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RESEARCH NOTE

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Indirect restrictions demobilize supporters of abortion rights

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Abstract

State governments have created a multitude of indirect restrictions on abortion in the decades since *Roe v. Wade*. Here we test whether indirect restrictions demobilize abortion supporters relative to direct restrictions. We draw on research from moral psychology showing that people judge indirect offenses as less morally wrong than direct offenses, holding constant the consequences of the offenses. In two experiments, pro-choice participants answered how much they oppose a banning policy (a direct restriction), a defunding policy (an indirect restriction), or an excluding policy (the same as defunding but framed as more direct). In both experiments, pro-choice participants were less opposed to defunding than banning or excluding, even when the number of women affected was held constant. These results support the hypothesis that indirect restrictions can demobilize political opponents.

KEYWORDS

abortion, abortion bans, experimental political science, indirect policies, moral psychology

INTRODUCTION

In recent decades, conservative legislators have designed a motley array of abortion restrictions to circumvent the rights previously protected by the Supreme Court under *Roe v. Wade*. These restrictions were designed to evade the Supreme Court's protection, but was this their only purpose? We examine whether indirect restrictions could also function to reduce opposition from political opponents. To motivate this hypothesis, we draw on research from moral psychology showing that people judge indirect offenses as less morally wrong than direct offenses, even when the consequences are held constant (e.g., Cushman et al., 2006; DeScioli et al., 2012; Mikhail, 2007). Accordingly, abortion supporters may be less morally outraged by policies that restrict abortion indirectly, compared to more direct restrictions. If so, they will be less motivated to mobilize against indirect restrictions. In two experiments, we study participants' judgments of defunding policies, a form of indirect restriction, compared to a direct ban on abortion.

While abortion rights were protected under *Roe*, state governments enacted hundreds of laws to restrict abortion. In a comprehensive study, Kreitzer (2015) found that between 1973 and 2013, states adopted over 700 abortion restrictions of 29 types. That figure is seven times greater than the number

of state policies adopted to support abortion rights, which was just over 100. The barrage of restrictions includes bans on abortion after points in gestation such as 20 weeks; prohibitions against public funding of abortion; prohibitions of clinics that provide abortion (even if the abortions were privately funded); prohibitions of public and private insurance that covers abortion; mandatory waiting periods, counseling, and parental consent to deter patients; and burdensome regulations of providers to deter doctors and clinics.

By these various means, abortion opponents sought to circumvent the right to abortion under *Roe v. Wade*. Restrictions multiplied most aggressively in conservative states where abortion opponents outnumbered supporters and controlled the state legislature (Bentele et al., 2018; Camobreco & Barnello, 2008; Kreitzer, 2015; Lax & Phillips, 2012; Medoff et al., 2011). Moreover, each time the Supreme Court weakened the protection with decisions like Webster (1989) and Casey (1992), another wave of restrictions followed (Kreitzer, 2015).

The obvious goal of many restrictions was to exploit loopholes in the law and block abortion by other means. However, we propose that indirect restrictions could serve another purpose for conservative legislators: reducing opposition from abortion supporters. Abortion supporters might perceive indirect restrictions—like defunding, requiring parental consent, and excessive regulations for clinics—as less invasive to abortion rights than direct bans, even if the policies have the same effect of blocking access to abortion. If so, then conservative legislators could use indirect restrictions as a tactic to satisfy abortion opponents without drawing too much opposition from abortion supporters. This tactic could be critical in competitive elections. Even if conservative legislators considered only Republican voters, a substantial fraction of Republicans support abortion rights: 38% nationally in a 2022 survey (Pew Research, 2022). Hence, conservative legislators could benefit by preventing abortion in roundabout ways that minimize outrage from their opponents.

Abortion laws and morality policy

Political scientists have long considered abortion laws to be a kind of morality policy, policies that are thought to provoke moral judgment and to be responsive to public opinion (Mooney & Lee, 1995; Mooney & Schuldt, 2008). Researchers argue that morality policies are more responsive to public opinion than other policies because citizens' moral judgments of the issue make them hold strong, uncompromising opinions and then hold legislators accountable to those opinions in elections. Thus, legislators need to be responsive to the public on these issues to win votes, making morality policies more aligned with public opinion than ordinary policies.

Particularly, researchers have found that abortion policies are responsive to public opinion. A number of researchers have examined the expansion of state restrictions on abortion since *Roe* (Bentele et al., 2018; Camobreco & Barnello, 2008; Kreitzer, 2015; Lax & Phillips, 2012; Medoff et al., 2011). They generally find that abortion restrictions are responsive to public opinion: States where the public is more opposed to legal abortion have more restrictions. This research also examines a number of other factors, including party control of the legislature and governor's office, the gender composition of legislators, and citizens' religion and political ideology.

