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Disentangling the direct and indirect effects of the initiative process

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Abstract Voter initiatives are important for policy making in many countries. While much research shows that the initiative process affects policy choices, almost no evidence explains *how* the initiative process affects policy. Initiatives might change policy directly through voters approving laws that override the legislature; or the initiative process may change policy indirectly by providing a threat that induces the legislature to change policy. This article develops an empirical strategy to measure the direct and indirect effects of the initiative based on the idea that direct effects can be inferred from states that actually pass initiatives while indirect effects can be inferred from states where the initiative is available but not used. Evidence from 50 states on nine separate issues suggests that both direct and indirect effects are important, but the direct effect is several times larger than the threat effect.

Keywords Direct democracy · Initiative · Public policy · Public opinion

1 Introduction

Direct democracy continues to be a central feature of the political landscape of national and subnational governments across the globe.[1] Citizen lawmaking is most common in Switzerland and the United States, but the practice is growing across the world; for example, voters recently decided important policy issues in Iceland, Sudan, Turkey, the United Kingdom, and Uruguay, and in 2010 the European Union introduced an EU-wide initiative process. In the American states, voters have decided more than 1,600 statewide ballot propositions in the 21st century. In some states, such as California and Oregon, it is impossible to understand state policy and politics without taking into account the initiative process, and even in states with less frequent citizen lawmaking, individual ballot measures continue to emerge

[1] For a description and overview of direct democracy practices across the globe, see International Institute for Democracy and Electoral Assistance (2008) and Kaufmann et al. (2010).

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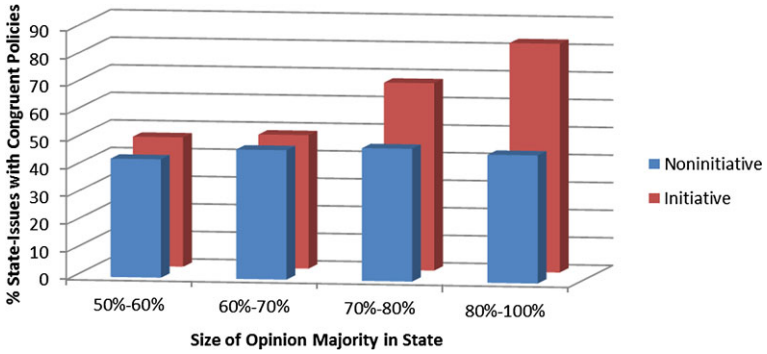

Fig. 1 Policies congruent with majority opinion, initiative versus noninitiative states

that have far-reaching impact. Many contentious issues in the American states are being fought out through ballot propositions, such as same-sex marriage, drug legalization, campaign finance, redistricting, taxes, and government borrowing, and legislatures in initiative states act under the shadow of an ever-present threat of future initiatives from disgruntled groups and individuals.

The common observation that ballot propositions drive policy choices has been reinforced by a substantial body of research that documents systematic policy differences between governments with and without the initiative process. In the United States, perhaps the most studied country, initiative states have been shown to choose policies that differ from those of noninitiative states across a variety of issues, including taxes, spending, abortion, death penalty, term limits, and others; and a healthy body of similar evidence exists for Switzerland.[2] Related research suggests that initiative states tend to choose policies that are more congruent with public opinion than noninitiative states (Matsusaka 2005, 2010). For example, Fig. 1 shows the percentage of states whose policy choices are congruent with majority opinion across nine separate issues, distinguishing initiative from noninitiative states (using data that are described in more detail later in the article). Initiative states are more likely than noninitiative states to choose the policy favored by the majority; the difference is small when public opinion is divided on an issue, but becomes enormous when public opinion is one-sided.[3]

While evidence that initiative states choose different policies than noninitiative states is abundant, exactly how those differences arise remains something of a mystery. Voters have approved some high profile initiatives, but many of the policy outcomes underlying the differences in Fig. 1 cannot be traced to measures that appeared on the ballot; apparently they resulted from decisions by legislators, or perhaps courts. The literature has suggested two broad channels through which the initiative process can influence policy. The direct channel is the most obvious: by voters approving new laws at the ballot box. The indirect channel is more subtle: by causing the legislature to adopt different policies than it would have adopted if the initiative process was unavailable. The initiative process can have an indirect effect if the legislature takes a preemptive action to deter the threat of being overridden (Gerber

[2]For surveys, see Lupia and Matsusaka (2004), Matsusaka (2005), and Garrett (2010). For recent evidence on same-sex marriage, see Lupia et al. (2010).

