological position of the new law while holding other parameters fixed. Here we see that backlash is stronger the more distant the new law is from the family's initial ideological preferences. Intuitively, a more distant law will require even more force to push back against successfully and prevent children from rapidly moving away from the family's preferences – consequently families find it optimal to push even further in terms of their backlash.

$$u_{i,t}(x_{i,t}) = -(x_{i,t} - b_{i,t})^2 - \alpha(b_{i,t+1} - b_{i,t})^2$$

$$b_{i,t} = \gamma_P x_{i,t} + \gamma_N \bar{x}_t^{j \neq i} + (1-\gamma)L$$

Another specific case of the model – mid-way between the the general case and the preceding simplified case – preserves the generational focus on children but allows for children to be influenced by others in society, not merely their parents. In this case, $\alpha_i = \alpha > 0$, $\alpha_j = 0 \; \forall j \neq i$, $\gamma_b = 0$, and N large. I also allow for parents to potentially exert a different level of influence, γ_P, than other members of society, γ_N, on the ideological preferences of their children.

Proposition 3: Consider two different societies with the same law, L. One is homogeneous, with all individuals sharing identical ideological preferences, $b_{i,t} = L + b$. The other is heterogeneous, with half sharing ideology $b_{i,t} = L + b$ and the other half sharing an opposing ideological preference $b_{j,t} = L - b$. Then, for each family i, $|x_{i,t+k}^{het,*} - L| < |x_{i,t+k}^{hom,*} - L|$ for sufficiently high k. That is, actions will converge more rapidly to the law in the heterogeneous society.

In other words, Proposition 3 says that the homogeneous society will be more successful at

preserving its ideology than the heterogeneous society. The backlash will persist longer in an ideologically homogeneous society. The bottom-right panel of Figure 2 varies the ideological makeup of the community while holding other parameters fixed; as can be seen, either a community more liberal on average or one more conservative on average than the individual of interest will undermine that individual's abilities to preserve his/her ideological preferences down through the generations. This highlights a subtle but interesting relationship that has much in common with the broad literature on the consequences of ethnic fractionalization (see, for example, Alesina, Baqir, and Easterly 1999 and Alesina and La Ferrara 2005), which is generally found to reduce social capital and reduce a community's ability to organize public goods provision. Here it is *ideological* fractionalization that contributes to a community's inability to retain its values in the face of institutional pressure. Division within the community means that left-wing and right-wing parents are undermining – rather than reinforcing – each other, meaning that the law has relatively more influence than the old norms in heterogeneous communities and consequently parents in these communities have little ability to transmit their preferences onward to future generations.

In Appendix A, I solve additional extensions to the model which endogenize passage of laws. In Appendix A.2, I endogenize the law by allowing individuals to vote on the law that will be in place in the next period. Given that systematic backlash results from laws, one might wonder whether any laws would actually be passed in equilibrium in the framework of this model. I show that, as long as people are sufficiently forward-looking, they are willing to pass laws and endure the short-/medium-run backlash in order to shift society toward the law in the long-run. In Appendix A.3, I endogenize the law in a different manner – allowing for backlash in the present period to affect laws in the subsequent period. I show that this provides only a limited additional inducement to backlash.

4 Empirical Framework

Does backlash actually exist in practice? In order to test the implications of the model, I first focus on one social policy law in detail: the Equal Rights Amendment (ERA) of the 1970s,

which aimed to guarantee equal rights to American citizens regardless of sex and was added to many state constitutions in that era. I examine this law in depth and study a variety of outcomes – attitudes that people express toward male/female equality, voting patterns, labor-market outcomes, the contours of and roles within marital relationships, etc. Then, to show that the ERA is not unique in generating backlash, I broaden the horizon to virtually every major social policy law of the past half-century for which state-level variation exists, studying the attitudinal outcomes corresponding to those laws. For example, with regard to the legalization of abortion, I study the attitudes people express toward abortion; with regard to gun control, I study the attitudes people express toward gun control; etc.

4.1 Data

I draw on survey data from the American National Election Studies (ANES), the General Social Survey (GSS), and Gallup Poll. Since its inception in 1948, the ANES has asked a random sample of Americans questions about political affiliation and intended voting patterns (virtually) every other year. Since the 1960s, the ANES has asked respondents to provide their "feeling thermometer" toward a wide range of groups (various ethnicities, various political groups, etc.) along with a broad array of other questions on political-economic matters. The ANES is publicly available at the individual level, and the restricted-access version contains state and county codes for each respondent from 1952 to the present.

The GSS asks a similarly-broad swathe of socio-political questions and has been running since 1972 – annually from 1972-1994 and every other year since then. It, too, is publicly-available at the individual level, and the restricted-access version contains state codes since 1973 and county codes since 1994. Many questions in the ANES and the GSS have been repeated without modification for decades, allowing for a consistent view of the evolution of public attitudes and positions. Gallup Poll, too, has asked a battery of socio-political questions since the 1930s. Unlike the ANES and the GSS, Gallup is less focused on academic research and hasn't always asked its questions repeatedly and in consistent time intervals, but some popular questions have been asked frequently and fairly consistently, and some of these pre-date the

ANES and the GSS, allowing for analysis of specific law changes not possible with the other two datasets.

With regard to my leading example, the ERA, the ANES has asked a question on equality of the sexes since 1972. Individuals are asked to rate, on a seven-point Likert scale, whether their attitude is closer to "Men and women are fundamentally equal" (1) or "A woman's place is the home" (7). I code a response of 1, 2, or 3 as indicating a positive attitude toward equality and also run regressions on a continuous outcome variable generated by converting this scale into a z-score. While the General Social Survey (GSS) also asks a few questions on views of women's roles, it falls short relative to the ANES in this particular context for two reasons. First, the GSS did not begin collecting county codes until the 1990s, long after all of the identifying variation of the 1970s had come and gone. This makes border-county regression specifications impossible. Second, the GSS did not even record state codes in its very first wave (1972) and only asked the questions about women's roles every other wave during those early years. As such, the first GSS wave for which both (i) the questions of interest are present and (ii) state codes are available was 1974. Because of the substantial number of state ERAs passed between late 1972 and late 1974, several crucial years of data are wiped out, reducing by eight the number of states that can be used for identification. Both of these reasons are the key impetus behind choosing the ANES over the GSS.

I additionally obtain data on voting returns from Dave Leip's Election Atlas, data on fertility patterns from the National Fertility Survey (NFS), and data on employment by sector from the publicly-available Decennial Census tables.

Gladstone (2004) lists the states that adopted ERAs and the years in which they were adopted. This information can be used to create a panel dataset indicating whether or not a given state has an ERA in effect in a given year – and the number of years it has already been effective. Such a panel can then be readily merged with the other data sources, yielding a panel dataset containing the ERA indicator, demographic characteristics, and all the outcome variables of interest.

4.2 Regression Strategy

As noted, the ANES, GSS, and Gallup survey data disclose the state of residence of each respondent. Many laws – including most social policy laws – vary sharply at the state level in the United States of America and have been changed over time in a staggered fashion. This allows analysis of outcomes in states where a given law is passed versus states where the law is not passed. To this end, a static state-level difference-in-differences regression approach can be taken.

$$Y_{ijt} = \alpha + \beta \cdot Law_{jt} + \gamma_j + \eta_t + \varepsilon_{ijt},$$

where Y_{ijt} denotes the value of outcome variable Y (say, attitudes about male/female equality) of person i in state j during year t, Law_{jt} is an indicator variable denoting whether the law in question was in effect in state j during year t, γ_j denotes state fixed-effects, and η_t denotes year fixed-effects. As the key right-hand-side variable of interest, Law_{jt} indicates whether an individual is in the treatment (1) or control (0) group. Regressions are weighted with the survey weights included in the corresponding dataset. Following Bertrand, Duflo, and Mullainathan, standard errors are clustered at the state level – the level at which treatment is assigned. Note that this yields nearly 50 clusters. While Cameron, Gelbach, and Miller (2008) and Cameron and Miller (2015) have raised concerns about finite-sample, few-cluster inference, they also show that by 50 clusters, these concerns have largely dissipated.

The identification assumption for a standard state-level difference-in-differences specification such as this one is that of parallel trends: the outcome variable of interest would have evolved analogously in the treatment and control if, counterfactually, the treatment group had not received treatment. For example, in the case of the state Equal Rights Amendments, this assumption is that attitudes expressed toward male/female equality in ERA states would have evolved similarly to non-ERA states if the ERAs had not been passed.

There are two key issues with this assumption: migration and policy endogeneity. The migration issue is that, since the ANES data is not longitudinal at the individual level, it could plausibly be the case that individuals are sorting into the states that have the policy they like. As will be seen, this ends up being a non-issue due to the sign of the effects I find. That is, since a

negative effect (backlash) is found, any such sorting would only serve to bias the effect toward zero, making the effect I measure in this static specification an *underestimate* of the true backlash. The policy endogeneity issue is that passage of state laws is not randomly-assigned; hence the states which chose to adopt a given law were plausibly on a different political path than those which chose not to adopt the law. Again, the sign of the effect revealed by the regressions will render this a questionable concern as well, unless one believes that states on a more conservative trajectory are more likely to adopt *liberal* laws (and vice versa).

Still, as one way of dealing with the concern of policy endogeneity, I restrict the sample to counties on either side of a border between an (eventual) law-implementing state and a non-law-implementing state and re-run an adapted version of the above specification:

$$Y_{ijkt} = \alpha + \beta \cdot Law_{jt} + \gamma_{jk} + \eta_t + \varepsilon_{ijkt},$$

where Y_{ijt} denotes the value of outcome variable Y of person i in state j along border k during year t and γ_{jk} denotes state-by-border fixed effects. So, for example, a different fixed effect is included for the counties along the western side of the Louisiana/Mississippi border versus those along its eastern side, both of which are different from each of the two fixed effects for either side of the Louisiana/Arkansas border. The idea is that, while a state that passes a certain law may plausibly be on a different political trajectory than a state which does not pass that law, communities just along the border of a state are likely to be much more similar – and evolve much more similarly – to the communities right on the other side of that border. And, insofar as they do differ in terms of levels, this will be captured by the highly versatile fixed-effects anyway. In short, the parallel-trends assumption is plausibly more likely to hold in the border-county setting.

Another way I deal with potential concerns of policy endogeneity is by running dynamic difference-in-differences specifications – sometimes referred to as event-study specifications – with pre- periods, as follows:

$$Y_{ijt} = \alpha + \sum_{m=A}^{B} \beta_m \cdot Law_{jt}^{m} + \gamma_j + \eta_t + \varepsilon_{ijt}$$

where Law_{jt}^{m} is an indicator variable denoting whether the law in question was in its m^{th} or (m +

1)th year in effect in state *j* during year *t*. For example, the Connecticut state ERA took effect in 1974. Thus 1975 is its second year in effect, 1976 its third year in effect, etc. The m^{th} and (m + 1)th years are grouped because some states pass an ERA in an even-numbered year and some states pass one in an odd-numbered year, whereas the ANES (and, recently, the GSS) is collected only every other year.[3] For all dynamic specifications, I set $A < 0$ in order to test for the existence of pre-trends and thereby provide evidence supporting the lack of policy endogeneity, the existence of parallel trends, and the overall cleanliness of the natural experiment. B denotes the point beyond which remaining periods are pooled. For example, if $B = 10$, ERA effects beyond 10 years after ERA passage are all pooled into one coefficient for compactness. This dynamic specification also responds to the concerns raised recently in the applied econometrics literature – such as in Borusyak and Jaravel (2017) – that running static specifications over long time horizons over which treatment effects may plausibly be heterogeneous can bias the static regression coefficient.

In order to test the implications of the model and further investigate the mechanism, I run a multitude of specifications wherein I study the heterogeneity of the law's effects across various categories of individuals or communities. These specifications take the form of the above regressions, but with an interaction term between the right-hand-side law variable and the heterogeneity variable of interest. For instance, in the case case of the static specification,

$$Y_{ijt} = \alpha + \beta_1 Law_{jt} + \beta_2 Heterogeneity_{ijt} + \beta_3 \cdot (Law_{jt} * Heterogeneity_{ijt}) + \gamma_j + \eta_t + \varepsilon_{ijt},$$

where $Heterogeneity_{ijt}$ is the heterogeneity variable of interest and, consequently, β_3 is the coefficient revealing heterogeneity (or lack thereof) of the law on the heterogeneity variable. For example, if the heterogeneity variable is income, β_3 provides evidence on the extent to which the law in question has a differential effect on high-income versus low-income individuals.

As noted above, while the number of clusters is near 50 for most of the state-level specifications, certain specifications – in particular, the border-county specifications – result in closer to 25 clusters. While simulations performed by Cameron and Miller (2015) suggest that

[3] Consequently, if the *Law* indicators only referred to one specific year *m*, the treatment group over which the coefficients are estimated would be inconsistent over time. For odd-numbered *m*, the treatment group would be composed solely of states which passed the ERA in an odd-numbered year; for even-numbered *m*, the treatment group would be composed solely of states which passed the ERA in an even-numbered year.

this too is basically high enough to avoid the statistical concerns associated with having too few clusters, to be safe, I alternatively compute p-values using the Wild Bootstrap-t procedure with 2000 repetitions that they propose in order to ensure that the results are robust. For an even further and more transparent robustness check, I compute p-values in-sample by running straightforward permutation tests (i) randomizing both the treatment states and each state's treatment year and, more strictly, (ii) fixing the treatment states but randomizing each state's treatment year for further assurance of robustness.

5 The State Equal Rights Amendments

5.1 Political Economic Context

The idea of an Equal Rights Amendment to the U.S. Constitution was a hotly-debated issue for over six decades, from the 1920s through the 1980s. The amendment sought to end all legal distinctions between men and women in terms of divorce, property, employment, and all other matters. A proposed Equal Rights Amendment was introduced in every session of Congress from 1921 to 1972, failing to secure passage every single time until the last. By the 1970s, individual laws increasingly existed codifying equal treatment in various dimensions, but advocates of the ERA pointed out that they could be overturned by subsequent laws or Supreme Court decisions, whereas an Amendment would have more permanence and be immune to changing composition of the Supreme Court. Perhaps most importantly, the symbolism of the ERA – declaring to society that not only were all men created equal, all women were as well – was viewed as paramount in itself (Mansbridge 1986).[4]

The debate over the Equal Rights Amendment was very public and very salient; it was one of the most major policy debates of the 1970s. Books and documentaries about the 1970s almost invariably include a chapter or episode on the ERA (e.g., Perlstein 2014, Lepore 2018, CNN 2017). Candidates for office were routinely asked for their views on the ERA with greater

[4] Mansbridge, herself an ERA advocate, wrote "One of the most important indirect effects might have been the effect on the public. … To the degree that having an ERA in the Constitution would remind Americans that equality for women ought to be an important goal in their everyday lives, and to the degree that increased commitment to this value would result in changed behavior on practical issues like who takes care of children, the ERA might have reached beyond the law to the social and economic patterns that produced most of the 59-cents [wage] gap" (pp. 43).

frequency than almost any other issue of the day. In terms of concrete data, in two waves of the General Social Survey in the late 1970s and early 1980s respondents were asked whether they had heard of the ERA; 88.4% of respondents answered affirmatively. A follow-up question explored whether individuals understood what the ERA meant; an impressive 82.2% did.

While the question of an ERA was a very contentious one indeed, the coalitions that emerged in support and opposition were not formed along strict and predictable partisan lines. The Republican Party included support for the ERA in its platform beginning in 1940, renewing said support at every Republican National Convention through 1976. The Democratic Party followed along beginning in 1944 at that year's Democratic National Convention, renewing this plank every four years through 1984. There were those in both parties who remained skeptical, however, and only in the early 1970s after a strong push by Michigan Democratic congresswoman Martha Griffiths did an Equal Rights Amendment pass both the House of Representatives and the Senate, whereupon it was immediately endorsed and signed by Republican President Richard Nixon in March of 1972. Unfortunately for its supporters, however, due to the constitutional requirement that all amendments be ratified by three-quarters (38) of the 50 state legislatures within 7 years, the Equal Rights Amendment never became law. Despite a three-year extension signed into law by President Jimmy Carter in 1978, the federal ERA fell short by three states.

Opposition to the ERA, rather than splitting cleanly along Democratic/Republican lines, split more along liberal/conservative lines – in an era where there were still large numbers of liberal Republicans and conservative Democrats. Furthermore, it created faultlines between upper-middle-class elites and the working-class populace. Opposition was led by Phyllis Schlafly, who established the STOP ERA coalition after the passage of a state ERA in her home state of Illinois. Schlafly argued passionately that the ERA would directly ameliorate the special protections and privileges women were given in modern American society – and indirectly by undermining the family unit (Schlafly 1972). The ERA, she claimed, threatened to make the American woman a partner expected to support herself financially, due nothing from her husband, even in case of divorce – and it would also be another set of words for the Supreme

Court to work with in an era of repeated liberal Supreme Court decisions. Gay marriage, gender-neutral bathrooms, government support for abortion, military drafting of women, and much more would be likely consequences of the ERA, according to Schlafly. Her ideas gathered much support amongst conservatives, and her advocacy is often regarded as a primary factor in the federal ERA's defeat (Mansbridge 1986). Her successful opposition has even been dramatized in the recent Hulu series *Mrs. America* (2020).

That said, through a distinct yet parallel process, Equal Rights Amendments to the constitutions of 20 states had been ratified by the end of the 1980s – with several more approved and ratified decades later. It is these state ERAs passed in the 1970s and 1980s that I utilize for variation. Table 1 lists the state ERAs and their years of passage; Figure 1 displays the states with an ERA on a map of the U.S.

For a number of reasons, the ERA constitutes a desirable natural experiment for studying the effects of laws on attitudes held by the public. One of the reasons is precisely the aforementioned high degree of salience; the ERA was on the mind of the public as an important issue with big implications. Furthermore, because the ERA was initially endorsed by both political parties, the pattern of ERA-adopting states differs from the usual red/blue divide typical of most other laws – and virtually all other social policy laws. There are plenty of states of every political variety and every region within the United States which adopted (and didn't adopt) the ERA. And unlike many laws, the state ERAs were not passed by legislative action but rather by referenda, which allows one to cleanly isolate the effect of the law itself from the campaign leading up to the law. While unanticipated judicially-induced laws (such as the legalization of abortion by *Roe v. Wade*) would avoid entanglement of a campaign effect with a law effect, precisely because these laws came as surprises there was limited public opinion survey data in their pre-period, heavily constraining the statistical techniques and robustness checks one can apply. While I do eventually broaden my focus to study many more social policy laws, these factors render the ERA a natural leading example.