While this literature assumes abortion is a moral issue, research on moral conviction has measured and confirmed that most citizens perceive abortion policy as a moral issue (Skitka et al., 2021; Skitka & Morgan, 2014). In these studies, participants commonly answer how much their opinion on the legality of abortion is "a reflection of your core moral beliefs about right and wrong," in addition to similar wordings, on a scale from "not at all" to "very much." As expected, participants generally feel morally convicted about abortion policy. For example, in a national sample of Americans, participants' moral conviction about abortion was on average just over the midpoint of the scale, corresponding with "moderately," and participants who chose abortion as the most important of 10 issues were on average near the maximum of the scale, "very much" (Ryan, 2014; see Figure 2 on p. 389). In another dataset from a national sample, we can break it down by party, and both Democrats

and Republicans were nearest to the midpoint, "moderately," on average, in their moral conviction about abortion (Vaisey, 2013).

Although abortion policies are morally charged and generally responsive to public opinion, there have been some mixed findings. For instance, one study did not find a correlation between public opinion and a state's targeted regulations of abortion providers, known as TRAP laws (Medoff et al., 2011). Kreitzer (2015) suggested that the mixed results occur because responsiveness depends on the details of the policy and how these details engage moral judgment: "Some types of abortion policy may be more easily defined outside of the morality framework, such as public funding of abortions…and TRAP policies" (p. 45).

Kreitzer's suggestion follows the idea that morality policies are a matter of perception and framing. As Mooney and Schuldt (2008) put it, "how an issue is framed, rather than its intrinsic content, leads to its classification as a morality policy" (p. 201). Thus, some abortion restrictions may be perceived as less morally wrong than others, and if so those policies may be less strongly opposed by abortion supporters.

Moral framing of policies

This idea is reinforced by the literature on moral framing (reviewed in Feinberg & Willer, 2019; Tetlock, 2003). In general, the framing of an issue refers to how it is described, in contrast to what is thought to be the consequences, payoffs, or substance of the issue. In the classic disease problem, for example, participants were much more likely to favor a policy that would save 200 out of 600 lives than a policy described as leading to 400 deaths out of 600, since the first description emphasizes the gains while the second emphasizes the losses (Tversky & Kahneman, 1981). Gains versus losses is only one kind of framing, and it may or may not be morally relevant. Saving lives is a moral issue, but this framing can also apply to profits, personal safety, and other decisions outside of morality.

Moral framing refers particularly to frames that affect our moral judgments, making us judge an action as more wrong, less wrong, or morally neutral. These frames can influence our moral reactions by changing any of the elements that contribute to moral judgment, including the consequences, actions, intentions, causation, and relevant actors and victims. In the disease problem, the frame altered the consequences in lives. Another moral framing concerns which morals and considerations are at stake. An issue may be framed as a moral concern against a secular concern, comprising a taboo tradeoff, or as a difficult choice between competing moral concerns, a tragic tradeoff (Tetlock, 2003). For instance, participants were more outraged by a hospital administrator who let a boy die to save a million dollars, a taboo tradeoff, than when the same decision was described as having the purpose of using the money to save other patients' lives, a tragic tradeoff (Tetlock, 2003).

Another kind of moral framing alters which morals apply to the policy and its goals. Much of this research focuses on moral principles from moral foundations theory: care, fairness, loyalty, authority, and sanctity (Haidt, 2012). Liberals and conservatives tend to weigh these principles differently, and reframing these issues to speak to someone's own moral concerns can sometimes persuade them. For example, conservatives were more likely to support policies to protect the environment after reading arguments that emphasized the sanctity of nature and that pollution is disgusting and impure (Feinberg & Willer, 2019).

There are many other kinds of moral framing that can make an action appear more wrong or more acceptable. Any aspect of moral psychology can generate a frame. To give another well-known example, journalists can use the active voice, such as "a police officer shot a man," to emphasize the offender's action, or they can use the passive voice, such as "a man was shot," to omit the offender from a headline and blunt the resulting outrage. The choice to state the offender or omit them with passive voice is a moral framing that encourages or reduces moral outrage, respectively. Similarly, authors may use euphemisms such as "enhanced interrogation" instead of familiar moral offenses such as "torture" to obscure wrongdoing in bland or rosy terms and reduce moral condemnation (Orwell, 1946). Oppositely, authors

may use dysphemisms such as calling taxes "theft" to liken them to the familiar offense of stealing and increase moral condemnation.

As we turn to next, moral judgment also depends on the intentions and causation of an action, apart from its consequences. Moral framing can alter perceptions of these factors. The frame may alter only the wording, analogous to passive voice versus active voice. Or the framing may involve different intentions and causes that have the same effect, such as the environmental policy (discussed above) cast with different intentions, purity versus care. Our experiments will examine both the wording and causation of indirect restrictions on abortion.