[3]For example, when public opinion is 80 % or more on one side, 82 % of initiative states choose the majority policy while only 46 % of noninitiative states choose the majority policy.

1996; Matsusaka and McCarty 2001) or if the legislature responds to new information that is revealed by election returns (Gerber 1999; Matsusaka 2004: Chap. 9). There is some anecdotal evidence that legislatures respond to initiative threats (Key and Crouch 1938; Gerber 1998) and that legislatures care about even unsuccessful measures (Gerber 1999), but there is little quantitative or statistical evidence on the relative importance of the different channels of influence. Until we can identify and quantify the channels through which the initiative process works, our understanding of the process will remain somewhat shallow.

The purpose of this article is to take some initial steps toward quantitatively disentangling the direct and indirect effects of the initiative, that is, to identify the mechanisms that create the gaps between initiative and noninitiative states in Fig. 1. A standard research design in this literature is to compare policies in initiative and noninitiative states, typically using state-level cross-sectional or panel data. With sufficient controls for other determinants of policy, noninitiative states serve as a control group, and the conditional differences between initiative and noninitiative states can be attributed to the presence of the initiative process. Typically, this boils down to introducing an initiative dummy variable (or index) into a multivariate regression; the coefficient on the initiative dummy reveals whether or not the initiative matters, but does not separate the initiative effect into direct and indirect channels.[4]

One contribution of this study is to illustrate how the initiative effect can be decomposed into pieces that represent direct and indirect effects so that the separate channels of influence can be isolated. Intuitively, the direct effect of the initiative on a particular issue can be identified from the policy choices of states in which an initiative was approved by the voters, while the indirect effect can be identified from the policy choices of states where the initiative process was available but an initiative was not approved, in both cases using the policies of noninitiative states as a control group. The indirect effect can be further parsed by distinguishing states in which a measure appeared on the ballot but was rejected (communication or signaling effect) versus states in which no initiative appeared on the ballot at all (pure threat effect). There is a straightforward empirical specification that captures the different channels, made precise below.

The substantive contribution of this study is the reporting of evidence on the three channels of influence discussed in the literature. Based on data describing policy choices in all 50 American states on nine different issues, and controlling for public opinion, demographics, and other variables, I find that the initiative influences policy through both the direct and indirect channels, but the direct channel is more important. In terms of congruence, initiative states are 16 % more likely than noninitiative states to choose a policy congruent with public opinion, but the difference is 35 % when initiatives are actually approved (direct effect) compared to 9 % when the initiative is only a threat (indirect effect). In terms of the ideological orientation of policy choices rather than congruence, initiative states are 16 % more likely than noninitiative states to choose a conservative policy, but the difference is 40 % when a conservative initiative is actually approved compared to 7 % when the initiative remains only a threat. If a conservative proposal qualifies for the ballot but is rejected by the voters, the state is 21 % less likely to adopt a conservative policy than a noninitiative state. These findings provide support for existing theories of the initiative process, but suggest that the much-discussed indirect effects may be less important in practice than commonly believed.

[4]The literature that uses a dummy variable or a simple comparison between initiative and noninitiative states is extensive, for example, Boehmke (2005) on death penalty and Indian gaming; Feld and Kirchgassner (2001) on debt; Feld and Savioz (1997) on economic performance; Gerber (1999) on abortion and death penalty; Matsusaka (1995, 2004) on fiscal policy and fiscal institutions; Matsusaka (2008) on the executive branch; Matsusaka (2009) on public employment; and Persily and Anderson (2005), Matsusaka (2006), and Smith (2008) on election law.

The study seeks to advance the literature in several ways. In terms of methods, it offers a strategy for disentangling the different effects of the initiative that could be extended to analyze the workings of other institutions with both direct and indirect effects, such as the executive veto. The core idea of distinguishing availability versus use of the initiative process to separate effects was first suggested by Boehmke and Witmer (2004) as something of a sidelight to their main analysis, but their contribution seems to have eluded the attention of direct democracy scholars.[5] In this article, I hope to call attention to the general approach and lay down its foundations more systematically. In terms of theory, the findings provide support for game theoretic models of the initiative process, such as Gerber (1996) and Matsusaka and McCarty (2001), which predict the initiative influences policy indirectly by providing a threat. The evidence lends support to a central message of these models that it is important to consider the strategic responses of political actors to institutional opportunities. Finally, the evidence highlights the importance for empirical research of considering both direct and indirect effects of the initiative. Gerber (1998: 192) speculates, "Studies that focus solely on direct influence [ballot propositions that are actually passed] are likely to seriously underestimate the influence of groups that use initiatives to achieve indirect influence." The findings confirm the importance of considering both direct and indirect effects, and by quantifying the direct and indirect effects, suggest that the extent of underestimation can be large if the indirect effects are ignored.