5.2 Main Results

The results of the static specifications discussed in section 4 are displayed in Table 2. The outcome variable in the table is an indicator for whether an individual expresses their attitude as a 1, 2, or 3 on the 7-point male/female equality [1] to inequality [7] scale – i.e., an indicator for positive attitudes toward male/female equality. This results in coefficients that are clean and easy-to-interpret: the percentage-point change in the share of individuals whose position is that men and women are closer to equal than unequal. As column (1) shows, there is an overall backlash effect when both men and women are pooled together in the regression. That is, there is a decrease by 5.8 percentage points in the share of people who believe in gender equality. Columns (2) and (3) make clear the existence of heterogeneous treatment effects: whereas introduction of a state Equal Rights Amendment marginally (but not significantly) increases the proportion of women who believe that men and women are indeed equal, it instead spurs a reaction by men – a *decrease* by approximately 15 percentage points in the share of men who believe in equality of women. Columns (1) through (3) use ANES data from 1972 to 1998 since this corresponds to the first ANES wave in which the aforementioned survey question was asked through the last wave for which ANES county geocodes are publicly-available[5]. Columns (4) and (5) show that if the end date is instead 1988 (the wave after the final state ERA in my sample is passed) or 2008 (the final year the aforementioned survey question is asked), the result is highly similar in magnitude and significance. Columns (6) and (7) turn to the border-county specification. The backlash effect on the part of males endures with no substantive change in significance.

Figure 3 displays the results of permutation tests run on the main state-level specification for male attitudes (i.e., the specification in column (2) of Table 2). These permutation tests form p-values within-sample rather than relying on standard errors computed from econometric theory to ensure that the results are robust. In particular, the left panel fixes the number of states that adopt ERAs but randomizes *which* states adopt them and randomizes the year in which each state adopts an ERA (by re-assigning the actual treatment years randomly across the placebo states). The right panel fixes specific states which were actually treated with an ERA but randomizes the

[5] ANES county geocodes for years after 1998 are available by application in their restricted-access dataset.

year in which each state adopted the ERA (again, by re-assigning the actual treatment years randomly across the states). There are minimal differences between the two permutation tests; both yield p-values of less than 0.0005, indicating that the results remain very strong. I also run the former test on the other specifications in Table 2, wherein the resulting p-values are reported for each. Also reported are p-values resulting from a Wild Bootstrap-t procedure with 2000 repetitions as another method of generating p-values within-sample, a suggestion of Cameron, Gelbach, and Miller (2008). The results are again robust to this technique.

Figure 4 displays the dynamic difference-in-differences specification with male attitudes toward equality as the outcome. As can be seen, pre-trends do not exist, and the effect is sharp, dramatic, and significant in the near aftermath of ERA passage. Indeed, if one extends the horizon as far as the data permits – 40 years – it can be seen that the backlash effect remains strong and persistent decades later; there is no evidence of fade-out or re-convergence. Figure B-1 presents this longer-horizon dynamic difference-in-differences. Figure B-2 shows the dynamics for female attitudes, which exhibit substantial pre-trends and no significant change on impact. This can be taken as further evidence that, if anything, ERA-adopting states were on a more liberal trajectory rather than a more conservative one.

In Table 3, I explore the effect of the state ERAs on voting patterns. Columns (1) through (4) use ANES data and columns (5) and (6) validate these results with official election returns data from Dave Leip's Election Atlas. The result is clear: ERA passage induces a sharp and statistically-significant swing in vote shares toward the Republican Party in the neighborhood of 5-9% – mostly consistent in both the ANES and official returns data. This is also consistent with historical anecdotes that the Republican party, as it moved in a more socially-conservative direction in the late 1970s, harnessed the ERA backlash effectively – Phyllis Schlafly, the architect of the STOP ERA coalition, was an important Republican operative and an early supporter of Ronald Reagan in his bid for the presidency. While the swing in vote shares I find is quite large, it should be noted that the margin of the 1980 Presidential Election was even larger: Ronald Reagan defeated Jimmy Carter by 9.7% of the national popular vote. Margins were smaller in certain states than others, so if the aforementioned swing was consistent across

states, it would mean that the ERA swung several ERA-adopting states from Carter to Reagan – but fell short of swinging the whole election. Figure 5 shows the dynamics of this effect, which reveals that the effects are largely driven by the 1980s-era Reagan elections, when the differences in social policy between the two parties were at their zenith; there is subsequent re-convergence.

Table 4, Panel 1 shows the effect of the state ERAs on a number of placebo outcomes: some of the political questions asked most consistently across 1970s waves of the ANES. No statistically significant effects are found. Table 4, Panel 2 shows the effect of the state ERAs on the various "feeling thermometer" questions asked consistently in the ANES. These questions asked individuals how warmly they felt toward various groups on a scale of 0 to 100. I find a significant effect of ERA passage on only one: feelings toward women's liberation activists, which decline markedly. This provides further evidence of backlash against gender equality.

In Appendix B.1, I further probe these main results. I explore alternative forms of the dependent variable (such as a continuous z-score measure and point-by-point regressions for each of the 7 responses on the 1-to-7 gender equality scale) and conduct robustness checks including the addition of state-specific time trends and the regression approach of Chaisemartin and D'Haultfoeuille (2020). The main result is robust to all of these approaches. In Appendix B.2, I explore some other outcomes, including fertility preferences of men and women and marital happiness. To summarize, I find evidence of more control by men over fertility choices and worsened happiness for married couples – but not for single men and women. Taken as a whole, these findings may suggest backlashing husbands constraining or otherwise chafing against their wives' choices.

5.3 Testing Other Implications of the Model

Plentiful and fairly robust evidence on the main implication of the model – backlash – was provided in the preceding section. However, the model has other, subtler implications which are also testable. Indeed, if these implications are borne out empirically, the fact that some of them are quite subtle and idiosyncratic to this model should greatly strengthen confidence that the

model truly represents the underlying mechanism at work.

First, according to the model, backlash should occur on both sides of the ideological spectrum. As seen in Corollary 1, backlash is not conditional on one's ideological position. As the ANES has asked since the early 1970s whether individuals consider themselves liberals or conservatives (and the intensity of that identification), it is possible to test that implication as well. Columns (1) and (2) of Table 5 reveals that, indeed, both liberals and conservatives exhibit a backlash that does not differ in magnitude. Column (1) uses the ideological self-identification from within the ANES as the interaction variable; column (2) uses 1968 county-level Republican vote share as the interaction variable. The conclusion is the same in both cases.

Second, an obvious implication of the simplified case of the model that focuses on ideological transmission from parents to children is that backlash should be stronger amongst those who have children. While the desire to influence society and its future preferences and priorities more broadly than within the confines of one's own family can also motivate some backlash, as shown in Appendix A.3 – the desire to influence one's own children is a powerful channel on its own, and under reasonable parameter values, should account for a large fraction of the total backlash. The ANES, unfortunately, only began asking whether individuals have children of any age later in the 1970s. Earlier – in 1972 – it asked whether individuals had *school-aged* children (specified as 5-18 in the survey questionnaire).[6] Because the ANES began asking the ages of respondents' individual children in 1978, one can construct an indicator for children aged 5-18 from 1978 onward and use this variable to study whether men with children experience a greater backlash to the ERA. This is imperfect, because some individuals who have children (in particular, children aged under 5 or over 18) will be regarded in the regression as not having children. However, this should only bias downward the extent of the heterogeneity I find. Despite the imperfections, column (3) of Table 5 reveals that, indeed, men with children exhibit a significantly stronger backlash than those without children. Consequently, the desire to transmit one's ideological preferences to one's children appears to be an important part of the

[6] It appears to have asked this question as a flag to determine whether or not the respondent should be asked the immediately following set of questions in the questionnaire, all of which pertain to experiences of parents with school-attending children.

mechanism.

Third, this implies that the backlash should be passed on to subsequent generations, as shown in Proposition 2. In order to test this hypothesis, I run a regression specification with *birth cohorts*, rather than years, as the time variable. In other words, I explore whether children born after the ERA have less favorable attitudes toward male/female equality than children born before. The dynamic difference-in-differences specification depicted in Figure 6 shows a sharp drop in belief in gender equality – i.e., backlash – amongst male children born in the birth cohorts shortly after state ERA passage. Men appear to successfully pass their backlash onto their sons, albeit at a reduced intensity, which is further reduced as time goes on[7].

Fourth, as shown in Proposition 3, persistence of backlash into subsequent generations should be stronger in ideologically homogeneous communities than it heterogeneous ones. This is arguably the most subtle of the implications. However, one can use data on county vote shares in the 1968 Presidential Election – the last one before the advent of the state ERAs – to determine whether individuals live in an ideologically homogeneous or ideologically heterogeneous community.[8] One can generate an indicator variable for whether the individual's county of residence had a 1968 Republican vote share between 40% and 60%. Almost exactly half of counties fall into that category, allowing for an even bifurcation into ideologically "homogeneous" and "heterogeneous" counties. Columns (4) and (5) result from a specification that interacts both the indicator variable for the first ten post-ERA cohorts and the indicator variable for second ten post-ERA cohorts with the ideological homogeneity indicator. This allows one to investigate whether backlash is stronger and/or more persistent in ideologically homogeneous counties. As can be seen, whereas there is backlash in both ideologically homogeneous and heterogeneous counties amongst the cohorts born shortly after ERA passage, for cohorts born more than a decade after its passage, the backlash only persists in ideologically

[7] Note that, because the first of the state ERAs was passed in 1970 and because the question on attitudes toward male/female equality was last asked by the ANES in 2008, no individuals born more than 20 cohorts after ERA passage are available for analysis. This is why the dynamic graph ends at +20.

[8] A measure of the share of liberals and conservatives at the county level would be somewhat more ideal since Democrat:Liberal :: Republican:Conservative was not a perfect correspondence in this era, but such data unfortunately does not exist.

homogeneous counties – precisely in line with the model[9]. In short, this subtlest of implications, too, is borne out in the data.

Fifth, laws should play a unique role in generating backlash, stronger than more bottom-up approaches. In a sense, this is more of an assumption of the model than an implication – it represents the fact that the law, L, is given an special role ($\gamma_L > 0$) in forming children's preferences. While the extension of the model does allow a role for the actions of others in society ($\gamma_N > 0$), every single family in a society rarely moves in concert in the way that a change in legislation does – and thus is unlikely to be capable of inducing strong backlash in the same way as a law. This can be tested by analyzing the other components of the women's movement. While the ERA was one of the movement's primary pillars, it did not stand alone. The entry of women into the labor force, the election of women to political office, and other new laws (such as those pertaining to contraceptive access) were also fundamental to it. In Appendix C, I explore these broader aspects of the women's movement and present evidence that, indeed, laws generated backlash while its more bottom-up aspects did not.

Finally, it is worth discussing the fact that backlash is observed primarily on the part of males. While this is not a direct implication of the baseline model, it is consistent with certain parameter values. For an individual to undergo backlash, it is necessary that they have $\alpha > 0$. That is, the individual must actually care sufficiently about the issue at hand to be willing to deviate from their bliss point in order to transmit their views on the issue to children, friends, and broader society. If the notion of gender roles is a fundamental component of male identity but of lesser importance to most women, then backlash to the ERA would indeed be driven by men. Indeed, there is much evidence from the psychology literature supporting this conjecture. The key importance of gender roles to male identity has been studied extensively in the body of literature known as masculinity research, summarized by Levant and Richmond (2007).

6 Alternative Mechanisms

6.1 Redefinition – a Fake Backlash

[9] The results are qualitatively the same if the threshold is altered to 33%/67% or 25%/75%.

What if the law caused no change whatsoever in attitudes? What if it merely caused the definition of gender equality to be redefined? For example, recall that the main ANES survey question asks individuals to state their attitudes toward male/female equality along a scale of 1 to 7. Consider an individual who is generally supportive of feminism but indifferent about an ERA. Perhaps prior to the ERA he would have considered himself a "2" – close to total commitment to male/female equality. But the fact that the ERA is now law and he is only indifferent might make it harder for the individual to consider himself near the forefront of male/female equality. So perhaps he now marks himself as a "3" or a "4", which would appear as backlash, despite the fact that his attitudes have gone unchanged.

The first response to this conjecture is quite simply that, if it was the case, material consequences in terms of voting patterns or the relationship patterns between men and women should have gone unchanged – the effects should remain limited to a survey where mental re-indexing of this sort can be done. However, I find evidence of material outcomes in a number of different dimensions.

A more direct response relies on the fact that the ANES also asks parallel questions about individual's perceptions of the Democratic Party and Republican Party's positions on the attitude-toward-equality scale. If individuals are mentally modifying the meaning of the index, responses to these two questions should also exhibit a backlash jump after passage of the law. If responses to these questions do not change and the positions of the two parties remain stable while the individual's position changes, this is evidence of a real change in attitudes.

Column (1) of Table 6 reveals that there is no change in individuals' perceptions of Democratic Party attitudes toward male/female equality, but column (2) suggests there may be a change in individuals' perceptions of Republican party attitudes. However, running the corresponding dynamic specifications, represented in Figure 7, reveals the existence of a substantial pre- trend. There is, in fact, no jump in individual's perceptions of either Democratic Party or Republican Party attitudes toward male/female equality resulting from the ERA – just a fairly flat line in the case of the former and a downward trend in the case of the latter (consistent with the Republican party moving in a more socially-conservative direction over the course of

the 1970s and 1980s). This suggests that the backlash is not a "fake" one driven by mental re-definition of the survey question.

6.2 Campaign Effects

Was it indeed the law itself which caused the backlash, or was it the *campaign* surrounding the law? That is, could the culprit for the male reaction have actually been seeing confident feminists forcefully voice their views and critiques of society on a regular basis in the months leading up to the state election? This conjecture does not necessarily seem far-fetched. Fortunately, the manner in which the state ERAs were passed allows for a novel way of adjudicating between these two possible mechanisms.

In the case of every single state ERA which was implemented, the ERA was approved by a majority vote through a ballot question in the style of a referendum. The path to such a referendum, however, takes several steps. In order to be approved for the ballot, a proposed ballot initiative must first collect signatures from a fixed (minimum) number of state residents. Typically the number is in the neighborhood of 5 - 10% of the number of votes cast in the most recent gubernatorial election. If the proposal does not receive the requisite number of signatures, it is discarded and does not make it to the ballot. If it does receive sufficient signatures, the proposal will appear on the subsequent state general election ballot, where it will then be subject to a simple Yes vs. No majority vote.

As such, the total effect of a state ERA can be decomposed into the campaign effect and the law effect. To isolate the campaign effect, the treatment group is the group of states where the ERA made it onto the ballot but did not pass. In such states, there would have been broad campaigns in favor of and against the ERA leading up to the general election – but no ERA itself. The control group, then, consists of the states where the ERA didn't make it onto the ballot at all. Meanwhile, to isolate the law effect, the treatment group is the group of states where the ERA passed. In such states there was both a campaign and implementation of a state ERA. The control group is the group of states where the ERA made it onto the ballot but did not pass – which had been the treatment group in the campaign-effect case. With this setup, one holds

constant the occurrence of a campaign and identifies purely the effect of the law itself.

The results of these regressions can be seen in columns (3) and (4) of Table 6. Comparing the two columns, it is apparent that the effect proceeds entirely through the law; the campaign itself has no significant effect whatsoever.

6.3 Persuasion and Media Effects

Another alternative mechanism is the effect represents ERA opponents ramping up their persuasion efforts in an attempt to convince supporters to turn against the ERA. Since the debate around the Federal ERA was still strongly ongoing after the states had passed their ERAs, ERA opponents would have a very salient reason to continue rallying opposition against the ERA. There is a peculiar facet about this alternative mechanism. If ERA opponents truly possessed such persuasive power, it is a bit odd that they did not make use of it during the campaign and thereby prevent the ERA from being passed in the first place. Still, perhaps it is possible that ERA opponents can speak with a greater, more convincing air of authority once the ERA has been passed and its consequences are beginning to be known to the public.

I present evidence that this does not appear to be the case. Using data from NewspaperARCHIVE, which has amassed a collection of hundreds of millions of local newspaper articles in the United States, I first examine the effects of ERA passage on the number of ERA articles appearing in newspapers and then decompose this into the number of negative- and positive-sentiment ERA articles, taking the ratio of the former to the latter.[10] The left panel of Figure 8 demonstates that ERA passage does indeed lead to an increase in the frequency of articles about the ERA. However, as can be seen from the right panel of the same figure, this increase does not occur disproportionately through negative- or positive-sentiment articles. Both increase by approximately equal amounts, and thus the ratio remains roughly constant. Although we cannot know for certain the "convincing power" of a typical negative-sentiment article relative to a typical positive-sentiment article, it is difficult to argue that persuasion is the main

[10] I count as "positive sentiment" any article featuring the words "Equal Rights Amendment" AND "necessary", "good", OR "positive". I count as "negative sentiment" any article featuring the words "Equal Rights Amendment" AND "unnecessary", "bad", OR "negative". Approximately 10% of articles overlap between the two categories. Results remain non-significant if I drop these overlapping articles.

channel of the effect given these results, especially when coupled with the finding that persuasion efforts during the campaign didn't do much of anything to attitudes.

Somewhat more generally, another way of measuring effects which pertain to information rather than the law in itself is to observe that Nielsen media markets often overlap state borders. Consequently, people watching TV news in one state often receive information about their neighboring state. For example, the majority of TV viewers in the West Texas media market live in El Paso, Texas. This market, however, also encompasses parts of Southern New Mexico. Consequently, the local news (and advertising) in those Southern New Mexico counties will be heavily geared toward West Texas. So individuals living in Southern New Mexico will hear much about the Texas ERA during the campaign and after it is passed (given the salience of the ERA issue in that era), but they will not themselves be subject to the law or its provisions. One can thus run a regression specification which includes two indicator variables – an indicator for whether the respondent's state is an ERA state (the standard indicator variable), another for whether the state containing the majority of the respondent's media market is an ERA state. One can also run a within-state regression with state-by-year fixed effects which relies on comparing counties that are in a non-ERA media market to counties in that are in an ERA media market within the same state (e.g., Northern versus Southern New Mexico before and after Texas' state ERA passage). Columns (5) and (6) of Table 6 runs both of these specifications, and they reveal that information effects through the media are not responsible for that backlash. Indeed, if anything, this channel results in a more *positive* view of male/female equality.

6.4 Policy Mood

Some political scientists – beginning with Stimson (1991) – have conjectured and provided evidence that aggregate public opinion in the United States has undergone a series of oscillations between liberal and conservative positions. This suggests it may not be too surprising for liberal laws to be followed by a conservative shift (and vice versa) not as a result of the laws themselves but of pre-existing trends. Such trends, however, are unlikely to be driving the backlash I uncover. First of all, Stimson's analysis pertains to aggregate, national-level public opinion, not

state-level public opinion. Because the laws I examine are state laws, which are implemented in a staggered fashion, for policy mood to drive my result it would be necessary for differing public-opinion cycles to exist in different states. And if this were true, it would smooth national-level public opinion and make the very cycles Stimson observes non-existent or at least quite muted. In any case, my dynamic specifications include pre-periods, and as was seen, there was no evidence of differential trends prior to treatment in the ERA-adopting states compared to the non-ERA-adopting states. Finally – and perhaps most crucially – Stimson's public-opinion cycles occur across a broad range of ideologically-coded outcomes simultaneously. The public shifts from being more liberal across a broad range of domains to being more conservative across a broad range of domains (or vice versa). My falsification tests showed that implementation of the ERAs led only to a backlash in the dimension of women's rights, not other domains.