The moral psychology of indirect offenses

To study how people react to indirect restrictions of abortion rights, we look to previous research from moral psychology about indirect offenses. Several strands of research find that people judge indirect offenses as less wrong than direct offenses, even when they have the same intentions and consequences. We review this research and then use it to propose that people may judge indirect policies with similar leniency compared to more direct restrictions of their rights.

First, dozens of experiments have found that people judge wrongful omissions less harshly than wrongful actions, even when the consequences are the same (Anderson, 2003; Cushman et al., 2006; DeScioli et al., 2011; DeScioli et al., 2012; Ritov & Baron, 1999). For example, in an economic game, participants punished a player who took someone's money by omission, allowing it to be transferred to themselves, with fines that were 30% less than a player who took the money directly by commission (DeScioli et al., 2011).

Second, numerous experiments have found that people judge offenses as less wrong when they occur as a byproduct of another goal compared to when the offense is the means to the goal (Cushman et al., 2006; DeScioli et al., 2012; Mikhail, 2007; Royzman & Baron, 2002). These judgments follow the principle of double effect from moral philosophy, first described by Thomas Aquinas in the thirteenth century. In one experiment, for instance, participants judged whether it is morally acceptable to cause one person's death in order to save five people from an oncoming trolley. Approximately 75% of participants said it is acceptable to flip a switch to redirect the trolley to a sidetrack where it would kill one person indirectly as a byproduct, whereas only 25% said it was acceptable to push one person in front of the trolley, killing them directly as a means to save others (Mikhail, 2007). In a similar dilemma, participants judged that to save nine people from a bomb, it is more acceptable to throw the bomb onto a patio with one person, killing them as a byproduct, than to throw one person on the bomb, killing them as means (Waldmann & Dieterich, 2007).

Third, research finds that people blame a leader less for decisions with bad results when the leader decided indirectly by delegating it to others compared to making the decision directly. In one experiment, participants judged whether a mayor deserved blame for deciding not to repair walls that then failed in a flood. Only 32% of participants blamed the mayor who held a vote to decide, whereas 87% blamed the mayor who decided directly (DeScioli & Bokemper, 2014).

Fourth, previous research argues that moral judgment draws on a fundamental cognitive distinction between direct and indirect causation, a distinction that also shapes people's verb choices in language (De Freitas et al., 2017). In language, speakers typically express direct causation with a single transitive verb, whereas speakers express indirect causation with an intransitive verb in a roundabout construction (Pinker, 2007; Wolff, 2003, 2007). For example, a speaker might say Alice killed the caterpillar when she stepped on it directly, encoding direct causation with the transitive verb kill. In contrast, a speaker might say Alice caused the caterpillar to die when she brushed it to the ground where someone else stepped on it, encoding indirect causation with a roundabout construction formed from multiple verbs: the generic verb cause and the intransitive verb die. Critically, people's moral judgments draw on the same cognitive distinction, which is also reflected in the language people use to justify their moral judgments. In a series of experiments, participants explained their judgments about different moral dilemmas. Tellingly,

participants' verb constructions differed depending on their moral judgment. Those who condemned sacrificing one person to save five people in the trolley problem chose the direct verb kill to describe the behavior, while those who approved of the sacrifice chose indirect constructions like cause to die (De Freitas et al., 2017).

Altogether, these strands of research show that people judge different forms of indirect offenses—including omissions, byproducts, and delegated decisions—as less wrong and blameworthy than direct offenses. As we previously discussed, these effects can be considered forms of moral framing that derive from the directness of an offense.

Although these studies focused on human actions, we expect that people also judge indirect policies with similar leniency. People may be less outraged by policies that cross their moral boundaries indirectly compared to policies that violate their morals directly. This idea draws on a parallel between human actions and policies: Just as people judge human actions as morally right or wrong, they also judge policies and laws as right or wrong (DeScioli, 2023). For example, people generally think that killing someone without reason is morally wrong, and they also think that a policy that permits needless killing is morally wrong. So both a human action and a policy can be objects of moral judgment, judged right or wrong.

One reason for the parallel might be that a policy itself ultimately derives from human actions: a policymaker's creation of the policy, as well as any enforcers' actions to uphold the policy. Hence, a policy itself represents the human actions that create and sustain it and may therefore be subject to moral judgment in the same way and by the same metrics. As we judge human actions by their intentions, causation, and effects, we can judge policies by these considerations in the policymakers and enforcers behind them.

The present experiments

We apply the moral psychology of indirect offenses to study how people react to abortion restrictions. Many of the common restrictions impede abortion indirectly, such as policies that prohibit funding, require counseling, and impose undue regulations on clinics. We hypothesize that indirect restrictions demobilize abortion supporters by provoking less opposition than direct bans.