2 Channels of influence

This section reviews existing theories on direct and indirect effects of the initiative, and discusses approaches to testing them.

2.1 Direct effect: override

The direct channel—voter approval of new laws proposed by citizen petition—is the most obvious way that the initiative process can influence policy. The potential importance of this channel depends on the degree to which legislatures respond to public opinion: if elected officials consistently and accurately represent their constituents, there would be little scope for initiatives to override their decisions.

Many reasons have been offered why legislatures might choose policies incongruent with public opinion. The Progressives, who agitated for the initiative process at the turn of the nineteenth century, focused on the influence of special interests:

> If we felt that we had genuine representative government in our state legislatures no one would propose the initiative and referendum in America. They are being proposed now as a means of bringing our representatives back to the consciousness that what they are bound in duty and in mere policy to do is to represent the sovereign people whom they profess to serve and not the private interests which creep into their counsels by way of machine orders and committee conferences. (Wilson 1912: 87–88)

[5] Boehmke and Witmer (2004) find a significant direct effect but an insignificant indirect effect of the initiative on state adoption of Indian gaming compacts. This finding may be closer to an example than evidence of a general pattern because it is based on a specific and somewhat narrow issue and because the direct effect is identified based on only two cases in their sample of 576 observations.

A recurrent theme in political economy research is the existence of barriers to citizen control of public policy, such as limited information of voters and representatives (Lupia and McCubbins 1998; Groseclose and McCarty 2000), interest groups (Olson 1965; Stigler 1971; Peltzman 1976) and legislative structure (Weingast et al. 1981; Cox and McCubbins 2005). Opinion surveys reveal that most citizens believe government to be more responsive to powerful interests than ordinary people.

Identifying a direct effect in the data is not as simple as it might seem. One cannot infer a direct effect simply by observing that an initiative was approved; it must also be the case that the initiative resulted in a policy different from what otherwise would have prevailed. The legislature might have adopted the same policy on its own even without an initiative, in which case the initiative is merely the vehicle, not the driver. A case in point: while initiatives have banned same-sex marriage in 11 states, legislatures have placed similar measures on the ballot in 21 other states, raising the possibility that the end result would have been the same in initiative states regardless of whether the process was available. Moreover, as Gerber et al. (2001) and Kousser et al. (2008) show, even when voters approve an initiative, it may not go into effect because of a court challenge or lack of enforcement. In order to identify a direct effect on a particular issue, then, the question is not whether voters approved an initiative on that issue, but whether the policy prevailing in states that approved an initiative differs from the policy prevailing in states where an initiative was not approved.

2.2 Indirect effect: threat

The indirect threat effect of the initiative is a central feature of game theoretic models (Gerber 1996; Matsusaka and McCarty 2001).[6] In these models, the legislature has a policy preference that may not coincide with the median voter's preference. If an interest group dislikes the current policy, a forward-looking legislature may accommodate the group by moving the policy closer to the group's preference in order to deter the group from placing a measure on the ballot. As a result, the policy choice may end up being different due to availability of the initiative even though a proposition does not appear on the ballot. With complete information, the policy changes brought about by the threat always help the median voter because only a threat to move toward the median voter is sufficiently credible to elicit a legislative response. However, with incomplete information about voter preferences or policy consequences, the legislature may accommodate an extreme interest group by moving policy away from the median voter (Matsusaka and McCarty 2001).

Existing evidence on the indirect threat effect is largely anecdotal. Gerber (1998) discusses specific cases wherein initiative threats appeared to prompt policy changes by the legislature, and a few other examples can be found in popular accounts. Randolph (2010) documents that legislatures enact more bills in initiative states than noninitiative states, which he interprets as greater deterrence activity. Assessing the importance of the threat effect is particularly important because game theoretic models suggest that the threat effect is the primary channel through which the initiative influences policy. Indeed, in models with complete information, the initiative influences policy *only* by threats—in equilibrium no initiatives ever reach the ballot because the legislature accommodates groups with credible threats in a way that deters all initiatives. The threat effect can be inferred by comparing

[6] All such models build on the agenda-setter framework of Romer and Rosenthal (1978, 1979).

policies of initiative states in which no initiatives actually were approved with the policies of noninitiative states.