6.5 Labor-Market Issues

What if the backlash to the ERA entirely boils down to material economic causes? Men may be concerned that the ERA will give women an edge over men in the labor market with regard to hiring and promotion – or simply that it would entice more women into the workplace, increase competition, and drive down men's wages. This conjecture yields several testable implications. If it is so, then (i) men for whom worries of competition and job precarity are greater should experience a larger backlash; men who are more comfortable or less worried about job/wage loss should be relatively less concerned. Additionally, (ii) married men should experience a relatively weaker backlash (other things equal), as the benefits obtained by their wives should at least partially offset the losses they experience, meaning the net reduction in household income would be lesser for married men. Finally, (iii) there should be backlash to *actual* female labor-market entry. That is, if the backlash to the ERA is a consequence of greater female involvement in the labor force, then greater female involvement in the labor force – measured directly – had better induce backlash itself.

Testing these first two conjectures is straightforward. For (i), it is possible to leverage the income variable in the ANES. Lower-income men are more subject to job precarity and

concerns about wage loss. So one can interact an indicator variable for men in the bottom quintile of the income distribution with the ERA indicator to study whether poorer men undergo a greater backlash. No significant heterogeneity is found, as revealed column (7) of Table 6. Column (8) tests (ii) using an indicator variable for married men, and there, too, no significant heterogeneity is uncovered.

With regard to conjecture (iii), as shown in Table C-1 and discussed in greater detail in Appendix C, the entry of women into the labor force – instrumented for using the previously-described shift-share – did not induce any statistically-significant backlash. If the entry of women into the labor force itself did not generate any backlash, it is hard to argue that the channel through which the ERA generated backlash was entry of women into the labor force.

6.6 Anger

One possible conjecture is that the backlash need not be rational or calculated at all. It may simply be that those who opposed the ERA feel anger toward the government for imposing a law with which they disagree. The immediate implication of such a mechanism, however, is that conservatives should undergo backlash against the Equal Rights Amendment, whereas liberals should not. This implication can be tested on the data, and as we have seen, in Table 5 it already was. Liberals and conservatives both undergo backlash – consistent with the paper's main model but not this alternative. Additionally, it should be noted that another implication of this alternative mechanism is that anger/distaste toward the government actually does increase. Column (9) of Table 6 – which makes use of the trust-in-government index present in the ANES since 1960 – does not even find statistically-significant evidence that this occurs.

6.7 Overturning the Law

A closely-related, more rational version of aforementioned mechanism relates to changing the law. What if individuals backlash against the law because doing so influences what the law will be in the next period? In Appendix A.3, I model why such a mechanism is unlikely to be capable of driving strong backlash. In intuitive terms, whereas an individual has a uniquely

privileged role in inculcating his children with his ideological preferences, any given individual will not have much control over the law. The marginal contribution of one individual to a backlash movement aiming to overturn a law is minimal – a drop in the policy ocean, so to speak. This can offer a very slight additional inducement toward backlash, but not a major one.

7 Beyond the ERA – Other Laws

The state Equal Rights Amendments generated significant and persistent backlash, but is this unique to the ERAs, or does it hold true more generally for other laws as well, as predicted by the model? To answer this question, I investigate some of the most major, most salient social policy laws of the past half-century.

7.1 The Civil Rights and Voting Rights Acts

Racial issues have remained at the forefront of U.S. social policy for virtually the entirety of this country's existence. During the Civil Rights Movement, the federal government passed three landmark laws advancing the rights of Black Americans: The Civil Rights Act of 1964, the Voting Rights Act of 1965, and the Civil Rights Act of 1968. The 1964 Act desegregated public accommodations (such as shops, restaurants, and recreational areas), and consequently it was binding in all the Southern segregated states but not in Northern states where public accommodations were not segregated. The 1965 Act prohibited racial discrimination in voting by outlawing voting requirements that had historically been used to disenfranchise black voters. Examples included literacy tests and the requirement that another registered voter in good standing with the community be required to vouch for you in order to vote. It was binding in a subset of these Southern states which did not meet the Act's requirements in terms of equality in accessibility to voting – specifically, Texas, Louisiana, Mississippi, Alabama, Georgia, South Carolina, and Virginia.[11] The 1968 Act prohibited discrimination on the basis of race or national origin in housing; individuals and neighborhoods would no-longer be able to deny sale, rental, or

[11] A handful of counties in other states – principally North Carolina and Florida – were also bound by the 1965 Act. Whether I exclude these from the analysis or simply mark them as untreated does not meaningfully change the results. The Act was later amended in 1975 to encompass additional jurisdictions, as analyzed in Ang (2019).

financing on these bases. It was binding across the country, as such discrimination had not been limited to the South.

What were the effects of these laws on attitudes toward blacks? Unfortunately, the ANES doesn't start asking relevant questions until the mid-1960s – too late to use for a dynamic specification that allows for observing potential pre-trends. Gallup, fortunately, began asking a relevant question in the 1950s: "If your party nominated a generally well-qualified man for president and he happened to be black, would you vote for him?" This question provides the best information available in this era at reasonably high frequency on attitudes of the general white population toward black people.

It is worth noting that all three acts were, additionally, binding only to the extent that there was any black population in the area. That is, an area that was nearly all-white would scarcely have been affected by these laws; for example, desegregation in public accommodations would not mean having to serve any blacks. Life for the white populace would continue virtually unchanged. Not so in a place that was 40% black. Consequently, it is necessary to interact the law variable with the black share of population in this setting.

Columns (1) through (3) of Table 7 reveal that, indeed, the Civil and Voting Rights Acts of the 1960s engendered a strong and significant backlash, with attitudes toward blacks becoming more negative. Notably, this occurs only in areas with a black population, which is sensible for the aforementioned reason – in places with no black population, when whites were compelled to desegregate public accommodations or surrender the vote to blacks, they effectively weren't compelled to do anything. They may possibly have gotten to experience the "warm glow" that came with patting themselves on the back for being a part of the new paradigm of racial equality, without having to undergo any real lifestyle changes whatsoever.

The top-left panel of Figure 9, which focuses on the Voting Rights Act, shows that there are no visible pre- trends prior to this effect (the Figure plots the interaction coefficient between the legislation and black population share), and consistent with both the ERA case and the model's implications, the effect constitutes a sharp level shift in the immediate aftermath of the law's implementation. These findings are consistent with the historical record and anecdotal accounts

of the era. The South of the 1960s was marked by "massive resistance" to desegregation on the part of white southerners and an increase in the popularity of explicitly racial rhetoric on the part of white southern politicians. Restaurant owner Lester Maddox, for example, won the office of governor in Georgia in 1966 after his public profile was elevated when he brandished an axe handle and chased off black patrons seeking to be served in his restaurant. Apparently – these findings would suggest – such politicians were catering to the hardened preferences of their constituents.

7.2 Gay Marriage Bans and Legalizations

Gay marriage has been another of the biggest and most contentious social policy debates of the past several decades. Beginning in the 1990s and extending into the 2000s, there was a push spearheaded by conservative activists for state Defense-of-Marriage Acts and Defense-of-Marriage Amendments (DOMAs). These laws defined marriage as exclusively between a man and a woman and consequently explicitly proscribed gay marriages. The movement started slowly but gathered strength in the early 2000s – particularly after Massachusetts legalized gay marriage in 2004. In that year alone, 13 states passed such an amendment. At their peak in 2012, 33 states had a DOMA in effect. Unlike the state ERAs, they were almost uniformly successful in referenda, with only two ever failing (Arizona in 2006 and Minnesota in 2012). Even California – often regarded as amongst the most liberal states – passed one in 2008.

California, however, would mark the beginning of the end for the DOMA movement, as it was the first such amendment to be totally held up by courts and not implemented. Challenges to other DOMAs were soon mounted across the states, and many state courts struck down DOMAs and legalized gay marriage in 2013 and 2014. Then, only three years after the number of DOMA states peaked, the Supreme Court struck down all DOMAs and legalized gay marriage nationwide in *Obergefell v Hodges* (2015). Because the DOMAs were rolled out in a staggered fashion and because some states had struck down their own DOMAs and legalized gay marriage before the Supreme Court decision did so nationwide, state variation was generated *in both directions* with regard to gay marriage law.

Unlike the the ERA and the Civil Rights Acts, the DOMAs were fundamentally conservative in nature. The legalization of gay marriage was liberal. This offers a unique opportunity, essentially within-law, to study whether backlash occurs against laws in both ideological directions. Since the late 1980s and early 1990s, the ANES has asked questions about attitudes toward gay people. It has repeatedly asked a question about one's general "feeling thermometer" toward gays – whereby respondents are asked to rate how warmly they feel toward gay people on a scale of 0 to 100. It has also asked questions about attitudes toward gays serving in the military and adoption of children by gays. I study the effects of the implementation – and then the repeal – of the DOMAs on these attitudes.

Column (4) and column (5) of Table 7 suggest that indeed backlash does occur against both liberal and conservative laws. DOMAs induce warmer attitudes toward gays and more support for gays serving in the military and adopting children, as shown in the former table. The striking down of DOMAs and consequent legalization of gay marriage does the opposite – inducing more negative attitudes toward gays and (marginally) less support for gays adopting children – as shown in the latter table.[12] The top-right panel of Figure 9 shows the dynamic specification in this setting; once again, backlash was not occurring prior to the law's passage. While there is some evidence of differential attitudes prior to the law change in states legalizing gay marriage, this actually goes in the *opposite* direction of backlash.

7.3 Gun (De-)Control

Gun control constitutes another major social policy debate that has played out over the past few decades in U.S. politics. The debate over concealed carry is one of the central policy debates within the issue of gun control. This concerns the ability of individuals to legally carry a concealed firearm on their person. These laws have been relaxed over time. In 1986, only 9 states were either Unrestricted or Shall-Issue states – states where concealed-carry is allowed with minimal regulatory impediment. By 2020, 42 states were. Did relaxation of gun control

[12] The question on military service was discontinued in 2016.

induce a backlash?[13]

While the ANES did not ask a question about gun control until more recently, the GSS has asked a Yes/No question about supporting gun permits for decades. This is conducive to analyzing the effects of gun control relaxation on attitudes toward gun control. Column (6) of Table 7 reveals that here, too, there is backlash. The relaxation of gun control leads to more support for gun control. The bottom-right panel of Figure 9 shows the non-existence of statistically-significant pre- trends; as in the other cases, the natural experiment appears to be a clean one. It is worth highlighting that, like the DOMAs, gun control relaxation is a policy typically advocated by conservatives. So here again I find evidence of backlash by against a conservative law change – backlash does not appear to be confined to laws that are at certain points along the political spectrum.

7.4 Marijuana Legalizations

Debates over drug policy have been yet another important front in the "culture war" that makes up the U.S. social policy landscape. Liberals typically support decriminalization/ legalization of at least some drugs, while conservatives typically oppose such policies. Since the 1990s, medical marijuana has increasingly been legalized at the state level, and it currently enjoys that status in 33 states.[14] 17 of these legalizations occurred by referendum; 16 occurred through the state legislature, with the legalizations by referenda occurring earlier on average (2005) than those by legislature (2012). It is important to note that, unlike the other laws profiled in this paper, there was a substantial implementation lag on medical marijuana availability after the law changed – in some cases over 4 years. Consequently, I also obtain the implementation dates (when the first marijuana dispensaries began to operate) for all of the

[13] There exist thousand of idiosyncratic laws relaxing and intensifying gun control at the state level, but unlike concealed-carry, which is a highly-standardized policy question and a particularly salient one – having been marked as a high-priority issue by the NRA – these varying idiosyncratic laws do not lend themselves well to difference-in-differences (or, really, to being coded as a unified variable at all). While I would have liked to analyze gun control intensification as well, concealed-carry changes have only occurred in one direction, and the major example of intensification in a manner standardized across states is the 1994 Federal Assault Weapons Ban. Because only 4 states had implemented a state assault weapons ban prior to this, though, the variation is lacking and not conducive to analysis.

[14] Recreational marijuana, too, has been legalized in a much smaller handful of states, but it had only been rolled out in two by the time of the 2016 wave of the GSS – not conducive to statistical analysis.

aforementioned legalizations from local news reports, and I use these dates in my regressions.

The GSS has asked a simple Yes/No question on attitudes toward marijuana legalization since 1973, which lends itself well to analyzing the effects of these legalizations on attitudes. Column (7) of Table 7 reveals that, indeed, here too there exists a backlash. Marijuana legalization reduces support for marijuana legalization. The bottom-left panel of Figure 9 shows that no significant pre-trends exist in this case, either, though the effect is slightly noisier than some previous laws.

7.5 Supreme Court Potpourri: Interracial Marriage, Abortion, and the Death Penalty

One of the reasons ERA opponents were so concerned about the ERA was because it would give the Supreme Court "another set of words to work with" in an era where the court had become known for rapid and often highly unexpected liberal decisions that had striking implications for the social policy in the United States. Amongst these 1960s/early 1970s court decisions was abortion (*Roe v. Wade*, 1973) – another of the most salient and substantial U.S. social policy debates of the past several decades. This was not the only one, though – the Supreme Court also struck down the practice of prayer in public schools in 1962 (*Lee v. Weisman*), struck down bans on interracial marriage in 1967 (*Loving v. Virginia*), and struck down use of the death penalty in 1972 (*Furman v. Georgia*) – only to re-institute it 4 years later (*Gregg v. Georgia*).

Likely because these decisions were fairly unexpected, limited data exists on public opinion about these issues before the decisions were handed down. For example, Gallup never asked a question about support for school prayer – a very common practice across the country – prior to the court's 1962 decision banning it nationwide. Anecdotally, it is known to be a decision that inspired much consternation amongst a still-very-religious U.S. public, but the lack of data prevents difference-in-differences analysis. The other cases are somewhat more opportune. Gallup asked about interracial marriage, which was banned in some states and legal in others, precisely once before the 1967 decision. Abortion had only been legalized at the state level in some states within the 5 years prior to *Roe v. Wade*. Gallup asked about attitudes toward

abortion in 1969, but the majority of the state legalizations occurred between that year and 1972, leaving little variation. Fortunately, the ANES asked about attitudes toward abortion right on the eve of *Roe v. Wade* in late 1972 and then repeatedly thereafter. Finally, the GSS began asking questions about attitudes toward the death penalty in 1975 – after the variation induced by its ban but just prior to the variation induced by its re-institution. This yields just enough data for a static difference-in-differences specification in each of these three cases, but does not permit examining any potential pre-trends. Still, the fact that these decisions were handed down to the states by the federal government rather than taken on the states' own initiative should be encouraging with regard to their exogeneity.

Columns (8) through (10) of Table 7 show that each of these law changes generated significant backlash.[15] The legalization of interracial marriage appears to have reduced support for interracial marriage; the legalization of abortion appears to have reduced support for abortion; and the re-institution of the death penalty appears to have reduced support for the death penalty. Backlash truly does seem to be a general phenomenon across the breadth of social policy laws.

7.6 Economic Policy – State Tax Changes and State Minimum Wage Increases

What about economic policy? Does it generate backlash? All the aforementioned variation has come from social policy law. Indeed, the extended model of Section 3.3 suggested that backlash should be stronger (i) for laws on issues to which people have deep, emotional or identity-based connections and (ii) for laws where penalties/enforcement are minimal or ill-defined. Both of these would seem to apply most clearly to social policy laws. Most individuals probably don't have a deep, identity-based connection to a specific tax rate or the level of the minimum wage. Furthermore, to the extent that paying minimal taxes matters to some individuals, they may still be reluctant to inculcate their children with a preference for tax-evasion. Unlike the majority of social policy laws, many economic laws come with clearer and more tangible penalties.

[15] Analysis is restricted to whites only for the interracial marriage case because Gallup only asked whites, not minorities, for their attitudes toward interracial marriage the first time the question was asked.

Still, there exists plentiful state-level variation over time on income tax rates and minimum wages, and the ANES asks a battery of questions pertaining to taxation and the role of the government in the economy. Using this variation, Panels 1 and 2 of Table 8 show, respectively, that there is no evidence of backlash in terms of *any* of these outcomes for either tax changes or minimum wage increases – regardless of whether I restrict to border counties or use only federally-induced variation in the minimum wage.[16] This provides some suggestive evidence that, indeed, backlash does not survive the leap from social to economic policy.

8 Conclusion

I find substantial and widespread evidence that laws do indeed affect the attitudes held by the public. However, instead of nudging the public in the direction of the law, the effect is one of persistent backlash. I first set up a model in which individuals care about the ideological preferences of people around them and have an imperfect ability to influence those preferences, which are also influenced by the law. I show that, in this setting, the optimal action in response to a liberal (conservative) law-change is for individuals to shift their actions in a more conservative (liberal) direction. There is a trade-off between public and private pressure, which manifests itself in a type of crowd-out effect. A law that clashes with an individual's ideological preferences places the persistence of those preferences into the future under threat. Society will move away from their ideology and toward the law – unless the individual pushes back against it. Meanwhile, if the law moves closer to an individual's ideological preferences, they can ease up somewhat in pushing their ideology and rely on the state to do so. Consequently, across the ideological spectrum, people move in the opposite direction of the law – backlash.

Empirically, the leading example I investigate is that of the state Equal Rights Amendments of the 1970s, which aimed to legislate gender equality. Amongst the most hotly-debated issues of its time, the ERA barely failed ratification as an amendment to the U.S. Constitution, but an ERA was successfully added to the constitutions of more than half of all states. Using data on

[16] For each state, the binding minimum wage is the maximum of the state minimum and the federal minimum. Many states have minimum wages above the federal minimum, but not all do, so it is possible to restrict solely to federally-induced minimum wage changes for plausibly greater exogeneity.

attitudes toward gender equality from the American National Election Studies (ANES) along with a difference-in-differences identification strategy, I find that passage of a state ERA actually leads to sharp reductions in the attitudes men express toward male/female equality. These findings are robust to a border-county identification strategy, state-specific linear time trends, dynamic difference-in-differences, various permutation tests, the wild bootstrap-t procedure, and a restriction to the closest ERA referenda. I also find evidence that this backlash translates into material outcomes – shifting voting patterns toward the Republican party and shifting norms within marital relationships. Beyond this headline result of backlash, the various subtler implications of the model also hold true – for example, that backlash is strongest amongst those with children, that the backlash is transmitted successfully to the next generation, and that backlash occurs amongst both liberals and conservatives. Furthermore, I present evidence against a variety of alternative mechanisms. Neither economic factors, ramped-up persuasion efforts through the media, anger/spite toward government, nor the campaign leading up to the law are found to be responsible for the backlash.