We test the demobilization hypothesis in two experiments by studying how pro-choice participants judge a policy that defunds abortion (an indirect restriction) compared to an outright ban (a direct restriction). Defunding policies aim to restrict abortion indirectly by withholding public funds. Defunding policies can be understood as a kind of omission—an omission of funding—in contrast to an action such as banning abortion directly.

We also test whether defunding provokes more opposition when it is framed as more direct. To do so, we examine a third policy, an excluding policy that is the same as defunding except that it is framed as actively excluding pregnant women from public health care that they would otherwise be entitled to. The transitive verb exclude expresses an action with direct causation, in contrast to a passive omission. Thus, we test whether pro-choice participants are more opposed to a restriction framed as an exclusion policy compared to a defunding policy.

EXPERIMENT 1

Methods

We recruited a convenience sample of participants from the United States using Amazon's Mechanical Turk (Berinsky et al., 2012). We conducted the experiment in the fall of 2015, during a surge in political efforts to defund abortion. In July of 2015, abortion opponents posted undercover videos of providers from Planned Parenthood and accused the organization of selling fetal tissue for profit.

The controversy spurred calls to defund Planned Parenthood. In September 2015, the U.S. House of Representatives passed the Defund Planned Parenthood Act, which was later blocked by the Senate in December. Thus, we conducted the experiment at a time when defunding policies were at the center of political attention.

We planned in advance to recruit participants until we had approximately 100 pro-choice participants per condition, providing enough observations to test our hypotheses with enough power to detect medium effect sizes. Specifically, a *t*-test comparing two means with 100 observations per condition can detect a minimum effect size of Cohen's $d = 0.38$ with 80% power (alpha = 0.05).

Participants completed a short survey for a small payment (50 cents). As planned in advance, we excluded from analysis participants who did not complete the study ($n = 32$) or did not correctly answer the comprehension question ($n = 8$), yielding a sample of 476 participants (49.7% female; age: $M = 34.6$, $SD = 10.6$).[1] Participants were randomly assigned to one of three conditions: banning, defunding, or excluding.

Participants first read "a fictional scenario" about a character who wants an abortion:

> Alice is a 25-year-old single woman who recently got pregnant. She works a low-paying job with income below the poverty line. Alice receives money from the government to help pay for housing and medical care. She wants an abortion because she does not feel she can afford to support a child.

The scenario centers on a character to make the issue concrete and relatable. We described the character to match common situations in which women seek abortions (Jerman et al., 2016).

Participants were then asked to consider an abortion restriction, which differed across three conditions. In the banning condition, participants read, "Do you think the law should ban abortions? That is, should the law prevent women like Alice from receiving abortions?" In the defunding condition, participants read, "Do you think the law should ban the government from paying for abortions? That is, should the law prevent women like Alice from receiving government money to pay for abortions?" In the excluding condition, participants read, "Do you think the law should exclude abortions from government health care programs for low-income women? That is, should the law exclude women like Alice from receiving abortions through government health care programs?"

Participants then judged the restriction by choosing how much they support or oppose the policy on a sliding scale from strongly support to strongly oppose (coded −5 to +5) with neutral at the midpoint. They also indicated what forms of political participation they would be willing to undertake to advocate for their views. Finally, participants answered questions about demographics and politics, including whether their position on abortion is pro-choice ($n = 323$), anti-abortion ($n = 121$), or undecided ($n = 32$). We excluded undecided participants from the analysis since they were small in number and not the focus of the study.

As we previously explained, the demobilization hypothesis predicts that pro-choice participants will oppose defunding, an indirect restriction, less than banning, a direct restriction. Additionally, the exclusion framing hypothesis predicts that participants will oppose the excluding policy more than the defunding policy. We did not have specific hypotheses about the anti-abortion participants since they generally support restrictions, but we include their results for reference.

Results and discussion

Figure 1 shows the results. Our main hypothesis is about pro-choice participants. Pro-choice participants were less opposed to defunding than banning, $t\ (211) = 2.89$, $p < 0.01$, $d = 0.40$. This finding supports the demobilization hypothesis that people oppose defunding, an indirect restriction, less than banning, a direct restriction. Moreover, when the same defunding policy was framed more directly as exclusion, participants opposed exclusion more than defunding, $t\ (221) = 1.99$, $p < 0.05$, $d = 0.27$,