2.3 Indirect effect: communication

Another channel of influence, less prominent than the override and threat channels, operates through an initiative's election returns communicating information about public opinion to elected officials, who adjust policy accordingly. This "communication" (or sometimes "signaling") channel is premised on the idea that elected representatives would like to follow public opinion, at least to some degree, but are unsure about what their constituents want.[7] The idea that representatives may make "honest mistakes" that can be corrected by ballot propositions is explored in Gerber (1998), and Matsusaka (1992, 2004). Communication between voters and legislators is presumably the rationale for nonbinding advisory votes that are held in many cities and some states.

The communication channel has been recognized for some time. Key and Crouch (1938: 457) observed that:

> [T]he initiative occasionally plays a role of varying importance in bringing about new legislation through the ordinary procedures of lawmaking. The campaign in support of the initiative may demonstrate to the legislature that, with certain alteration, the program would be in accord with public opinion; it may bring to the public attention abuses requiring correction; it may bring opposing groups to recognition of the futility of demanding enactment of their unaltered ideas, thereby facilitating compromise.

The belief that the communication channel is important is widely held by initiative proponents: in a survey of initiative proponents, Gerber (1999: Chap. 5) finds that the single most important reason businesses and other groups put measures on the ballot is to signal support for the law to the legislature, not to gain passage of the initiative itself. The importance of the communication channel can be inferred by comparing policies in states where initiatives failed with policies in initiative states without unsuccessful initiatives, and linking those policy differences to the number of votes received by unsuccessful initiatives.

2.4 Other indirect effects

Indirect effects other than threat and communication have been postulated, or can be inferred from existing research. Several studies have emphasized the so-called educative effects of initiatives: initiative campaigns may cause citizens to become more informed about and active in politics (e.g., Smith and Tolbert 2004). A more informed electorate may put pressure on legislators to respect constituent views, leading to policies that are more congruent with public opinion. Another view is that availability of the initiative process creates incentives for interest groups to form and mobilize (Boehmke 2005; Boehmke and Bowen 2010). These interest groups could influence the legislature through lobbying, campaign contributions, and so on, leading to different policies than if the groups were absent. Although this study does not attempt to construct tests targeted directly at these channels, to the extent that the evidence is consistent with the idea of indirect channels of influence, it lends some support to the existence of these other channels.

[7] For a discussion and evidence that legislators do change their positions in light of information revealed in elections, see Kousser et al. (2007).

3 Empirical strategy

3.1 Methods

The analysis focuses on a set of $n = 1, \ldots, 9$ policies in $s = 1, \ldots, 50$ states using a regression framework of the form

$$y_{ns} = a + b\boldsymbol{I}_{ns} + c\boldsymbol{X}_{ns} + u_{ns}, \tag{1}$$

where y is a dichotomous policy outcome equal to 0 or 1, $\boldsymbol{I}$ is a vector-valued variable capturing initiative channels of influence, $\boldsymbol{X}$ is a vector of control variables, u is an error term, and a, b, and c are parameters to be estimated.[8] In much of the existing literature, $\boldsymbol{I}$ is specified as a dummy variable equal to 1 if a state allows the initiative and 0 otherwise.[9] With such a specification, the coefficient on the dummy variable absorbs all of the different initiative effects, and provides a summary indicator of the overall effect of the initiative.[10]

In order to disentangle the different channels of influence, multiple dummy variables are introduced. The simplest specification distinguishes direct from indirect effects by using two dummy variables:

$$b\boldsymbol{I}_{ns} = b_0 I_s^{AVAILABLE} + b_1 I_{ns}^{DIRECT}, \tag{2}$$

where $I_s^{AVAILABLE} = 1$ if the initiative was available in state s, and zero otherwise; $I_{ns}^{DIRECT} = 1$ if an initiative on issue n was approved in state s, and zero otherwise.

Since initiative availability is captured with $I_s^{AVAILABLE}$, the direct effect of the initiative is given by b_1 and the indirect effect is given by b_0. The "full effect" in a state where voters approved an initiative is captured by $b_0 + b_1$ because both channels of influence are at work. The questions of interest are, first, whether b_0 and b_1 are different from zero (that is, whether either effect matters) and, second, how the two coefficients compare to each other in magnitude.