Finally, I expand my focus beyond the ERA. I show that significant backlash has resulted from virtually every major social policy law of the past half-century in the United States, just as the model would predict. The laws I examine include the Civil Rights Acts of the 1960s, the legalization of abortion in the 1970s, the relaxation of gun control beginning in the 1980s, the Defense-of-Marriage Acts of the 1990s, the legalization of marijuana beginning in the 2000s, the legalization of gay marriage in the 2010s, and more.

The fact that backlash has been so systematic – and the fact that it can lead to material consequences – suggests that social policy laws, be they liberal or conservative, may consistently be accompanied by an additional and non-trivial cost that has heretofore been largely overlooked. More precisely, laws come with a functional component – specifying a crime and the punishment that will be enforced for it – and an expressive component – signaling the beliefs and norms of the society that instituted the law. This paper has argued and presented evidence that the expressive component triggers systematic backlash, which suggests that policymakers should consider the extent to which a law will be functional or expressive. Will it, like the Civil Rights

Acts, generate a strong backlash that nonetheless pales in comparison to the direct, functional benefit of providing a large portion of the citizenry voting rights and the right to equal public accommodation for the first time? Or will it, like the state ERAs, generate massive backlash that seemingly overwhelms small direct effects? Asking these questions can help shape the efficacy of future social policy.

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Tables & Figures

Table 1: State ERA Adoption Years

State	Year of Adoption
California	1879
Wyoming	1890
Utah	1896
New Jersey	1947
Illinois	1970
Pennsylvania	1971
Virginia	1971
Alaska	1972
Hawaii	1972
Maryland	1972
Texas	1972
Washington	1972
Colorado	1973
Montana	1973
New Mexico	1973
Connecticut	1974
New Hampshire	1974
Louisiana	1975
Massachusetts	1976
Rhode Island	1986
Florida	1998
Iowa	1998
Nebraska	2008
Oregon	2014
Indiana	2018
Delaware	2019

Note: This table represents the year in which a state Equal Rights Amendment was passed by each of the above states. This information is from Gladstone (2004). My results are identified off of the 16 state ERAs passed in the 1970s and the 1980s, as this is when the big push for the Equal Rights Amendment occurred and when the ERA was a political issue of central importance. Additionally, the main survey outcome of interest is no longer asked by the American National Election Studies in recent years.

Table 2: Static Specifications – ERA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome: Indicator for Reporting Positive Attitudes toward Gender Equality	State Diff-in-Diff					Border Discontinuity	
Sex:	Both	Male	Female	Male	Male	Male	Female
ERA Indicator	-0.058**	-0.150***	0.015	-0.140***	-0.134***	-0.175***	-0.041
	(0.026)	(0.047)	(0.019)	(0.045)	(0.033)	(0.042)	(0.038)
Permutation Test p-values	0.039	<0.001	0.677	0.001	<0.001	0.009	0.428
Wild Bootstrap-t p-values	0.039	<0.001	0.471	<0.001	<0.001	<0.001	0.321
Dependent Variable Means	0.592	0.609	0.578	0.556	0.630	0.613	0.578
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Border FEs	No	No	No	No	No	Yes	Yes
Individuals in Sample	All	All	All	All	All	Border Residents	Border Residents
Years of Data	1972-1998	1972-1998	1972-1998	1972-1988	1972-2008	1972-1998	1972-1998
Clustering	State	State	State	State	State	State	State
Observations	23,683	10,448	13,235	6677	11,953	3780	4776

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Outcome variable is constructed using the ANES question on attitudes toward male/female equality: "Some people believe that men and women should have an equal role in running business, industry, and government. Others believe a woman's place is the home. Where would you place yourself on this [7-point] scale?" On the scale, 1 indicates total agreement with the former statement; 7 indicates total agreement with the latter. I code a response of 1, 2, or 3 into an indicator variable representing generally positive attitudes toward gender equality. Coefficients in the table can thus be interpreted as changes in the proportion of individuals expressing positive attitudes toward male/female equality.

Table 3: ERA Effects on Voting Patterns

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Rep Vote Indicator		Dem Vote Indicator		Rep Minus Dem Vote Share	
Sex:	Both	Both	Both	Both	Both	Both
ERA Indicator	0.049**	0.064*	0.012	0.007	0.047**	0.098**
	(0.022)	(0.037)	(0.027)	(0.031)	(0.022)	(0.040)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Border FEs	No	Yes	No	Yes	No	Yes
Data Source	ANES	ANES	ANES	ANES	Voting Returns	Voting Returns
Individuals in Sample	All	Border Residents	All	Border Residents	All Voters	Border Voters
Clustering	State	State	State	State	State	State
Observations	12,898	4783	12,898	4783	399	9,443

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Regressions in columns (1)-(4) use the individual-level ANES survey data, which asks a survey question about whom the respondent voted for in the most recent presidential election. Coefficients can thus be interpreted as the change in the vote share associated with ERA passage. Regressions in columns (5) and (6) use official voting returns data on county vote shares.

Table 4: Falsification Tests – ERA Effects on Other Outcomes

Panel 1: Policy-Related Questions	(1) Approve of Abortion	(2) Support Segregation	(3) Support Busing	(4) Approve of Protesting	(5) Support Foreign Interven.	(6) Prioritize Policing over Rights	(7) Support Govt.'s Econ Policy	(8) Govt. Wastes Taxes	(9) Trust Govt.
Sex:	Male	Male	Male	Male	Male	Male	Male	Male	Male
ERA Indicator	-0.072	-0.002	-0.019	0.008	-0.022	-0.016	-0.031	-0.008	0.023
	(0.044)	(0.029)	(0.024)	(0.068)	(0.032)	(0.045)	(0.040)	(0.006)	(0.021)
Observations	3555	3082	3787	1818	8090	3562	1830	10540	10998
Panel 2: Thermometer Questions	(1) Thermom: Blacks	(2) Thermom: Whites	(3) Thermom: Liberals	(4) Thermom: Conserv-atives	(5) Thermom: Unions	(6) Thermom: Big Business	(7) Thermom: Radical Students	(8) Thermom: Black Power	(9) Thermom: Women's Lib
Sex:	Male	Male	Male	Male	Male	Male	Male	Male	Male
ERA Indicator	-1.5163	1.453	-3.560	1.293	0.013	2.770	-1.017	0.120	-3.303**
	(1.390)	(0.924)	(2.228)	(1.167)	(1.899)	(2.671)	(1.905)	(2.406)	(1.503)
Observations	9858	8125	9484	9521	9335	6937	2830	4525	7931

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. All regressions contain state and year fixed-effects and use ANES survey data from years 1972 to 1988 to mirror the main regressions. Panel 1 studies a selected set of important social and economic questions asked in the ANES. In each column, the outcome is an indicator variable for whether the respondent agrees with the statement in the column title. Panel 2 studies thermometer questions asked as of 1972 in the ANES. Thermometer questions asked individuals how warmly they felt toward various groups on a scale of 0 to 100 (100 being warmest). In each column, the outcome is the corresponding thermometer variable.

Table 5: Testing Model Implications

	(1)	(2)	(3)	(4)	(5)
Outcome:	Contemp. Attitudes toward Gender Equality	Contemp. Attitudes toward Gender Equality	Contemp. Attitudes toward Gender Equality	1st Generation Post-ERA Gender Attitudes	2nd Generation Post-ERA Gender Attitudes
Characteristic:	Ideology - Lib/Con Scale	Ideology - Dem/Rep Scale	Have Children Indicator	Ideological Homogeneity Indicator	Ideological Homogeneity Indicator
Sex:	Male	Male	Male	Male	Male
ERA Indicator	-0.178***	-0.145***	-0.076*	-0.086**	0.023
	(0.050)	(0.052)	(0.043)	(0.035)	(0.029)
ERA Indicator	0.008	-0.001	-0.043**	-0.035***	-0.067**
*Characteristic	(0.008)	(0.006)	(0.019)	(0.014)	(0.031)
Year FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
Years of Data	1972-1988	1972-1988	1972-1998	1972-2008	1972-2008
Clustering	State	State	State	State	State
Observations	7,922	10,388	6,938	10,322	10,322

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. All columns feature the same outcome variable as in the main regression table: an indicator variable for those expressing positive attitudes toward gender equality. Coefficients in the table can thus be interpreted as changes in the share of males expressing positive attitudes toward gender equality. However, columns (1), (2), and (3) result from the standard *year* difference-in-differences specification comparing changes in attitudes in years before versus after ERA adoption. Columns (4) and (5) result from a *cohort* difference-in-differences specification comparing changes in attitudes in birth cohorts born before versus after ERA adoption in order to understand the extent of backlash amongst the subsequent generation. The interacted characteristic in column (2) is a 7-point "Very Conservative" through "Very Liberal" self-reported ideology scale. The interacted characteristic in column (3) is a 7-point "Strong Democrat" through "Strong Republican" self-reported party identification scale. The interacted characteristic in columns (4) and (5) – the ideological homogeneity indicator – is an indicator for whether the county the individual lives in had a Republican vote share between 40% and 60% in 1968. Column (4) studies the first ten cohorts born after ERA adoption; column (5) studies cohorts 10 through 20.

Table 6: Alternative Mechanisms

	(1) Dem Party's Position	(2) Rep Party's Position	(3) Campaign Effect	(4) Law Effect	(5) ERA and ERA DMA	(6) DMA Within-State	(7) Low Income	(8) Marital Status	(9) Gov. Trust Index
Outcome:	Views of Dem Party's Attitude	Views of Rep Party's Attitude	Attitude toward Gender Equality	Attitude toward Gender Equality	Attitude toward Gender Equality	Attitude toward Gender Equality	Attitude toward Gender Equality	Attitude toward Gender Equality	Trust in Govt.
Sex:	Male	Male	Male	Male	Male	Male	Male	Male	Male
ERA Indicator	0.009	-0.038**	-0.025	-0.177***	-0.199***		-0.146***	-0.121**	-0.096
	(0.025)	(0.016)	(0.052)	(0.053)	(0.057)		(0.046)	(0.046)	(1.368)
ERA_DMA					0.052	0.059*			
Indicator					(0.034)	(0.031)			
ERA Indicator							-0.015	-0.028	
*Characteristic							(0.030)	(0.026)	
Characteristic							Q1 Income	Married	
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	DMA	State	State	State
Observations	12,165	12,165	6089	4903	10,448	10,448	9719	10,425	10,971

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. All regressions use 1972-1988 data for consistency with the main specifications. Columns (1) and (2) use data on respondents' views about a political party's attitudes toward gender equality to demonstrate there is little evidence of the main effect representing a mental redefinition of what it means to support gender equality resulting from the ERA. Columns (3) and (4) test whether the backlash results from the campaign instead of the law itself; in column (3), states where the ERA was on the ballot but never passed are the treatment group and states without the ERA on the ballot are the control group; in column (4), states where the ERA passed are the treatment group and states where the ERA was on the ballot but never passed are the control group. Columns (5) and (6) study whether the backlash is driven by media-market effects, finding no evidence of this using the fact that media markets overlap state borders (such that a media-market primarily serving state A where an ERA is passed may also encompass part of state B, where no ERA is in effect but individuals would nonetheless be exposed to media coverage of the ERA). Columns (7) and (8) explore whether material concerns may be responsible for the backlash, finding no evidence that men in the lowest quintile of income (who one would expect to be more sensitive to increased labor-market competition) and no evidence that married men (who may experience countervailing positive material effects of the ERA through their wives) exhibit different levels of backlash. Column (9) studies effects of the state ERAs on the index for trust in government found in the ANES.

Table 7: Other Social Policy Laws

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Civil Rights Act 1964	Voting Rights Act 1965	Civil Rights Act 1968	Gay Marriage Bans	Gay Marriage Legal-ization	Gun Control	Marijuana Legal-ization	Interracial Marriage Legal-ization	Death Penalty Legal-ization	Abortion (Roe v. Wade)
Outcome:	Would Vote for Black President	Would Vote for Black President	Would Vote for Black President	Feeling Thermom: Gays	Feeling Thermom: Gays	Support Gun Permits	Support Marijuana Legalization	Support Interracial Marriage	Support Death Penalty	Support Abortion
Sub-population:	Whites	Whites	Whites	All	All	All	All	Whites	All	All
Law Indicator	0.234***	0.258***	0.436***	2.677***	-4.689***	-0.036**	-0.044*	-0.085***	-0.052*	-0.094***
	(0.067)	(0.059)	(0.030)	(1.009)	(1.386)	(0.015)	(0.020)	(0.027)	(0.025)	(0.026)
Law Indicator	-0.892***	-0.989***	-0.511***							
* Black Pop. Share	(0.357)	(0.340)	(0.181)							
Dependent Var. Means	0.368	0.368	0.368	40.060	49.751	0.763	0.298	0.104	0.692	0.249
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years of Data	1958-1978	1958-1978	1958-1978	1984-2012	2012-2016	1973-2016	1973-2016	1958, 1968	1974-1980	1972, 1976
Clustering	State	State	State	State	State	State	State	State	State	State
Observations	23,100	23,100	23,100	20,332	8,114	38,828	36,099	2,891	8,451	4,581

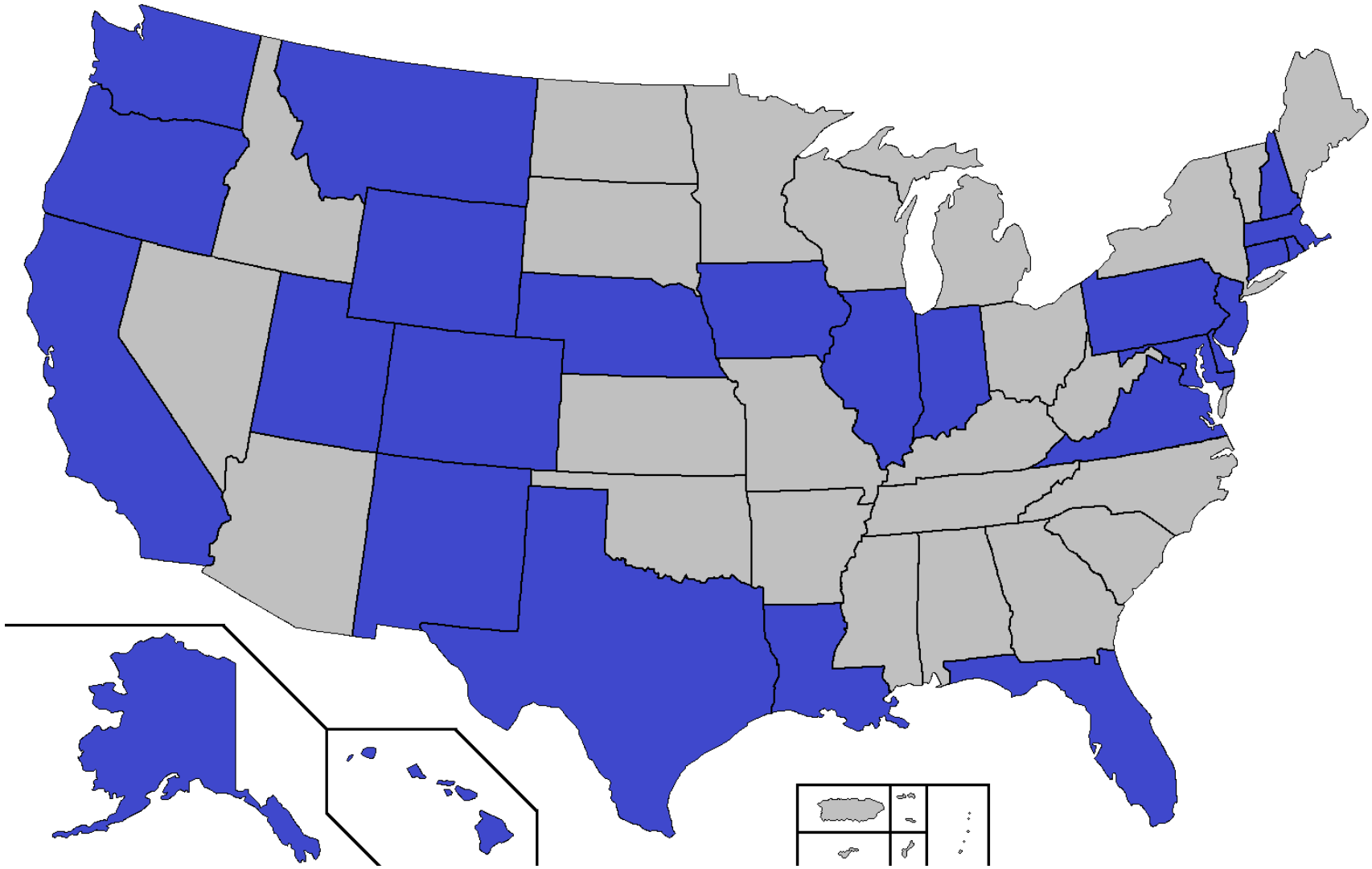
Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Data for Civil Rights Act and interracial marriage regressions is from Gallup. Data for gay marriage and abortion regressions is from the ANES. Data for gun control, marijuana, and death penalty regressions is from the GSS. The samples for the civil rights and interracial marriage regressions are restricted to whites only because whites were initially the only group of individuals asked these questions by Gallup. In the case of the Civil Rights Acts, their provisions would only be binding in places with any black population, so it is necessary to interact the indicator variable for the law with black population share. The outcome variable in columns (1) through (3) is an indicator for whether respondents would be willing to vote for a black candidate for president. The outcome variable in columns (4) and (5) is a 0-to-100 feeling thermometer measuring respondents' attitudes toward gay people. The outcome variables in columns (6) through (10) are indicators for individuals' self-reported support of the corresponding issue.