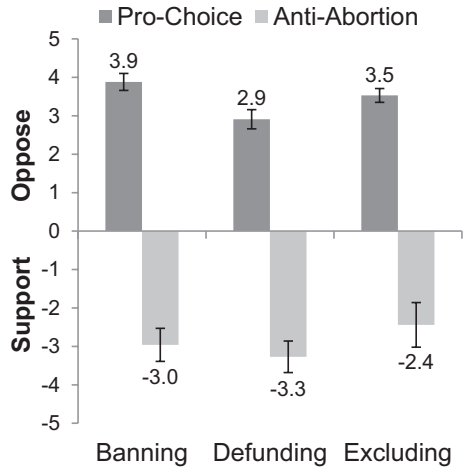

FIGURE 1 Participants' judgments of abortion restrictions by abortion stance and policy type, Experiment 1. Error bars are standard errors. The sample sizes for the pro-choice group were: banning $n = 104$, defunding $n = 117$, and excluding $n = 111$. For the anti-abortion group, they were: banning $n = 43$, defunding $n = 45$, and excluding $n = 33$.

supporting the exclusion framing hypothesis. Participants' opposition to exclusion did not statistically differ from banning, $t\ (208) = 1.25$, $p = 0.21$, $d = 0.17$, indicating that the exclusion framing eliminated the demobilization observed for the defunding policy.

Among anti-abortion participants, we find no significant differences between conditions: defunding vs. banning, $t\ (86) = 0.52$, $p = 0.60$, $d = 0.11$; defunding vs. excluding, $t\ (76) = 1.20$, $p = 0.23$, $d = 0.27$; excluding vs. banning, $t\ (74) = 0.73$, $p = 0.46$, $d = 0.17$. We note, however, that the samples of anti-abortion participants are smaller because they were less common on Mturk and we did not have a quota since they were not the focus of the study. Last, on the participation item, we did not find differences across conditions for participants who were pro-choice or anti-abortion (Appendix, Tables A1 and A2).

In sum, we find that pro-choice supporters were less opposed to defunding policies than banning policies. They were also more opposed to the excluding policy than the defunding policy, indicating that framing can increase perceptions of directness.

These results support the demobilization hypothesis. But the difference between banning and defunding has at least two interpretations. The first is that it is mainly the indirectness of defunding that reduces opposition, which is further supported by greater opposition to exclusion than defunding. The second is that participants perceived defunding as less limiting than banning, so they were less opposed to it. This second possibility may or may not be related to directness. People could judge the policy mainly by assessing how many people are likely to be restricted, or they could perceive indirect restrictions as less limiting absent further information. We next address these interpretations by holding constant the number of women expected to be limited by the restrictions.

EXPERIMENT 2

In Experiment 1, pro-choice participants opposed defunding less than banning, but one alternative interpretation is that participants assessed that defunding would restrict fewer women than banning. We now address this alternative interpretation by adding a clarifying statement to the policy question. The statement provides an estimate that the policy would prevent 1 million women per year from receiving abortion services. With this statement, we hold constant across conditions participants' perceptions of the consequences of the law for women's access to abortion services.

Methods

We recruited participants from the United States on Mturk (45% female; age: $M = 33$, SD = 11 years). The experiment was conducted in February of 2016. As before, we planned in advance to recruit participants until we had approximately 100 pro-choice participants per condition. This sample size allows us to detect an effect size of $d = 0.38$ with 80% power (alpha = 0.05). As planned in advance, we excluded participants who did not complete the study ($n = 25$) or failed the comprehension question ($n = 29$), yielding a final sample of 456 participants.[ii]

The methods are the same as Experiment 1, except that we added a clarifying statement after describing each abortion restriction: "If this law was applied throughout the U.S., we estimate that each year about 1 million women who want abortions would be unable to receive abortion services from a medical professional."

In addition, we refined the participation question by adding the participant's position on the abortion policy (banning, defunding, or excluding) to the text of the question to specify which policy and position they would be advocating for. The participant's position (support or oppose) and the policy (banning, defunding, or excluding) were piped into the question about political participation. Participants answered whether they would sign petitions, write letters to politicians, donate money to an organization or campaign, volunteer time for an organization or campaign, vote for politicians based on their abortion views, and join political protests. Participants rated each activity on a sliding scale from 0 (*not at all willing*) to 10 (*extremely willing*). We also added a sentence to clarify that we would not actually ask them to take these actions so they would not mistake the question for a solicitation.

Results and discussion

Figure 2 shows the results. Again, pro-choice participants opposed defunding less than banning, $t(195) = 4.31$, $p < 0.001$, $d = 0.61$. This finding further supports the demobilization hypothesis, showing the same effect as Experiment 1, this time when the policy effects were held constant in the text of the scenarios.