The indirect channel can be further disentangled into threat and communication components by introducing a third dummy variable:

$$b\boldsymbol{I}_{ns} = b_0 I_s^{AVAILABLE} + b_1 I_{ns}^{DIRECT} + b_2 I_{ns}^{COMM}, \tag{3}$$

where $I_{ns}^{COMM} = 1$ if an initiative on issue n appeared on the ballot in state s but was not approved, and zero otherwise.

In this case, the direct effect associated with approval of an initiative continues to be captured by b_1. The indirect effect from communication (associated with an unsuccessful

[8] The article reports results from linear probability models (1), but every regression was also estimated in a logistic specification, which has theoretical advantages given the dichotomous dependent variable. I have chosen to report coefficients from the linear specification because they have a direct interpretation as marginal probabilities, and the linear specifications do not produce findings that differ in material ways from the logistic specification.

[9] A simple dummy variable formulation implicitly assumes that the initiative process is "equally effective" in every state that allows it. While this is a reasonable starting point, there is some evidence that initiative effects depend on how easy it is to use the process, as determined, for example, by signature requirements or petition periods. I discuss this issue below.

[10] More precisely, b captures differences between initiative and noninitiative states. To the extent that initiative and noninitiative states are otherwise identical conditional on $\boldsymbol{X}$, one can interpret b as a causal parameter. Matsusaka (2004) contains a longer discussion, and presents evidence suggesting that initiative and noninitiative states are not likely to differ in terms of unobserved ideology or culture.

measure) is captured by b_2 and the indirect threat effect is captured by b_0.[11] Note that I^{DIR} and I^{COMM} are not mutually exclusive; a state could have had both a successful and unsuccessful initiative on a particular issue.

3.2 Data

The investigation focuses on nine public issues across the 50 states as of 2006, giving a total of 450 observations in most estimates. The issues were selected based on availability of opinion data in the American National Election Studies (ANES). The ANES treats each policy as having a dichotomous outcome (for example, allowing or prohibiting the death penalty), so there is a well-defined majority position on each issue. Table 1 reports descriptive and summary information on the nine issues.

An important control variable in regression (1) is public opinion. The literature has tended to control for public opinion using an ideology index (such as the state ideology variable in Erikson et al. 1993), demographic variables, or both. The estimates below utilize issue-specific measures of public opinion from the ANES, which in principle are more accurate than general ideology or demographic variables, but I also examine broad-based ideology measures and demographics for robustness. All of these measures have some limitations; the appendix discusses the variables in more detail.[12]

Model (1) is operationalized in two ways. The first approach utilizes a dependent variable that measures the congruence between policy and public interest. For issue n and state s, "congruence" is defined as

$$y_{ns} = \begin{cases} 1 & \text{if state } s \text{ chooses the outcome preferred by the majority on issue } n; \\ 0 & \text{otherwise.} \end{cases} \tag{4}$$

Thus, $y_{ns} = 1$ means that state s has adopted the majority's position (which is also the median position) on issue n, while $y_{ns} = 0$ means the state has adopted the minority's position. The initiative variables in (2) and (3) then reveal how the different channels of influence affect the congruence of policy and opinion.[13]

The other approach to model (1) utilizes the actual policy choice as the dependent variable. In order to consider different policies in the same regression, the outcomes must be expressed using a common metric. I use the ideological orientation of the policy, that is, y_{ns} is defined as:

$$y_{ns} = \begin{cases} 1 & \text{if state } s \text{ chooses the conservative outcome on issue } n; \\ 0 & \text{otherwise.} \end{cases} \tag{5}$$

[11] In principle, the communication effect should depend on vote totals. For example, a one-sided rejection of a measure might convey more information than a 49–51 rejection; similarly, the results from a high turnout election may communicate more than results from a low turnout election. Some estimates in this vein are discussed below.

[12] One particular limitation is worth noting here: for the most part the ANES is not designed to be representative at the state level (the exception is the 1988–1992 pooled Senate study). To the extent that state-level public opinion is measured incorrectly, the regression coefficients will be estimated with error, and biased toward zero. Thus, the limitations of the ANES data bias against finding significant results, and strengthen our confidence in findings when they occur.