Table 8: Economic Policy Laws

Panel 1: Tax Changes	(1) Government Wastes Taxes	(2) Government Wastes Taxes	(3) Government Healthcare	(4) Government Healthcare	(5) Government Job Guarantee	(6) Government Job Guarantee	(7) Government More Services	(8) Government More Services
Tax Change	-0.003	-0.006	-0.001	-0.004	0.005	-0.003	0.009***	0.019***
	(0.005)	(0.008)	(0.001)	(0.007)	(0.005)	(0.004)	(0.003)	(0.005)
Individuals in Sample	All	Border Residents	All	Border Residents	All	Border Residents	All	Border Residents
Observations	26,596	9,600	18,412	6,733	25,913	9,475	26,284	9,586
Panel 2: Minimum Wage Changes	(1) Government Wastes Taxes	(2) Government Wastes Taxes	(3) Government Healthcare	(4) Government Healthcare	(5) Government Job Guarantee	(6) Government Job Guarantee	(7) Government More Services	(8) Government More Services
MW Change	-0.042***	-0.055***	-0.015	0.024	0.009	0.004	0.012	0.006
	(0.013)	(0.017)	(0.014)	(0.015)	(0.013)	(0.011)	(0.013)	(0.014)
Set of MW Changes	All	Fed-Induced	All	Fed-Induced	All	Fed-Induced	All	Fed-Induced
Individuals in Sample	All	All	All	All	All	All	All	All
Observations	37,800	29,146	27,000	20,697	39,963	30,063	30,572	23,720

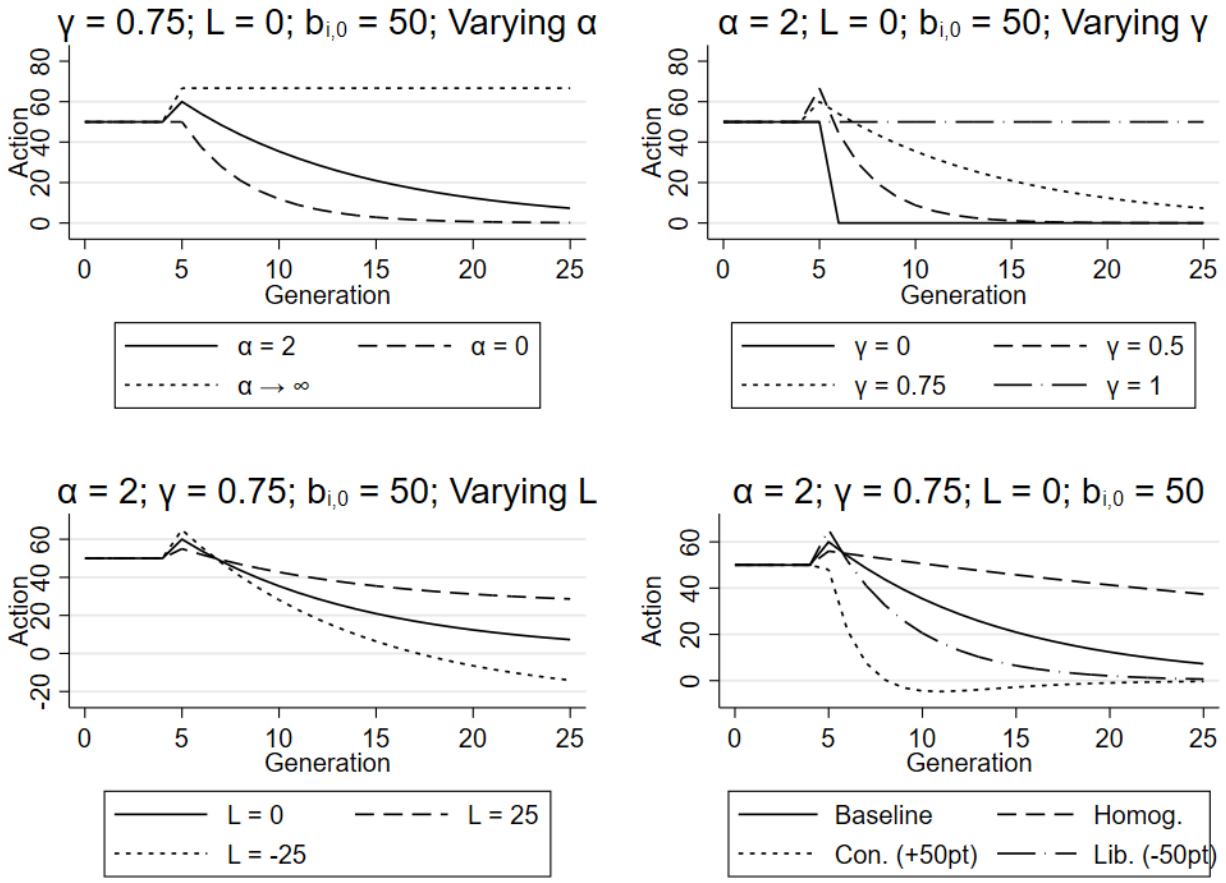
Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. All regressions use ANES data and include state fixed-effects and year fixed-effects, mirroring the main specifications. Right-hand-side variable in Panel 1 is the cumulative change in the state top marginal income tax rate over the past 5 years. Panel 2 is an indicator variable for whether there has been a minimum wage increase in the past 5 years. (Results are minimally changed if one defines the changes over 1 year or 2 years instead.) Columns (1) and (2) use a question about whether the government wastes taxes as the outcome. Columns (3) and (4) use a question about whether the government should provide healthcare. Columns (5) and (6) use a question about whether the government should provide a job guarantee for citizens. Columns (7) and (8) use a question about whether the government should have a role in providing more (as opposed to fewer) services. In some cases, these questions included multiple possible responses (e.g., "Yes", "No", "Not Sure"). In all such cases, I generated an indicator variable corresponding to the "Yes" response.

Figure 1: State Equal Rights Amendment Map



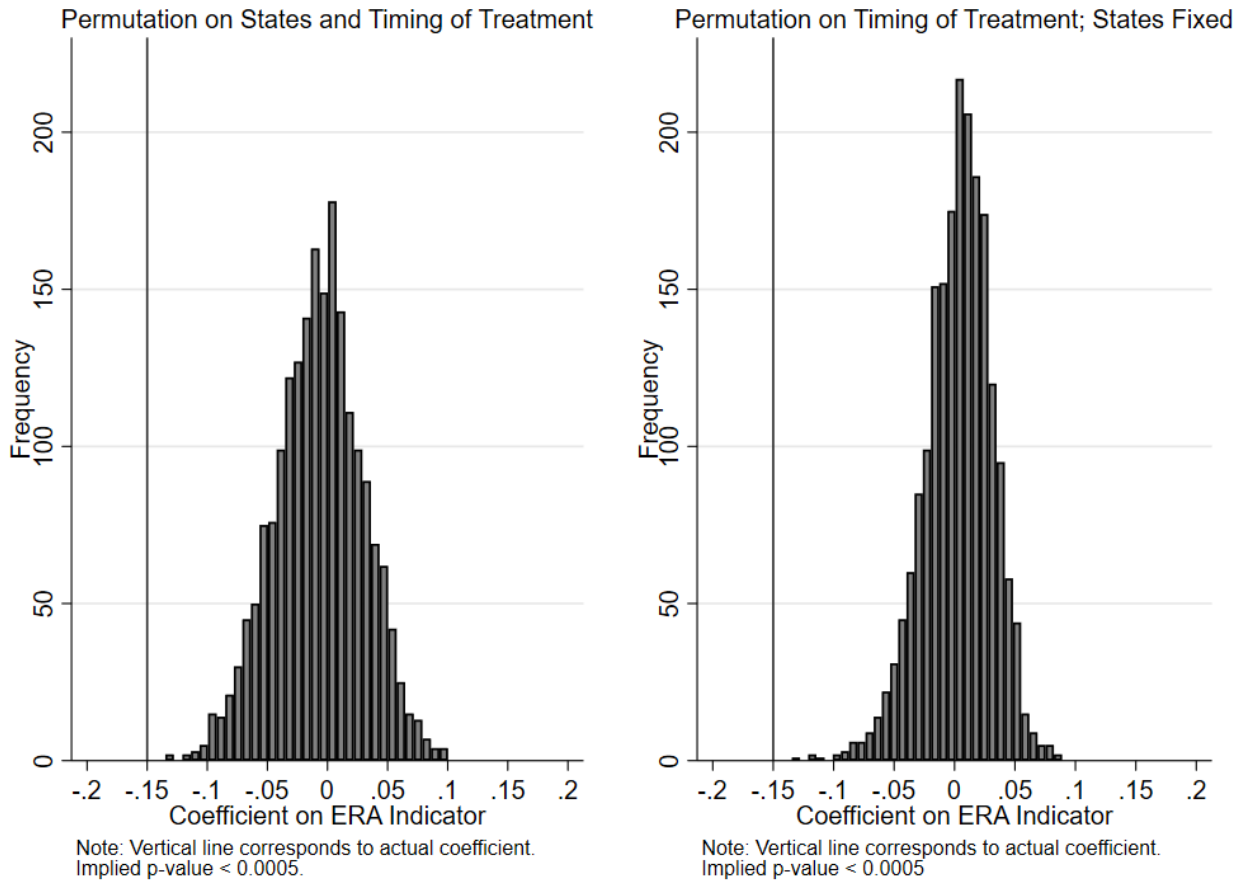
Note: Blue coloration denotes states with state ERAs (as of 2022).

Figure 2: Effects of a Law Change, with Varying Parameter Values



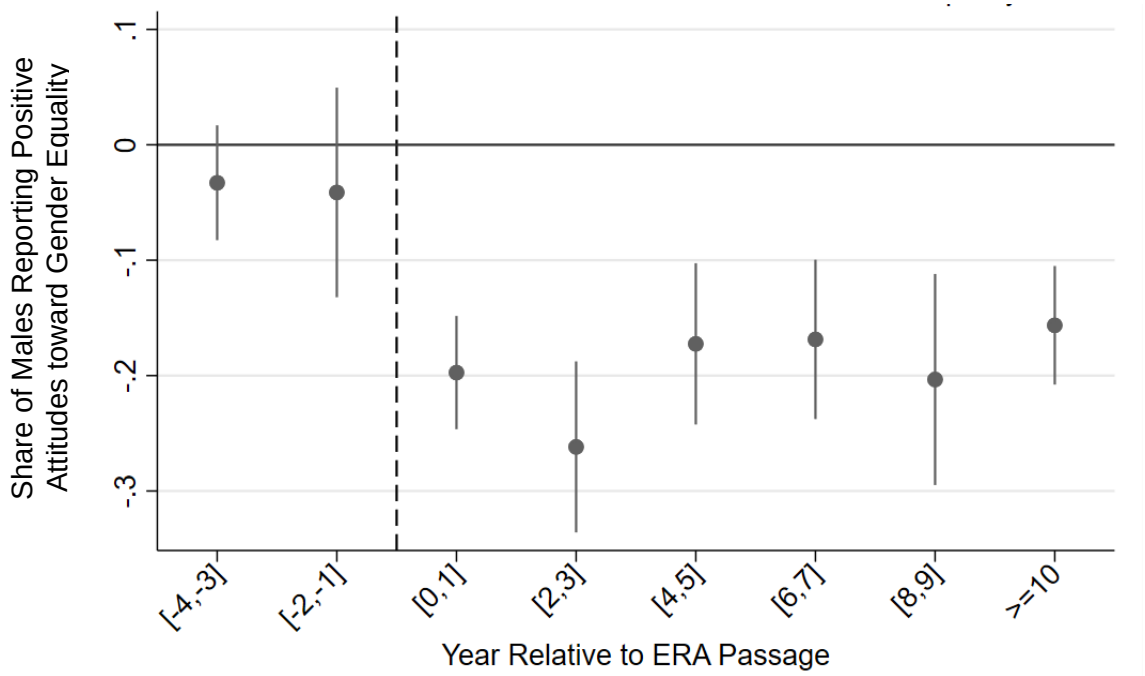
Note: Each panel in Figure 2 considers the effects of a law change from $L = 50$ to $L = 0$ in generation five on the subsequent actions of a family i which initially has ideological bliss point $b_{i,0} = 50$. In the top-left panel, the parameter α – governing the extent to which families care about the ideological preferences of the next generation – is permitted to vary, with backlash resulting as long as $\alpha > 0$ (though decaying over time as long as $\alpha < 1$). In the top-right panel, γ – which governs the extent to which families have ideological influence over their children – is allowed to vary, with backlash resulting as long as parental influence is existent but incomplete ($0 < \gamma < 1$). In the bottom-left panel, the ideological character of the new law is permitted to vary, with backlash occurring in all cases (though of varying magnitudes). In the bottom-right panel, the extension to the model featuring a role for broader society is considered. (The society is held in stasis until the law change in period 5.) An ideologically-homogeneous society generates stronger persistence of the initial ideology than a heterogeneous one wherein half of society is 50 points more liberal or 50 points more conservative than family i. Note that the interpretation of a "generation" in this figure is a literal decades-long generation. Consequently, it may take decades before the backlash is undone.

Figure 3: Permutation Tests



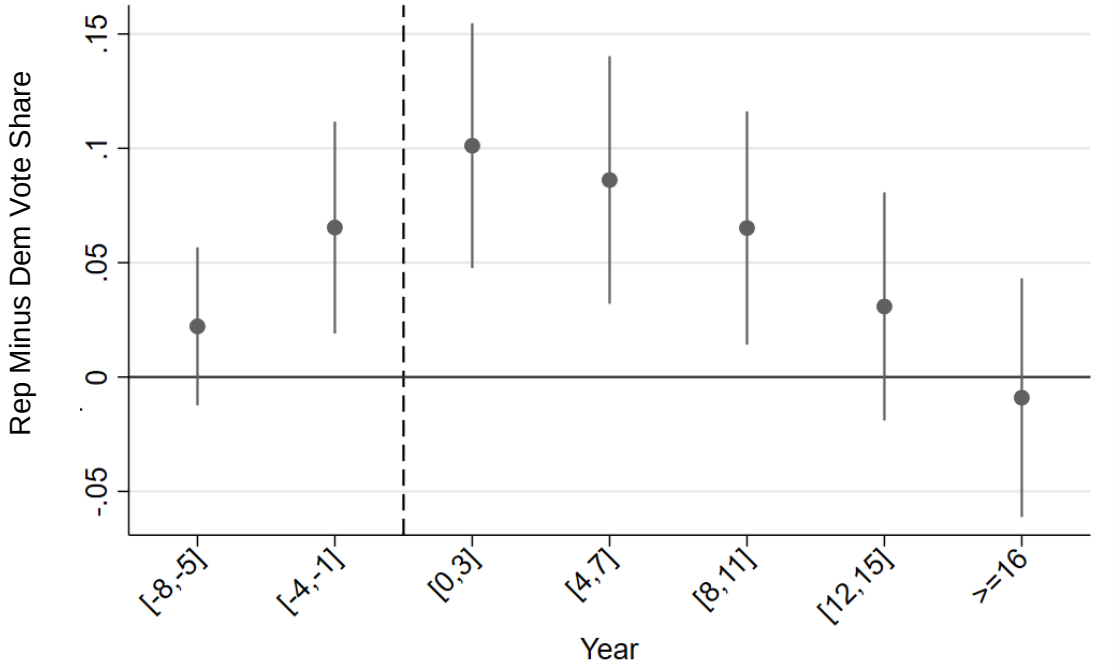
Note: The permutation test displayed in the left panel randomly selects 16 states to receive a placebo ERA, then re-assigning the year of treatment at random from the list of the 16 actual treatment years of the 1970s/80s-era ERAs. The permutation test displayed in the right panel holds constant the 16 states which receive treatment but re-assigns their treatment years at random.

Figure 4: Dynamic Differences-in-Differences – ERA Effects on Male Attitudes



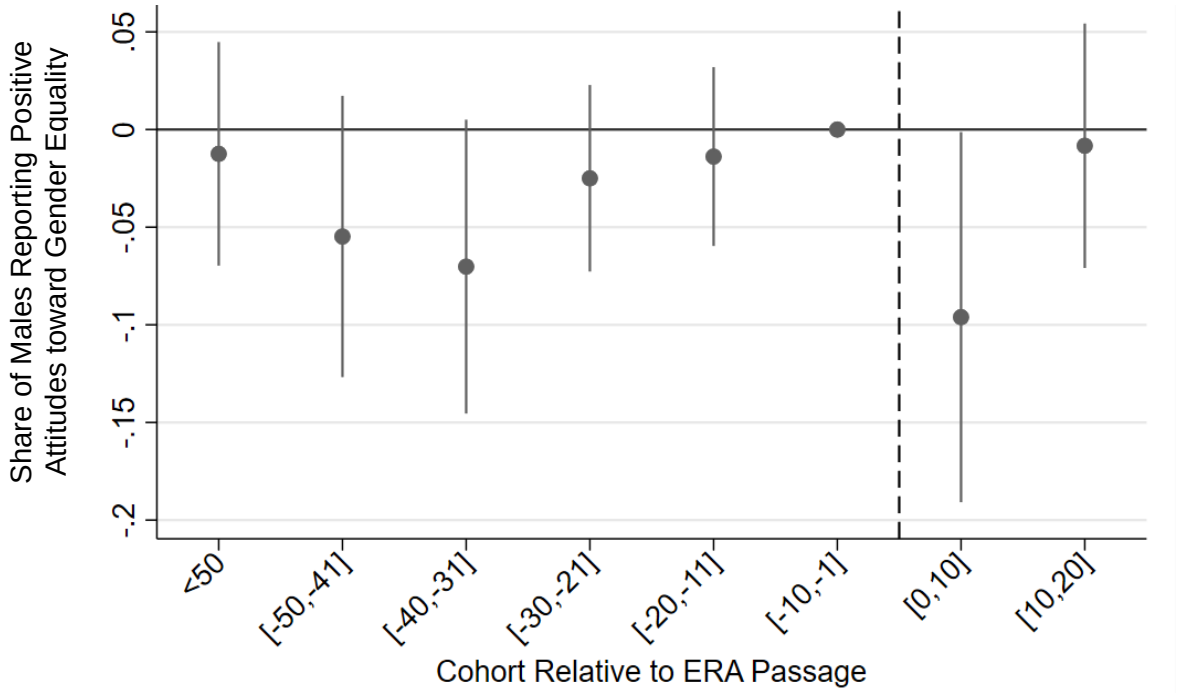
Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -4.