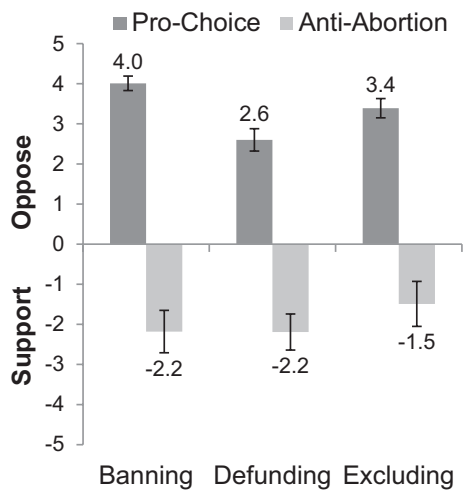

FIGURE 2 Participants' judgments of abortion restrictions by abortion stance and policy type, Experiment 2. Error bars are standard errors. The sample sizes for the pro-choice group were: banning $n = 103$, defunding $n = 94$, and excluding $n = 103$. For the anti-abortion group, they were: banning $n = 40$, defunding $n = 49$, and excluding $n = 39$.

Pro-choice participants also opposed excluding more than defunding, t (195) = 2.17, p = 0.031, d = 0.31, showing that an exclusion frame of the same policy reduced the demobilization of opponents. Pro-choice participants opposed excluding less than banning, t (204) = 2.09, p < 0.038, d = 0.29.

Finally, we again find no significant differences between conditions among anti-abortion participants: defunding vs. banning, t (87) = 0.01, p = 0.99, d = 0.003; defunding vs. excluding, t (86) = 0.99, p = 0.32, d = 0.21; excluding vs. banning, t (77) = 0.90, p = 0.37, d = 0.21.

We next look at pro-choice participants' willingness to participate in politics against restrictions. Table 1 shows the results. Across the six forms of political participation, pro-choice participants said they would be less willing to take action against defunding than banning. In contrast, participants' willingness to take action against exclusion did not consistently differ from banning or defunding (see Table 1 for the exceptions). Among anti-abortion participants, most of the differences between conditions were not significant with two exceptions: they were more willing to donate and volunteer to support a ban compared to defunding, t (87) = 2.49, p = 0.015 for both measures (Appendix, Table A3).

These results for political participation differ from Experiment 1, where we did not find differences between conditions. We attribute the different results to our refinement of the question in which we piped each participant's policy position into the question to specify the policy position they would be advocating for.

In sum, these findings support the hypothesis that pro-choice supporters oppose defunding less than banning and excluding policies, even when the estimated consequences are held constant across the policies.

GENERAL DISCUSSION

In two experiments, we found that pro-choice participants were less opposed to a defunding policy (an indirect restriction) compared to a direct ban on abortion. In Experiment 1, pro-choice participants opposed the banning policy with an average rating of 3.9 (out of a maximum of 5), while their opposition decreased to 2.9 for the defunding policy. In Experiment 2, we added a statement to hold constant the consequences of the policies by stating the number of women whose access to abortion would be blocked. Pro-choice participants were again less opposed to defunding than banning. They opposed the banning policy with an average rating of 4, while their opposition decreased to 2.6 for the defunding policy. Participants also said they would be less willing to sign petitions, write letters, donate, volunteer, vote, or protest in order to oppose defunding compared to a ban. Thus, we find support for the demobilization hypothesis: Indirect restrictions provoke less opposition than direct restrictions.

TABLE 1 Pro-choice participants' willingness to participate in politics to oppose restrictions.

	Banning			Defunding			Excluding		
	M	**SD**	***t***	***M***	**SD**	***t***	***M***	**SD**	***t***
Sign petitions	6.7	3.2	2.7**	5.3	3.5	2.3*	6.5	3.4	0.4
Write letters	4.3	3.1	2.6**	3.2	2.8	1.0	3.6	3.1	1.5
Donate	4.1	3.2	2.9**	2.9	2.6	0.5	3.1	2.9	2.4**
Volunteer	3.3	2.9	2.9**	2.2	2.4	1.4	2.7	2.8	1.5
Vote	7.1	3.2	2.9**	5.9	3.6	1.8	6.8	3.3	0.8
Protest	3.1	3.1	3.5***	1.8	2.1	2.9**	2.9	3.2	0.4
Average	4.8	2.4	3.4***	3.6	2.3	1.9	4.2	2.5	1.5

Note: Participants' willingness to participate on a scale from 0 (*not at all willing*) to 10 (*extremely willing*). The *t*-test for each policy compares to the policy to the right, while the test for the last policy, excluding, compares to the first policy, banning.

*p < 0.05. **p < 0.01. ***p < 0.001.

Furthermore, we found that pro-choice participants became more opposed to defunding when it was framed as actively excluding women from public health care. In both experiments, pro-choice participants were more opposed to the excluding policy than the defunding policy, even though the policies were the same except for how they were framed. This finding reinforces the role of directness. Participants were more opposed to defunding, which was framed as excluding, a verb that expresses direct causation, in contrast to defunding framed as a passive omission. In other words, the demobilization effect for indirect policies can be diminished by framing those policies in more direct language.