Figure 5: Dynamic Differences-in-Differences – ERA Effects on Voting Patterns



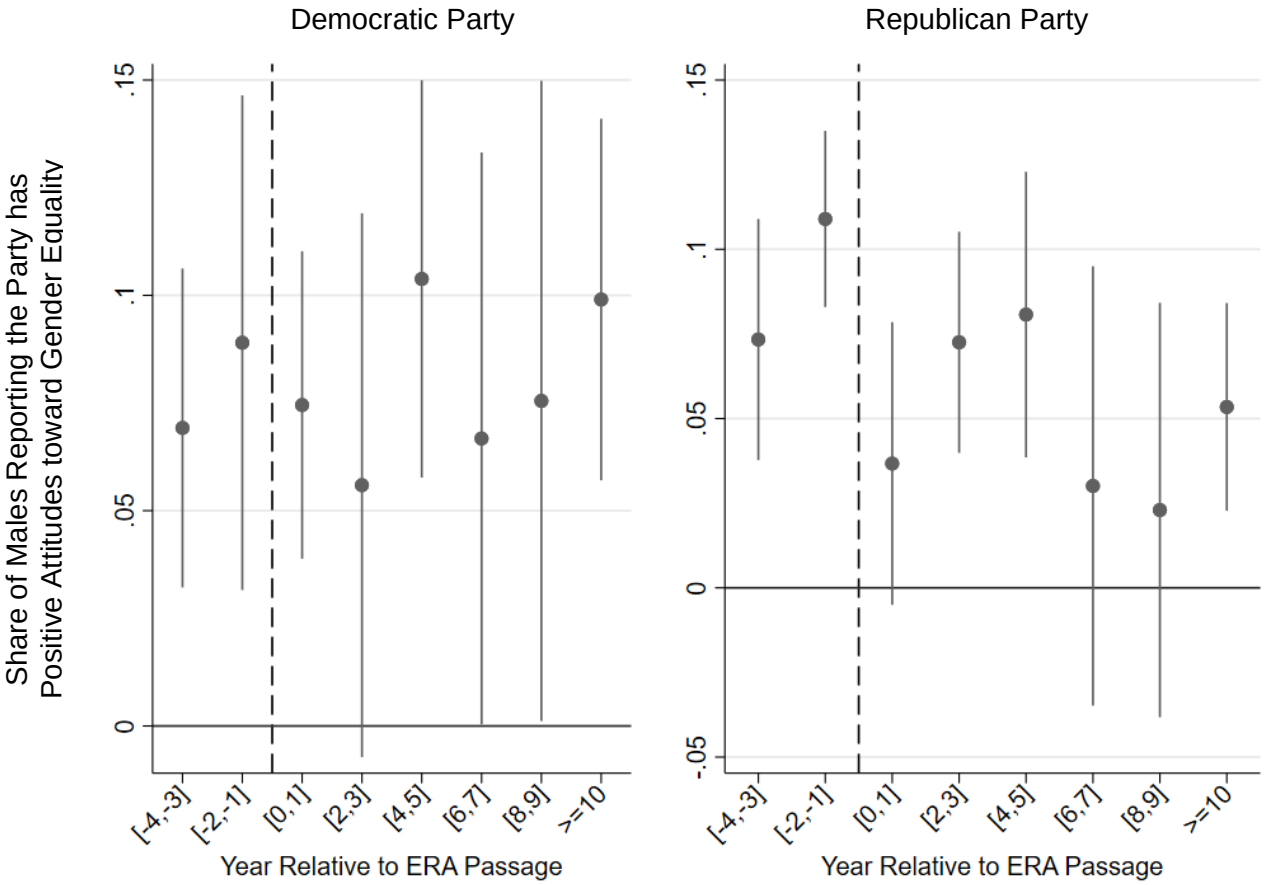
Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -8.

Figure 6: Dynamic Differences-in-Differences – ERA Effects on the Next Generation of Men



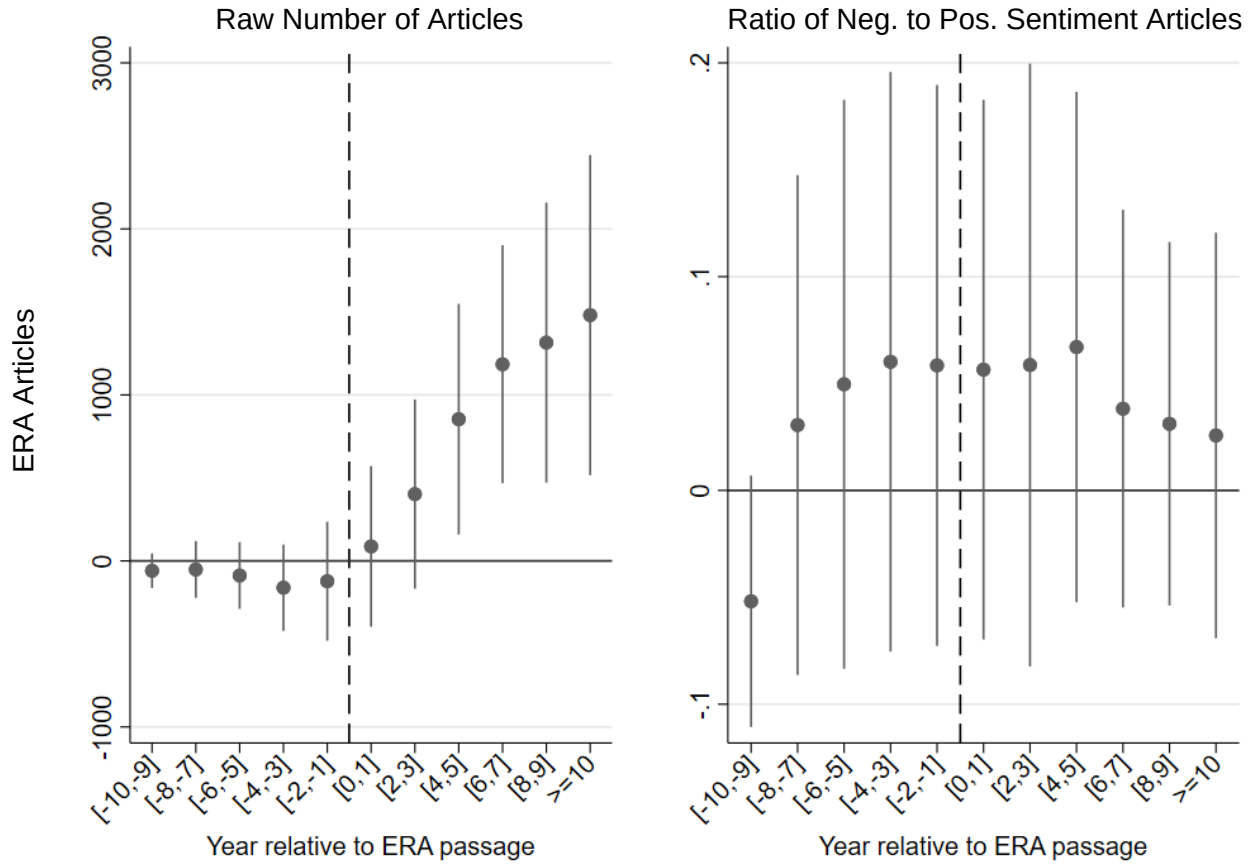
Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is cohorts [-10, -1].

Figure 7: Dynamic Differences-in-Differences – ERA Effects on Male Perception of Party Attitudes



Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -4. The ANES survey questions represented in these graphs is analogous to the main survey question, but instead of asking the respondent's position on the 1-to-7 gender equality scale, they ask where the respondent would place the Democratic party and the Republican party on the very same scale. As in the main specifications, I create an indicator variable representing generally positive attitudes toward gender equality from responses of 1, 2, or 3 on the scale.

Figure 8: Dynamic Differences-in-Differences – ERA Effects on Newspaper Articles about the ERA



Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -10. I count as "positive sentiment" any article featuring the words "Equal Rights Amendment" AND "necessary", "good", OR "positive". I count as "negative sentiment" any article featuring the words "Equal Rights Amendment" AND "unnecessary", "bad", OR "negative". Approximately 10% of articles overlap between the two categories. Results remain non-significant if I drop these overlapping articles. Results remain non-significant if I use a broader dictionary of positive and negative synonyms.

Figure 9: Dynamic Differences-in-Differences – Effects of Other Major Social Policy Laws on Corresponding Attitudes



Note: Year 0 corresponds to the year the relevant law took effect. In the top-left panel, the space between some coefficients is not to (time) scale because Gallup did not always ask the relevant question at consistent intervals in a way comparable to academic survey datasets such as the ANES or the GSS. Furthermore, in the top-left panel the plotted coefficients are the interaction terms between black population share and an indicator for the 1965 Voting Rights Act. As discussed in more detail in the body text of the paper, this is because – unlike the other laws studied here – the Civil Rights Acts of the 1960s were only binding where black population actually existed.

Appendix A: Proofs and Extensions to the Model

A.1 Proofs

Proof of Proposition 1: First, substituting the equation for the formation of ideological preferences into the utility function, we have

$$u_{i,t}(x_{i,t}) = -(x_{i,t} - b_{i,t})^2 - \sum_j \alpha_j (\gamma_b b_{j,t} + \gamma_x \bar{x}_t + \gamma_L L - b_{i,t})^2 .$$

Differentiating the utility function,

$$\partial u_{i,t} \Big/ \partial x_{i,t} = -2(x_{i,t} - b_{i,t}) - \sum_j 2\alpha_j \gamma_x \frac{1}{N}\left(\gamma_b b_{j,t} + \gamma_x \frac{1}{N}\sum_k x_{k,t} + \gamma_L L - b_{i,t}\right) = 0$$

$$\Rightarrow x_{i,t} = \left(1 + \sum_j \alpha_j \gamma_x \frac{1}{N}\right) b_{i,t} - \left(\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}\right) L - \left(\sum_j \alpha_j \gamma_x \gamma_b \frac{1}{N} b_{j,t}\right)$$

$$- \left(\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}\right) x_i - \left(\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}\right) \sum_{k \neq i} x_{k,t}$$

$$\Rightarrow x_{i,t} = \frac{1 + \sum_j \alpha_j \gamma_x \frac{1}{N}}{1 + \sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} b_{i,t} - \frac{\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}}{1 + \sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} L - \frac{\sum_j \alpha_j \gamma_x \gamma_b \frac{1}{N}}{1 + \sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} b_{j,t} - \frac{\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}}{1 + \sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} \sum_{k \neq i} x_{k,t}.$$

This is not a closed-form solution, as $x_{k,t}$ itself depends on L and on $b_{j,t}$ $\forall j$. So,

$$\frac{\partial x_{i,t}}{\partial x_{j,t}} = -\underbrace{\frac{\sum_j \alpha_j \gamma_x^2 \frac{1}{N}}{1 + \sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}}}_{\equiv A} \frac{1}{N}\left(1 + (N-2)\frac{\partial x_{k,t}}{\partial x_{j,t}}\right)$$

$$\frac{\partial x_{i,t}}{\partial x_{j,t}}\left(1 + \frac{N-2}{N} A\right) = -\frac{1}{N} A$$

$$\frac{\partial x_{i,t}}{\partial x_{j,t}} = \frac{-\frac{1}{N} A}{\frac{N + (N-2)A}{N}} = -\frac{A}{N + (N-2)A}.$$

Using this result to derive the necessary comparative static,

$$\frac{\partial x_{i,t}^*}{\partial L} = -\frac{\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}}{1+\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} - A\left(\frac{\partial x_{i,t}}{\partial L} + \frac{\partial x_{j,t}}{\partial x_{i,t}}\frac{\partial x_{i,t}}{\partial L}\right)$$

$$\frac{\partial x_{i,t}^*}{\partial L}\left(1+A-\frac{A^2}{N+(N-1)A}\right) = -\frac{\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}}{1+\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}}$$

$$\frac{\partial x_{i,t}^*}{\partial L}\left(\frac{N+(2N-1)A+(N-2)A^2}{N+(N-1)A}\right) = -\frac{\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}}{1+\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}}$$

$$\frac{\partial x_{i,t}^*}{\partial L} = -\frac{\sum_j \alpha_j \gamma_x \gamma_L \frac{1}{N}}{1+\sum_j \alpha_j \gamma_x^2 \frac{1}{N^2}} \frac{N+(N-1)A}{N+(2N-1)A+(N-2)A^2} < 0.$$

Proof of Proposition 2: First, substituting children's preferences into the utility function,

$$u_{i,t}(x_{i,t}) = -(b_{i,t}-x_{i,t})^2 - \alpha(b_{i,t}-\gamma x_{i,t}-(1-\gamma)L)^2 .$$

Differentiating the utility function and setting the result equal to zero in order to find a maximum,

$$\partial u_{i,t} \Big/ \partial x_{i,t} = -2x_{i,t} + 2b_{i,t} - 2\alpha\gamma\left(\gamma x_{i,t} + (1-\gamma)L - b_{i,t}\right) = 0$$
$$\Rightarrow x_{i,t}(1+\alpha\gamma^2) = b_{i,t}(1+\alpha\gamma) - L(\alpha\gamma(1-\gamma))$$
$$\Rightarrow x_{i,t}^* = \frac{1+\alpha\gamma}{1+\alpha\gamma^2} b_{i,t} - \frac{\alpha\gamma(1-\gamma)}{1+\alpha\gamma^2} L.$$

This means that

$$\begin{aligned} x_{i,t+1} &= \frac{1+\alpha\gamma}{1+\alpha\gamma^2} b_{i,t+1} - \frac{\alpha\gamma(1-\gamma)}{1+\alpha\gamma^2} L \\ &= \frac{1+\alpha\gamma}{1+\alpha\gamma^2}\left(\gamma x_{i,t} + (1-\gamma)L\right) - \frac{\alpha\gamma(1-\gamma)}{1+\alpha\gamma^2} L \\ &= \gamma\left(\frac{1+\alpha\gamma}{1+\alpha\gamma^2}\right)^2 b_{i,t} - \gamma\left(\frac{1+\alpha\gamma}{1+\alpha\gamma^2}\right)\left(\frac{\alpha\gamma(1-\gamma)}{1+\alpha\gamma^2}\right)L + \frac{(1-\gamma)(1+\alpha\gamma)}{1+\alpha\gamma^2} L - \frac{\alpha\gamma(1-\gamma)}{1+\alpha\gamma^2} L \\ &= \gamma\left(\frac{1+\alpha\gamma}{1+\alpha\gamma^2}\right)^2 b_{i,t} - \frac{\gamma(1+\alpha\gamma)\left(\alpha\gamma(1-\gamma)\right) + \left(\alpha\gamma(1-\gamma)\right)(1+\alpha\gamma^2) - (1-\gamma)(1+\alpha\gamma)(1+\alpha\gamma^2)}{1+\alpha\gamma^2} L. \end{aligned}$$

Consequently,

$$
\begin{aligned}
&\partial x^{*}_{i,t+1} \big/ \partial L < 0 \\
&\Leftrightarrow \gamma(1+\alpha\gamma)\big(\alpha\gamma(1-\gamma)\big)+\big(\alpha\gamma(1-\gamma)\big)(1+\alpha\gamma^2)-(1-\gamma)(1+\alpha\gamma)(1+\alpha\gamma^2)>0 \\
&\Leftrightarrow \gamma(1+\alpha\gamma)\alpha\gamma+\alpha\gamma(1+\alpha\gamma^2)-(1+\alpha\gamma)(1+\alpha\gamma^2)>0 \\
&\Leftrightarrow \gamma(1+\alpha\gamma)\alpha\gamma+\alpha^2\gamma^3-1-\alpha\gamma^2-\alpha^2\gamma^3>0 \\
&\Leftrightarrow \alpha^2\gamma^3-1>0 \\
&\Leftrightarrow \alpha > \sqrt{\gamma}\big/\gamma^2 .
\end{aligned}
$$

Proof of Proposition 3: It is without loss of generality to set $L = 0$ here since the bliss points of both types of individuals are defined relative to L. Taking the first-order condition,

$$
\begin{aligned}
&\partial u_{i,t} \big/ \partial x_{i,t} = -2(x_{i,t}-b_{i,t})-2\alpha\gamma_P(\gamma_P x_{i,t}+\gamma_N \bar{x}_t^{j\neq i}+\gamma_L L-b_{i,t})=0 \\
&\Rightarrow x_{i,t}=\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}b_{i,t}-\frac{\alpha\gamma_P\gamma_L}{1+\alpha\gamma_P^2}L-\frac{\alpha\gamma_P\gamma_N}{1+\alpha\gamma_P^2}\frac{1}{N}\sum\nolimits_j x_{j,t}.
\end{aligned}
$$

Case (a): Homogeneous society – every individual has identical ideology $b_{i,t}$

Since all individuals have the same underlying ideological preference, their problems are symmetric, and they will all have the same optimal action. In other words, the solution from Proposition 3 simplifies to

$$
\begin{aligned}
x_{i,t} &= \frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}b_{i,t}-\frac{\alpha\gamma_P\gamma_N}{1+\alpha\gamma_P^2}x_{i,t} \\
x_{i,t}\left(\underbrace{1+\frac{\alpha\gamma_P\gamma_N}{1+\alpha\gamma_P^2}}_{=B}\right) &= \frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}b_{i,t} \\
x^{hom,*}_{i,t} &= \frac{1}{B}\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}b_{i,t}.
\end{aligned}
$$

Case (b): Heterogeneous society – half of population with ideology $b_{i,t}$, half with ideology $-b_{i,t}$

By symmetry, in this society we will have $\bar{x}_t = \frac{N}{2}x_{i,t}+\frac{N}{2}(-x_{i,t})=0$. As such, the above solution simplifies to

$$x_{i,t}^{het,*} = \frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2} b_{i,t}.$$

Consider the persistence of actions into future generations. By definition of $b_{i,t}$, we have

$$x_{i,t+k}^{hom,*} = \frac{1}{B}\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2} b_{i,t+k} = \frac{1}{B}\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}(\gamma_P+\gamma_N) x_{i,t+k-1}^{hom,*} = \left[\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}\frac{(\gamma_P+\gamma_N)}{B}\right]^k x_{i,t}^{hom,*},$$

$$x_{i,t+k}^{het,*} = \frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2} b_{i,t+k} = \frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}\gamma_P x_{i,t+k-1}^{het,*} = \left[\frac{1+\alpha\gamma_P}{1+\alpha\gamma_P^2}\gamma_P\right]^k x_{i,t}^{het,*}.$$

Consequently, if $(\gamma_P+\gamma_N)/B > \gamma_P$, then $x_{i,t+k}^{het,*}$ will be the closer to 0 of the two expressions for sufficiently large k. Observe that

$$\begin{aligned}
&(\gamma_P+\gamma_N)/B \; - \gamma_P \\
&= \frac{(\gamma_P+\gamma_N)(1+\alpha\gamma_P^2)}{(1+\alpha\gamma_P^2+\alpha\gamma_P\gamma_N)} - \gamma_P \\
&= \frac{\gamma_N}{1+\alpha\gamma_P^2+\alpha\gamma_P\gamma_N} > 0.
\end{aligned}$$

So, indeed, $|x_{i,t+k}^{hom,*} - L| > |x_{i,t+k}^{het,*} - L|$ for sufficiently large k.

A.2 Extension – Endogenized Laws, Voting

Given that backlash is systematic, will any laws ever be passed in the first place? In order to answer this question, it is possible to fully endogenize the passage of laws. Consider a scenario where individuals, at the start of each period, vote on changing the law in a referendum. They are given the choice between re-affirming the law that was in effect in the previous period or replacing it with a law corresponding to the bliss point of the median voter, b_{median}. Individuals have the following utility function – a slight adaptation of the simplified case:

$$u_{i,t}(x_{i,t}) = -(x_{i,t}-b_{i,t})^2 - \alpha(b_{i,t+1}-b_{i,t})^2 - \sum_j \beta(x_{j,t}-b_{i,t})^2$$

$$b_{i,t+1} = \gamma x_{i,t} + (1-\gamma)L$$

Ideological preferences are formed as in the simplified case. A third term is added to the utility function to indicate that individuals care about the extent to which *other* members of society take actions close to their preferences. For example, conservatives wish others behaved in a manner

consistent with conservative ideology and liberals wish others behaved in a manner consistent with liberal ideology. Note that we could think of this new third term as having been present in the utility function of the simplified case as well – there it would have been a constant, as individuals had no influence over the contemporaneous actions of other individuals. Here, because changing the law changes the actions of others, such an influence does exist.

It can be shown that, indeed, despite the existence of backlash, as long as individuals are sufficiently forward-looking, in equilibrium they will vote for laws that are close to their bliss point in order to move society (and future generations of their family) toward the law. They will tolerate the short-term backlash in order to attain long-term convergence. If individuals are not forward-looking and care disproportionately about the present and near future, the law will not be changed in equilibrium.

Proposition A: For α sufficiently high, the existing law will be replaced in a majority vote with the new law, $L_{new} = b_{median}$.

Proof: First, note that once the law is chosen, the problem faced by individuals here is identical to that in the simplified case; individuals' actions do not affect the value of the third term. As such, the optimal action is identical.

Thus, in order to decide how to vote, each individual will assess their utility under the existing law. Denote by C_b and C_L the coefficients on $b_{i,t}$ and L, respectively, in the solution for the optimal action, and note from the proof of Proposition 1 that $C_b + C_L = 1$.

$$\begin{aligned}
u(x^*(L)) &= -(x^*_{i,t} - b_{i,t})^2 - \alpha(\gamma x^*_{i,t} + (1-\gamma)L - b_{i,t})^2 - \beta\textstyle\sum_j (x^*_{j,t} - b_{i,t})^2 \\
&= -\left(C_b b_{i,t} + C_L L - b_{i,t}\right)^2 - \alpha\left(\gamma C_b b_{i,t} + \gamma C_L L + (1-\gamma)L - b_{i,t}\right)^2 - \beta\textstyle\sum_j (C_b b_{j,t} + C_L L - b_{i,t})^2 \\
&= -\left((C_b - 1)b_{i,t} + C_L L\right)^2 - \alpha\left((-1+\gamma C_b)b_{i,t} + \left((1-\gamma) + \gamma C_L\right)L\right)^2 - \beta\textstyle\sum_j (C_b b_{j,t} + (1-C_b)L - b_{i,t})^2 \\
&= -\left((1-C_b)b_{i,t} - C_L L\right) - \alpha\left((1-\gamma C_b)b_{i,t} - (1-\gamma C_b)L\right)^2 - \beta\textstyle\sum_j \left((b_{i,t} - L) - C_b(b_{j,t} - L)\right)^2 \\
&= -\left(C_L b_{i,t} - C_L L\right) - \alpha\left((1-\gamma C_b)b_{i,t} - (1-\gamma C_b)L\right)^2 - \beta\textstyle\sum_j \left((b_{i,t} - L) - C_b(b_{j,t} - L)\right)^2.
\end{aligned}$$

They will compare this to their utility under the new law and will vote for the new law if it

provides higher utility. For the median voter (and all individuals further from the pre-existing law than the median voter),

$$
\begin{aligned}
& u(x^*(L_{new})) > u(x^*(L)) \\
\Leftrightarrow & -\left(C_L b_{i,t} - C_L L_{new}\right)^2 - \alpha\left((1-\gamma C_b)b_{i,t} - \left(1-\gamma C_b\right)L_{new}\right)^2 - \beta\sum\nolimits_j \left((b_{i,t} - L_{new}) - C_b(b_{j,t} - L_{new})\right)^2 \\
& > -\left(C_L b_{i,t} - C_L L\right)^2 - \alpha\left((1-\gamma C_b)b_{i,t} - \left(1-\gamma C_b\right)L\right)^2 - \beta\sum\nolimits_j \left((b_{i,t} - L) - C_b(b_{j,t} - L)\right)^2 \\
\Leftrightarrow & \left(C_L^2 + \alpha(1-\gamma C_b)^2\right)(b_{i,t} - L_{new})^2 + \beta\sum\nolimits_j \left((b_{i,t} - L_{new})^2 - 2C_b(b_{i,t} - L_{new})(b_{j,t} - L_{new}) + C_b^2(b_{j,t} - L_{new})^2\right) \\
& < \left(C_L^2 + \alpha(1-\gamma C_b)^2\right)(b_{i,t} - L)^2 + \beta\sum\nolimits_j \left((b_{i,t} - L)^2 - 2C_b(b_{i,t} - L)(b_{j,t} - L) + C_b^2(b_{j,t} - L)^2\right) \\
\Leftrightarrow & \left(C_L^2 + \alpha(1-\gamma C_b)^2\right)\left((b_{i,t} - L_{new})^2 - (b_{i,t} - L)^2\right) \\
& < \beta\sum\nolimits_j \left(\left((b_{i,t} - L) - C_b(b_{j,t} - L)\right)^2 - \left((b_{i,t} - L_{new}) - C_b(b_{j,t} - L_{new})\right)^2\right) \\
\Leftrightarrow & \alpha > \frac{\beta\sum\nolimits_j \left(\left((b_{i,t} - L) - C_b(b_{j,t} - L)\right)^2 - \left((b_{i,t} - L_{new}) - C_b(b_{j,t} - L_{new})\right)^2\right)}{\left((b_{i,t} - L_{new})^2 - (b_{i,t} - L)^2\right)(1-\gamma C_b)^2} - \frac{C_L^2}{(1-\gamma C_b)^2}.
\end{aligned}
$$

Note that the inequality is flipped in the last line above because $(b_{i,t} - L_{new})^2 - (b_{i,t} - L)^2 < 0$ for the median voter and all individuals further from the pre-existing law than the median voter. Since the above inequality specifies the value of α needed for a given individual to vote for the new law, there must exist some value of α satisfying the inequality for the majority of individuals – i.e., a value of α sufficient for the new law to pass.

A.3 Extension – Endogenized Laws, Backlash

Consider an extension to the simplified model whereby the actions individuals take influence what the law will be in the next period. Individuals also obtain disutility from the sheer existence of laws which are far from their own ideological preferences. That is,

$$u_{i,t}(x_{i,t}) = -(x_{i,t} - b_{i,t})^2 - \alpha(b_{i,t+1} - b_{i,t})^2 - \mu(L_{t+1} - b_{i,t})^2$$

Ideological preferences are formed as before, $b_{i,t+1} = \gamma x_{i,t} + (1-\gamma)L_t$, but the law is now determined similarly by a weighted average of the public's actions and the law itself in the preceding period: $L_{t+1} = \pi\bar{x}_t + (1-\pi)L_t$. Note that we could again think of the third term of the utility function as having been present in the baseline simplified model as well. There, however,

it would have been a constant since the law was exogenous. Similarly, individuals might care about the distance of the law from their preferences during the present generation, $(L_t - b_{i,t})^2$, but this too would be a constant and will thus fall out of the function during maximization.

As before, to maximize utility, we differentiate the utility function with respect to $x_{i,t}$.

$$\begin{aligned}
\partial u/\partial x_{i,t} &= -x_{i,t} + b_{i,t} + \alpha\gamma\left(-\gamma x_{i,t} + (1-\gamma)L_t + b_{i,t}\right) + (\mu\pi/N)\left(-\pi\bar{x}_t - (1-\pi)L_t + b_{i,t}\right) = 0 \\
&= b_{i,t}(1+\alpha\gamma+\mu\pi/N) - x_{i,t}(1+\alpha\gamma^2) - L_t\left(\alpha\gamma(1-\gamma) + (\mu\pi/N)(1-\pi)\right) - \bar{x}_t\,\mu\pi^2/N = 0 \\
&= b_{i,t}(1+\alpha\gamma+\mu\pi/N) - x_{i,t}(1+\alpha\gamma^2+\mu\pi^2/N^2) \\
&\quad - L_t\left(\alpha\gamma(1-\gamma) + (\mu\pi/N)(1-\pi)\right) - \textstyle\sum_{j\neq i} x_{j,t}\,\mu\pi^2/N^2 = 0 \\
\Rightarrow x_{i,t} &= \frac{1+\alpha\gamma+\mu\pi/N}{1+\alpha\gamma^2+\mu\pi^2/N^2} b_{i,t} - \frac{\alpha\gamma(1-\gamma)+(\mu\pi/N)(1-\pi)}{1+\alpha\gamma^2+\mu\pi^2/N^2} L_t - \frac{\mu\pi^2}{N^2}\sum_{j\neq i} x_{j,t}.
\end{aligned}$$

Observe that this is not yet a closed-form solution – $x_{j,t}$ remains on the right-hand-side.

$$\begin{aligned}
\frac{\partial x_{i,t}}{\partial x_{k,t}} &= -\frac{\mu\pi^2}{N^2} - \frac{\mu\pi^2}{N^2}\sum_{j\neq i,k}\frac{\partial x_{j,t}}{\partial x_{k,t}} \\
\frac{\partial x_{i,t}}{\partial x_{k,t}}\left(1+\mu\pi^2\frac{N+2}{N^2}\right) &= -\frac{\mu\pi^2}{N^2} \\
\frac{\partial x_{i,t}}{\partial x_{k,t}} &= \frac{-\mu\pi^2}{\mu\pi^2(N+2)+N^2} < 0.
\end{aligned}$$

Now it is possible to compute $\partial x^*_{i,t}/\partial L_t$ in order to make study the extent of the backlash,

$$\begin{aligned}
\frac{\partial x_{i,t}}{\partial L_t} &= -\frac{\alpha\gamma(1-\gamma)+(\mu\pi/N)(1-\pi)}{1+\alpha\gamma^2+\mu\pi^2/N^2} - \frac{\mu\pi^2}{N^2}\sum_{j\neq i}\left(\frac{\partial x_{j,t}}{\partial L_t} + \frac{\partial x_{j,t}}{\partial x_{i,t}}\frac{\partial x_{i,t}}{\partial L_t}\right) \\
\frac{\partial x_{i,t}}{\partial L_t} &= -\frac{\alpha\gamma(1-\gamma)+(\mu\pi/N)(1-\pi)}{1+\alpha\gamma^2+\mu\pi^2/N^2} - \frac{\mu\pi^2(N+1)}{N^2}\left(\frac{\partial x_{j,t}}{\partial L_t} - \frac{\mu\pi^2}{\mu\pi^2(N+2)+N^2}\frac{\partial x_{i,t}}{\partial L_t}\right) \\
\frac{\partial x_{i,t}}{\partial L_t}\left(\underbrace{1+\frac{\mu\pi^2(N+1)}{N^2}\left(1-\frac{\mu\pi^2}{\mu\pi^2(N+2)+N^2}\right)}_{\equiv B}\right) &= -\frac{\alpha\gamma(1-\gamma)+(\mu\pi/N)(1-\pi)}{1+\alpha\gamma^2+\mu\pi^2/N^2} \\
\frac{\partial x_{i,t}}{\partial L_t} &= -\frac{\alpha\gamma(1-\gamma)+(\mu/N)\pi(1-\pi)}{B\left(1+\alpha\gamma^2+\mu\pi^2/N^2\right)} < 0.
\end{aligned}$$

Here we see that the extent of the backlash is increasing in both the extent to which individuals care about their children's preferences, α, and the extent to which individuals care about the law

being consonant with their own preferences, μ. Importantly, however, it is decreasing in N. Because one individual in a large society can only contribute a small amount to changing the law, the ability to change the law contributes little to the inducement of backlash relative to the ability to influence one's children. For example, consider a case where $\pi = \gamma$ and $\alpha = \mu$ for simplicity. In such a case, in a society of one million, the inducement to backlash provided by the inculcation-of-children channel is one million times the inducement provided by the change-the-law channel.

Appendix B: The ERA – Further Outcomes and Robustness

The surpising richness of the 1970s-era American National Election Studies and other contemporaneous survey datasets on women's issues allow for additional exploration of the state ERAs and the backlash they induced. In the first part of this appendix, I conduct additional robustness checks on the main result – backlash in terms of male attitudes. In the second part, I explore additional material outcomes – male and female fertility preferences, marital discontent, and women's economic outcomes – presenting some evidence that the ERA backlash had effects along these margins as well.

B.1 Additional Robustness Checks

Figure B-2 modifies the main dynamic difference-in-differences specification. Instead of pooling all periods more than 10 years after ERA passage into one "long-run" indicator variable, it separates them into a multitude of indicators, the last of which ends 4 decades after ERA passage. This specification is responsive to the finding of Borusyak and Jaravel (2017) that pooling many periods into one "long-run" term – even in a dynamic difference-in-differences specification – may bias the remaining coefficients. In this context, however, I find that the effect size is virtually unchanged when one runs this alternative dynamic specification. The specification also reveals that the backlash is sustained for many decades.

Figure B-3 examines the effects of the ERA on *female* attitudes toward male/female equality. It can be seen that there is no evidence of sharp backlash on the part of women. It is worth noting that, prior to ERA passage, women in ERA-passing states are more sympathetic to the concept of male/female equality than women in non-ERA-passing states, further cementing the observation that, if anything, ERA-passing states were more liberal in their gender attitudes than non-ERA-passing states.

Table B-1 revisits the results using a standardized z-score version of the male/female equality question as the outcome variable, rather than an indicator variable. A higher z-score value represents more positive attitudes toward gender equality. This provides a more continuous outcome measure at the expense of less readily-interpretable coefficients. In any case,

the results are fundamentally the same. The introduction of a state ERA leads to a movement in male attitudes toward gender equality by one-third of a standard deviation in the conservative direction.

Table B-2 decomposes the effect into each individual point on the 7-point scale to provide a sense of how the distribution of attitudes toward male/female equality amongst men is changing. That is, are views becoming more polarized or is there a clear movement in one direction? The evidence is that the latter is the case, with views closer to equality becoming less common and views closer to inequality becoming more common. There appears to be an overall rightward shift of the distribution, consistent with the implications of the model.

Column (1) of Table B-3 re-runs the state-level specifications with a linear state time trend included in the regression. This is one way of controlling for the possibility that ERA-adopting states are on a more conservative trend than non-adopting states. Including this time trend does not ameliorate the effect, providing evidence that ERA adopters are not on a more conservative trajectory than non-adopters, which makes intuitive sense. Column (2) adds linear border-specific time trends to the border-county specification, yielding a similar result. Columns (3) and (4) add age, race, income quintile, and urbanism (urban, suburban, rural) fixed effects to the main and border-county specifications, respectively. The main result is strongly sustained. Another robustness check is proposed by Chaisemartin and d'Haultfoeuille (2020), who extend the argument of Borusyak and Jaravel (2017) further and argue that there may be circumstances under which even dynamic difference-in-differences specifications suffer from the same negative-weighting issue that may plague static difference-in-differences specifications. In particular, if the year-*t* dynamic treatment effects are actually heterogeneous across states (for at least some values of *t*), this could drive such a bias. I apply the procedure of Chaisemartin and d'Haultfoeille using their Stata package `did_multiplegt` and find that both the state-level and the border-county versions of my result are robust to it, as seen in columns (5) and (6) of Table B-3.

B.2 Additional Outcomes

There are many outcomes beyond attitudes and voting patterns that may be affected by backlash. Indeed, the model suggests that any ideologically-coded actions which have the capacity to signal one's ideological positions may manifest backlash. In the context of the ERAs, relationship patterns amongst husbands and wives seem like a particularly relevant outcome.

The National Fertility Survey asked women questions about their *preferred* number of children they'd ideally like to have and about the number of children they *expected* to have, after the joint decision is made by themselves and their husbands. Data from the National Fertility Survey is used in the regressions in Table B-4, and they reveal statistically-significant evidence of divergence. This suggests that, whereas women appear to move in the direction of preferring fewer children, men evidently move toward preferring more or are otherwise exerting more influence over their wives' decision-making.[17]

Given the evidence of divergence between men and women in various dimensions, one might wonder if tensions are increased in marriages as a consequence of the ERA. The GSS has asked questions on self-reported happiness and marital happiness since its inception. Table B-5 shows that, indeed, there is significant evidence of reduced marital happiness and overall happiness for married individuals – but no change in happiness for unmarried individuals. Figure B-4 shows the dynamic specification, which suggests that there are no significant pre-trends. The effect does not predate ERA passage; rather, it responds sharply afterward.

[17] The effect on the gap is statistically significant, but the effect on male and female preferences separately is not.

Appendix C: The Broader Women's Movement

The ERA was one of the primary pillars of the women's movement; however, it was not the only one. Large-scale entry of women into the labor force, election of female legislators, and legislation liberalizing access to contraceptives for unmarried women were three of its other biggest facets. Whereas the latter – like the ERA – was imposed in the form of a law, the former two were more bottom-up in nature. This provides the ideal setting for testing whether, indeed, laws play a unique role in generating backlash.

C.1 Female Labor-Force Entry

I study the effect of women's entry into the labor market using a shift-share instrument which exploits the fact that, in different industries, female employment has grown at different rates nationally, and prominence of different industries varies from area to area. Consequently, if industry *j* has rapid female employment growth from 1970 to 1990 and it makes up a high share of employment in county *i*, then county *i* will be treated with a large increase in female employment. Formally, the instrument is

$$\Delta S_i^{70,90} = \sum_j \pi_{ij}^{70} \Delta_{-i,j}^{70,90}$$

where π_{ij}^{70} represents the share of industry *j* in total employment of county *i* in 1970 and $\Delta_{-i,j}^{70,90}$ is the national growth of female EPOP (employment-to-population ratio) in industry *j* from 1970 to 1990, computed as a leave-one-out mean using data from the Decennial Census. I can then run the first-stage regression $\Delta E_i^{70,90} = \eta + \gamma \cdot \Delta S_i^{70,90} + u_i$, where $\Delta E_i^{70,90}$ represents the growth of female EPOP in county *i*. This yields a strong first-stage F-statistic of 149, allowing for a valid application of the second-stage regression,

$$\Delta Y_i^{70,90} = \alpha + \beta \cdot \Delta \hat{E}_i^{70,90} + X\gamma + \varepsilon_i$$

where $\Delta Y_i^{70,90}$ denotes the change in some outcome variable of interest in county *i* over the corresponding 1970 to 1990 period[18]. Focusing on the change in attitudes toward male/female

[18] I focus on 1970-1990 both because these were the two decades of most rapid female labor-force entry and because

equality and the change in Republican vote share as my outcomes of interest, I find no significant evidence of backlash in either domain, as can be seen in Table C-1. With regard to attitudes toward male/female equality, there is no statistically-significant correlation even in the OLS specification. Standard errors are very wide, but the point estimates suggest, if anything, the opposite of backlash. With regard to Republican vote share, to the extent that there is a correlation both in the OLS and shift-share IV regressions, the sign is again the opposite of backlash, with more female labor-force entry associated with reduced Republican vote shares.

C.2 Election of Female Legislators

I study the effect of women's election to political office using an electoral RDD on House of Representatives and State Legislature elections. I follow Gyourko and Ferreira (2014), who performed this exercise for mayors, comparing the outcomes generated by male and female mayors subsequent to elections that pitted a male and a female candidate against each other and identifying the effect off of the discontinuity at the 0% victory margin between the male and female candidates. Formally,

$$Y_{it} = \alpha + \beta \cdot FemaleLeg_{it} + f(x_{it}) + \varepsilon_{it} \qquad \forall x_{it} \in (c-h,\, c+h),$$

where Y_{it} is the outcome of interest in district i over some defined period subsequent to the election year t, x_{it} is the vote share for the female candidate, $FemaleLeg_{it} = \mathbf{1}\{x_{it} > c\}$, and h is the bandwidth around the cutoff c. Again, focusing on attitudes toward male/female equality and the Republican vote share in the subsequent election as my outcomes of interest, I find no significant evidence of backlash, as can be seen in Table C-2, column (7). If I instead study effects on female candidates in the subsequent election, I actually find some evidence of *increased* future female vote shares – the opposite of backlash.