These findings contribute to the literature on the moral psychology of indirect actions (e.g., Cushman et al., 2006; DeScioli et al., 2012; Mikhail, 2007). The current experiments extend previous findings to the case of indirect policies, particularly the multitude of indirect restrictions on abortion. Just as the directness of causation shapes how people judge someone's actions, it also shapes how they judge government policies. Policies that indirectly restrict people's rights draw less opposition than policies that restrict rights directly.

More generally, these experiments add to research at the intersection of moral psychology and politics, including research on politics and moral conviction (e.g., Delton et al., 2020; Ryan, 2017, 2019; Skitka et al., 2021; Skitka & Morgan, 2014), moral foundations (Haidt, 2012; Hatemi et al., 2019; Kertzer et al., 2014; Rathbun et al., 2019), and moral taboos (Del Ponte & DeScioli, 2022; Ginges et al., 2007; Tetlock, 2003).

Additionally, these experiments complement the literature on the expansion of state restrictions on abortion since *Roe* (Bentele et al., 2018; Camobreco & Barnello, 2008; Kreitzer, 2015; Lax & Phillips, 2012; Medoff et al., 2011). This literature has focused on analyses of observational data about restrictions, public opinion, party control of the legislature, and so on. The present research complements this work with experimental methods that offer insight into causation, specifically the psychological causes of public opinion.

Moreover, this work builds on previous research on abortion laws as a kind of morality policy (Mooney & Lee, 1995; Mooney & Schuldt, 2008). This research generally finds that abortion policies are responsive to public opinion, but there have been some mixed conclusions, as we previously discussed. Kreitzer (2015) suggested that some indirect restrictions like defunding and regulations of clinics may partly evade moral framing. Indeed, our experiments support this interpretation and provide an explanation grounded in moral psychology. Indirect offenses provoke less outrage, so indirect restrictions on abortion partly escape the moral condemnation of abortion supporters.

The limitations of this study include the convenience samples that we recruited online. In recent years, researchers have been increasingly concerned about low-quality participants from Amazon's Mturk such as bots and spam (Chmielewski & Kucker, 2020; Kennedy et al., 2020). However, researchers have also found practices that can minimize these issues (Aguinis et al., 2021; Del Ponte et al., 2024; Douglas et al., 2023). Future work with online samples should consider the best practices and alternatives.

What do these experiments imply for mobilizing abortion advocates? Pro-choice organizations can use moral framing to counter the demobilizing effects of indirect restrictions. Messages that emphasize directness with word choice and equivalences to direct restrictions could help to spur action. Advocates probably use these frames already to some extent, and future work could study the role of directness in political rhetoric about indirect restrictions.

Finally, what are the implications of these experiments now that the Supreme Court has overturned the constitutional right to abortion? On the one hand, legislators may be less likely to create indirect restrictions because they no longer need to evade the Supreme Court. If evasion was their only purpose, then their popularity should fade. On the other hand, legislators may continue to create indirect restrictions if they also serve the purpose of demobilizing political opponents. In states where abortion is a contested issue with roughly equal numbers on each side, conservative legislators may gain an edge with indirect policies that demobilize their opponents. Moreover, in states where a large fraction of Republicans support abortion rights, conservative legislators could use indirect restrictions to satisfy voters against abortion while preserving the votes of pro-choice Republicans. In short, conservative legislators may continue to favor indirect restrictions without

the need to evade the Supreme Court. If so, further research on the psychology of indirect policies could help to understand public opinion about abortion policies as well as legislators' strategies for appeasing the public.

CONFLICT OF INTEREST STATEMENT

None.

DATA AVAILABILITY STATEMENT

The data, code, and output replicating the tables and figures will be uploaded on the Harvard Dataverse upon conditional acceptance of the article.

ETHICS STATEMENT

This research received IRB approval, and all subjects provided their consent to participate in the research, meaning that they were aware that they were participating in a research study. Documentation of informed consent was waived since the survey was conducted online. No deception was used. Participants were paid 50 cents for each survey, which lasted about 5 minutes. Compensation is in line with the prevailing rate on MTurk in 2015 and decided based on the rates seen for similar Human Intelligence Tasks (HITs) on MTurk.

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Endnotes

[i] We also analyzed the data without any exclusions and found the same pattern of mean differences.

[ii] As before, we also analyzed the data without any exclusions and found the same pattern of mean differences.

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APPENDIX A

A.1 | Experiment 1

Supplementary Methods.

Participation question: What political activities would you be willing to participate in to express your views on this issue (above)? [Sliding scale: not at all willing, slightly willing, somewhat willing, very willing, extremely willing].

- Sign petitions.
- Write letters to politicians.
- Donate money to an organization or campaign.
- Volunteer time for an organization or campaign.
- Vote for politicians based on their views on abortion.
- Join political protests.

Supplementary Results**.**

TABLE A1 Pro-choice participants' willingness to participate in politics to oppose restrictions.

	Banning			Defunding			Excluding		
	M	***SD***	***t***	***M***	***SD***	***t***	***M***	***SD***	***t***
Sign petitions	5.6	3.9	0.4	5.4	3.8	0.3	5.2	3.7	0.6
Write letters	3.0	3.3	0.8	3.3	3.4	0.2	3.2	3.1	0.5
Donate	3.0	3.3	0.3	3.2	3.4	0.2	3.3	3.1	0.5
Volunteer	2.5	2.9	0.8	2.8	3.2	0.2	2.9	3.1	0.9
Vote	6.4	3.9	1.4	5.7	3.8	0.8	6.2	3.5	0.5
Protest	2.4	3.1	0.0	2.5	3.2	0.6	2.8	3.0	0.7
Average	3.8	2.9	0.0	3.8	3.0	0.3	3.9	2.6	0.2

Note: Participants' willingness to participate on a scale from 0 (*not at all willing*) to 10 (*extremely willing*). The *t*-test for each policy compares to the policy to the right, while the test for the last policy, excluding, compares to the first policy, banning.

TABLE A2 Anti-abortion participants' willingness to participate in politics to support restrictions.

	Banning			Defunding			Excluding		
	M	**SD**	***t***	***M***	**SD**	***t***	***M***	**SD**	***t***
Sign petitions	6.1	3.9	1.0	6.9	3.2	1.6	5.5	3.4	0.6
Write letters	4.2	3.8	0.7	4.8	3.5	0.9	3.9	3.6	0.3
Donate	4.0	3.7	0.5	4.4	3.3	1.5	3.0	3.7	1.1
Volunteer	3.6	3.8	0.1	3.7	3.2	1.1	2.8	3.8	0.8
Vote	6.2	4.1	1.3	7.2	3.2	1.6	5.8	4.0	0.4
Protest	2.7	3.4	0.1	2.7	3.0	0.2	2.6	3.2	0.2
Average	4.5	3.3	0.7	4.9	2.7	1.4	3.9	3.2	0.7

Note: Participants' willingness to participate on a scale from 0 (*not at all willing*) to 10 (*extremely willing*). The *t*-test for each policy compares to the policy to the right, while the test for the last policy, excluding, compares to the first policy, banning.

A.2 | EXPERIMENT 2

Supplementary Methods.

Participation question: Participants answered the following question, where the value in the first brackets depended on whether they said they supported, opposed, or neither, and the value in the second brackets matched the experimental condition.

What political activities would you be willing to participate in to [support/oppose/express your views about] [banning abortion/a ban on funding abortions/excluding abortions from government health care services]? (Note: We will *not* ask you to actually do any of these activities.) [Sliding scale: not at all willing, slightly willing, somewhat willing, very willing, extremely willing].

- Sign petitions.
- Write letters to politicians.
- Donate money to an organization or campaign.
- Volunteer time for an organization or campaign.
- Vote for politicians based on their views on abortion.
- Join political protests.

Supplementary Results**.**

TABLE A3 Anti-abortion participants' willingness to participate in politics to support restrictions.

	Banning			Defunding			Excluding		
	M	**SD**	***t***	***M***	**SD**	***t***	***M***	**SD**	***t***
Sign petitions	5.9	3.5	0.3	5.7	3.1	0.2	5.8	3.4	0.1
Write letters	4.6	3.3	1.5	3.5	3.5	1.0	4.2	3.1	0.6
Donate	4.4	3.1	2.5*	2.8	2.9	1.6	3.9	3.3	0.7
Volunteer	4.5	3.4	2.5*	2.8	3.0	1.5	3.8	3.3	1.0
Vote	6.3	3.5	0.4	6.0	3.4	0.8	6.6	3.0	0.4
Protest	3.3	3.2	1.6	2.3	2.8	1.0	2.9	2.9	0.6
Average	4.8	2.8	1.7	3.9	2.6	1.2	4.5	2.6	0.5

Note: Participants' willingness to participate on a scale from 0 (*not at all willing*) to 10 (*extremely willing*). The *t*-test for each policy compares to the policy to the right, while the test for the last policy, excluding, compares to the first policy, banning.

*$p < 0.05$.

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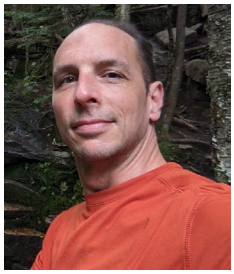

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