C.3 Liberalization of Female Access to Contraceptives

I study the liberalization of contraception access to unmarried women using the difference-in-differences framework applied throughout most of this paper. Like the ERA, this is a pillar of

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one of the key outcomes of interest – attitudes toward male/female equality – is not available prior to the 1970s.

the women's movement operationalized through the law. Its effects on fertility patterns and female labor-market decisions were studied in detail by Goldin and Katz (2002). Figure C-1 displays the results of a dynamic specification analagous to the one run in the context of the ERA and reveals that, just like the ERA, this law generated a sharp and significant backlash in male attitudes. Thus there is indeed evidence that laws play a unique role in generating backlash, distinct from the more bottom-up components of the women's movement that were not actualized through legislation.

Appendix Tables & Figures

Table B-1: Static Specifications – ERA (Z-Score Outcomes)

Outcome: Gender Role Attitudes (z-score)	(1) State Diff-in-Diff	(2) State Diff-in-Diff	(3) State Diff-in-Diff	(4) Border Dis-continuity	(5) Border Dis-continuity
Sex:	Both	Male	Female	Male	Female
ERA Indicator	-0.115**	-0.301***	0.027	-0.354***	-0.069
	(0.051)	(0.088)	(0.041)	(0.087)	(0.080)
Year FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
State-by-Border FEs	No	No	No	Yes	Yes
Individuals in Sample	All	All	All	Border Residents	Border Residents
Years of Data	1972-1988	1972-1988	1972-1988	1972-1988	1972-1988
Clustering	State	State	State	State	State
Observations	22,208	9757	12,451	3535	4469

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level
* Denotes significance at the 10% level. Regressions in this table are the analogues of regressions in Table 2, albeit with the ANES gender role attitude variable converted into a z-score. A larger value indicates more positive attitudes toward gender equality.

Table B-2: Gender Equality Scale Point-by-Point ERA Regressions

Outcome: Point-by-point indicators for gender equality position	(1) 1	(2) 2	(3) 3	(4) 4	(5) 5	(6) 6	(7) 7
Sex:	Male	Male	Male	Male	Male	Male	Male
ERA Indicator	-0.140***	-0.002	-0.008	-0.007	0.023	0.021	0.082***
	(0.038)	(0.028)	(0.018)	(0.023)	(0.016)	(0.013)	(0.021)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	State	State
Observations	10,448	10,448	10,448	10,448	10,448	10,448	10,448

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Regressions in this table are the analog of the main specification for males – column (2) in Table 2 – but with the regressions run point-by-point along the 7-point gender role attitude variable. An indicator variable is generated for each point; thus the coefficient can be interpreted as the change in the share of men who are at that point in the gender role attitude distribution. "1" corresponds to "men and women should have an equal role in running business, industry, and government." "7" corresponds to "a woman's place is the home."

Table B-3: Additional Robustness

	(1) State Time Trend	(2) State Time Trend	(3) Additional FEs	(4) Additional FEs	(5) Chaisemartin-D'Hault.	(6) Chaisemartin-D'Hault.
Sex:	Male	Male	Male	Male	Male	Male
ERA Indicator	-0.133**	-0.150**	-0.138***	-0.173***	-0.194**	-0.263**
	(0.052)	(0.069)	(0.049)	(0.044)	(0.090)	(0.131)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Border FEs	No	Yes	No	Yes	No	Yes
Individuals in Sample	All	Border Residents	All	Border Residents	All	Border Residents
Years of Data	1972-1998	1972-1998	1972-1998	1972-1998	1972-1998	1972-1998
Clustering	State	State	State	State	State	State
Observations	10,448	3780	9650	3449	10,448	3780

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Column (1) adds state-specific time trends to the baseline specification; column (2) adds border-specific time trends to the border-county specification. Columns (3) and (4) add age, race, income quintile, and urban/suburban/rural FEs to the baseline and border-county specification, respectively. Columns (5) and (6) apply the approach of Chaisemartin and d'Haultfoeuille (2020) using their `did_multiplegt` package in Stata.

Table B-4: National Fertility Survey

	(1)	(2)	(3)
Outcome:	Ideal # Children, Self	Expected # Children	Ideal # Children Gap
Sex:	Female	Female	Female
ERA Indicator	-0.134	0.140	0.287***
	(0.087)	(0.111)	(0.092)
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes
Years of Data	1965, 1975	1965, 1975	1965, 1975
Clustering	State	State	State
Observations	8,983	9,002	8,967

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Regressions use 1965 and 1975 waves of the National Fertility Survey. 1970 wave lacks publicly-available state codes. The outcome for column (1) is women's responses to a question about the number of children they'd ideally like to have, if it was up to them. The outcome for column (2) is women's responses to a question about the number of children they *expect* to have, a joint decision made by them and their husbands. The outcome for column (3) is the gap between these two responses.

Table B-5: Happiness

	(1) Satisfaction w/ Family Life (z-score)	(2) Marital Happiness (z-score)	(3) Happiness (z-score)	(4) Happiness, Married People (z-score)	(5) Happiness, Single People (z-score)
Sex:	Both	Both	Both	Both	Both
ERA Indicator	-0.161**	-0.104***	-0.101***	-0.144***	-0.011
	(0.063)	(0.034)	(0.023)	(0.018)	(0.078)
Year FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
Individuals in Sample	All	All	All	All	All
Years of Data	1973-2016	1973-2016	1973-2016	1973-2016	1973-2016
Clustering	State	State	State	State	State
Observations	24,070	29,566	56,104	26,619	11,864

Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Regressions use GSS data on self-reported happiness.

Table C-1: Female Labor-Force Entry Shift-Share

	(1) OLS, Attitude toward Gender Equality	(2) IV, Attitude toward Gender Equality	(3) OLS, Rep Vote Share	(4) IV, Rep Vote Share
Sex:	Male	Male	Both	Both
ΔFemaleEPOP	2.971	19.215	-1.124***	-4.982***
	(2.146)	(37.108)	(0.321)	(1.248)
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Data Source	ANES	ANES	Electoral Atlas	Electoral Atlas
Years of Data	1972-1998	1972-1998	1968-1992	1968-1992
Clustering	State	State	County	County
Observations	76	76	3,106	3,106

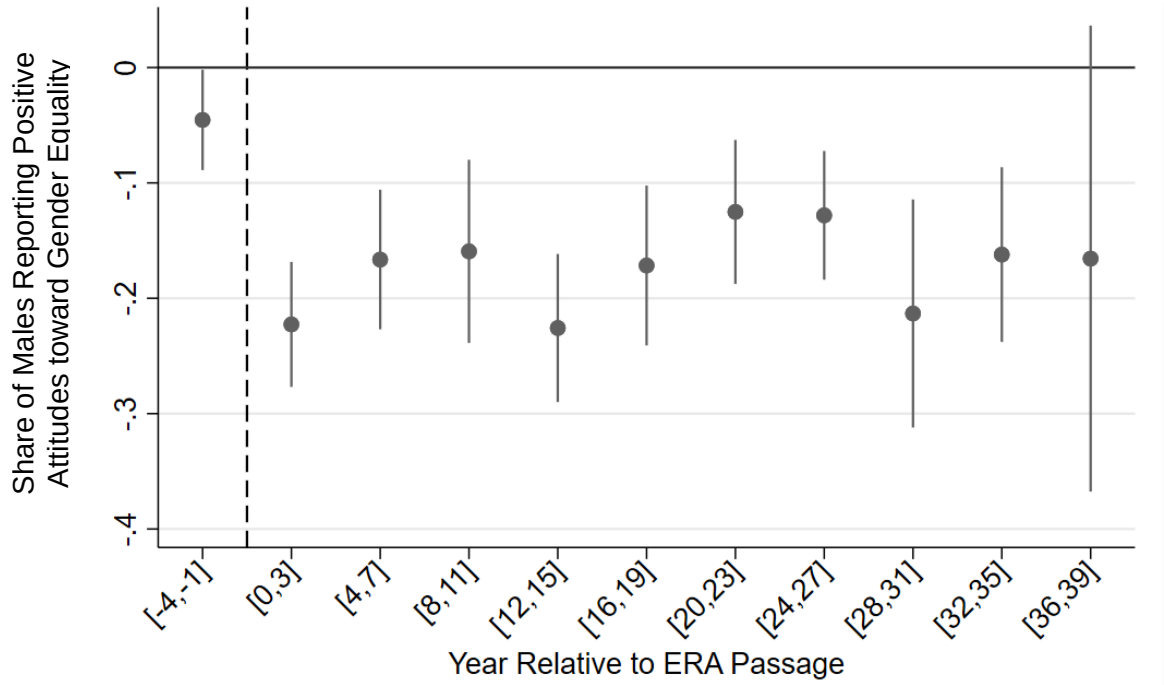
Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Columns (1) and (2) use the indicator for positive attitudes toward gender equality generated from the ANES data as their outcome variable. Columns (3) and (4) use data on official presidential election returns from Dave Leip's electoral atlas as their outcome variable. OLS specifications in columns (1) and (3) regress the outcome directly on the change in the female employment-to-population ratio (EPOP). IV specifications in columns (2) and (4) instrument the latter with the shift-share instrument.

Table C-2: Female Legislators RDD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female Vote Share, HoR	Female Vote Share, State Leg.	Female Vote Share, HoR (Non-Incumbent)	Female Vote Share, State Leg. (Non-Incumbent)	Republican Vote Share, HoR	Republican Vote Share, State Leg.	Attitude toward Gender Equality, HoR
Sex:	Both	Both	Both	Both	Both	Both	Male
FemaleVictory	0.100***	0.027**	0.065	0.005	-0.007	0.022	0.061
	(0.037)	(0.012)	(0.059)	(0.015)	(0.042)	(0.015)	(0.078)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Data Source	MIT Election Lab	Klarner et al.	MIT Election Lab	Klarner et al.	MIT Election Lab	Klarner et al.	ANES
Years of Data	1976-2018	1967-2010	1976-2018	1967-2010	1976-2018	1967-2010	1972-2008
Bandwidth	5%	5%	5%	5%	5%	5%	5%
Clustering	District	District	District	District	District	District	District
Observations	1,928	24,166	1,462	19,176	1,912	23,873	3,565

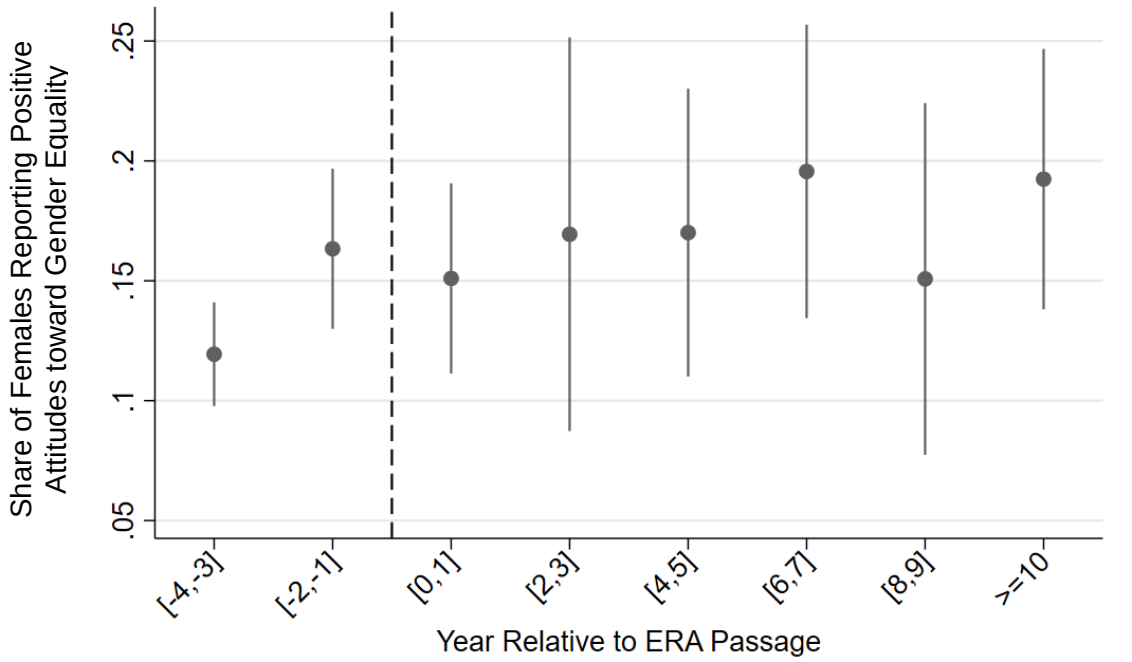
Note: *** Denotes significance at the 1% level; ** Denotes significance at the 5% level; * Denotes significance at the 10% level. Columns (1) and (2) use the vote share of female candidates as their outcome. Columns (3) and (4) use the vote share of non-incumbent female candidates as their outcome. Columns (5) and (6) use Republican vote share as an outcome. Column (7) uses the indicator for positive attitudes toward gender equality generated from the ANES data as their outcome variable. The odd-numbered columns study close House of Representatives elections using data from the MIT election lab. The even-numbered columns study close state legislative elections using data from Klarner et al. (2013). The fact that the ANES does not contain state legislative district geocodes makes it infeasible to run a specification studying the effect of close state legislative elections on ANES gender role attitudes.

Figure B-2: Dynamic Difference-in-Differences – ERA Effects, Long Horizon



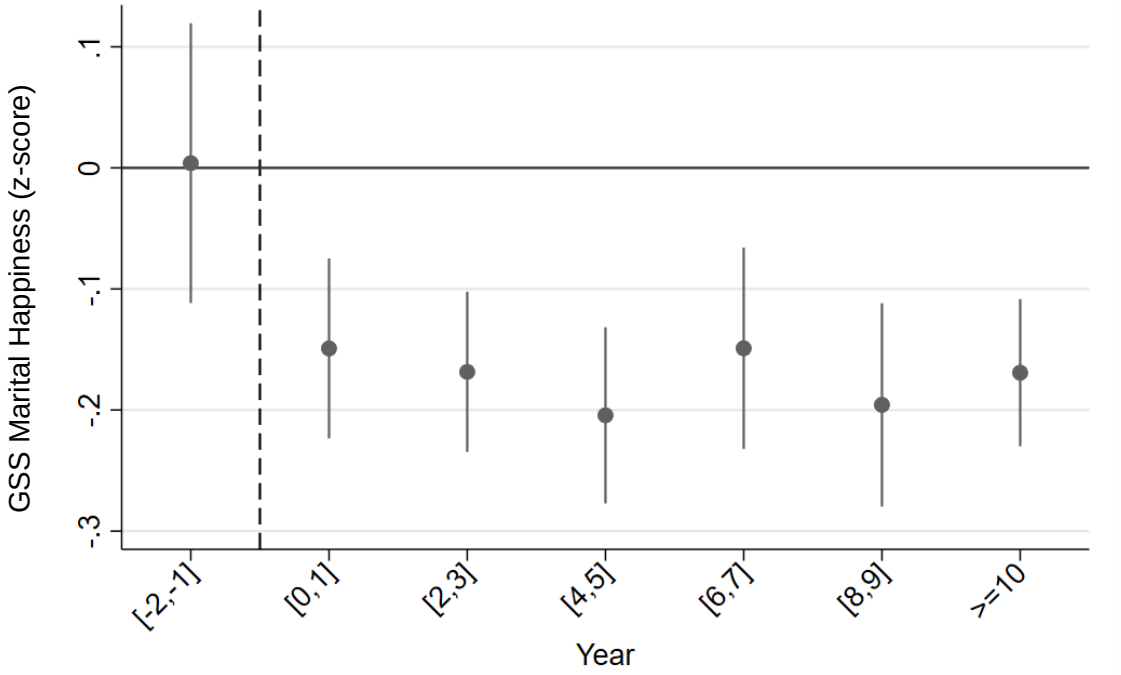
Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -4.

Figure B-3: Dynamic Difference-in-Differences – ERA Effects on Female Attitudes



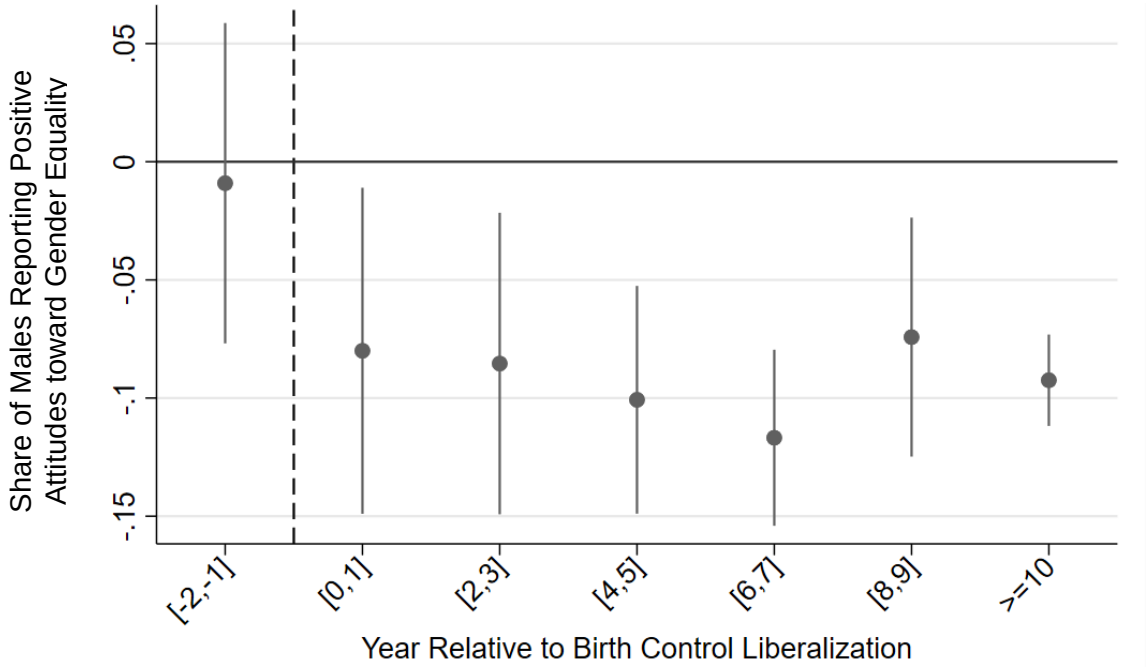
Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -4.

Figure B-4: Dynamic Difference-in-Differences – ERA Effects on Marital Happiness



Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -4.

Figure C-1: Dynamic Difference-in-Differences – Birth Control Pill Legislation Effects on Male Attitudes



Note: Year 0 corresponds to the year the state ERA takes effect. Omitted category is years < -2.