[13] There is a large literature concerned with estimating policy congruence and policy responsiveness. The present article does not attempt to break new ground on the core research design questions in that literature but rather uses "off the shelf" variables that were developed previously. The measure in (4) is essentially the concept stated in Gerber (1999) as implemented in Matsusaka (2010).

Table 1 Issues and number of initiatives

Issue	Question	Mean % in favor	Years	Number of states with initiatives on issue	Number of states with successful initiatives
Abortion, late term/partial birth	"There has been discussion recently about a proposed law to ban certain types of late-term abortions, sometimes called partial birth abortions. Do you favor or oppose a ban on these types of abortions?"	67.6	1998, 2000, 2004	2	0
Abortion, parental consent	"Would you favor or oppose a state law that would require parental consent before a teen-ager under 18 could have an abortion?"	74.4	1988, 1990, 2000	4	2
Abortion, public funding	"Would you favor or oppose a law in your state that would allow the use of government funds to help pay for the costs of abortion for women who cannot afford them?"	49.4	1988	7	3
Death penalty	"Do you favor or oppose the death penalty for persons convicted of murder?"	78.0	1988, 1990, 1994, 1996, 1998, 2000, 2004	5	5
English language, official	"Do you favor a law making English the official language of the United States, meaning government business would be conducted in English only, or do you oppose such a law?"	71.4	1990, 1998, 2000	6	6
Estate tax	"There has been a lot of talk recently about doing away with the tax on large inheritances, the so-called '[estate/death]' tax. Do you favor or oppose doing away with the [estate/death] tax?"	72.5	2002, 2006	4	3
Job discrimination, homosexuals	"Do you favor or oppose laws to protect homosexuals against job discrimination?"	65.4	1990, 1994, 1996, 2000, 2004	6	2
Same-sex marriage	"Should same-sex couples be allowed to marry, or do you think they should not be allowed to marry?" Responses: 1 = Allowed. 5 = Not allowed. 7 = Not allowed to marry, but civil unions allowed. ("In favor" = response 1)	32.3	2004	11	10
Term limits	"A law has been proposed that would limit members of Congress to no more than 12 consecutive years of service in that office. Do you favor or oppose such a law?"	78.3	1990, 1994, 1996, 1998	21	19

Note. "Question" is the question asked in the American National Election Studies survey. "Year" is the study year, except that 1988 refers to the 1988–1992 ANES Pooled Senate File. Statistics for "% in favor" were computed with the state as the unit of observation. A successful initiative was one approved by the voters or an indirect initiative approved by legislature

Most of the policies studied in this study have a natural ideological orientation, for example, support for the death penalty is the "conservative" position and opposition is the "liberal" position. To provide a systematic classification, I regressed the percentage of citizens in a state that favor a given outcome on the state ideology index developed by Erikson et al. (1993). For seven issues, conservative states were more likely to express what would normally be considered the conservative opinion. For two issues, English as the official language and estate tax, there was no significant correlation between state ideology and positions for or against the policy. Given the lack of evidence on whether support or opposition is the conservative position on these two issues, there was no convincing way to classify outcomes for those issues, and they are not considered in the outcome regressions. For the remaining issues, "conservative" outcomes are defined as opposition to "partial-birth" abortion, opposition to public funding of abortion, opposition to anti-discrimination laws based on sexual orientation, opposition to same-sex marriage, support for parental notification before a minor has an abortion, support for the death penalty, and support for term limits.

The empirical strategy requires distinguishing states according to availability of the initiative, as well as presence and passage of ballot measures. Information on initiative availability was taken from Matsusaka (2004: Appendix 1).[14] Information on initiatives that reached the ballot was collected by searching the Initiative and Referendum Institute's *Initiatives Historical Database* (version 2012-2) that lists and describes all statewide initiatives that have reached the ballot since the first one in Oregon in 1904. I extracted every initiative related to one of the nine issues that had appeared on the ballot through 2006.[15] For each state and each issue, I determined whether or not an initiative had appeared on the ballot, and whether or not an initiative had passed.[16] Table 1 shows the number of states that had an initiative on each subject and how many passed. As can be seen, the most common issue was term limits, which appeared on the ballot in 21 states. By way of comparison, 23 states allow the initiative (excluding Illinois, as discussed above). Same-sex marriage initiatives were also common, appearing in 11 states. Initiatives concerning partial-birth abortion were the rarest, appearing in only two states. The approval rate varied by issue, with 100 % of states with death penalty initiatives approving at least one, compared to no successful partial-birth abortion initiatives. For the nine issues overall, 15 % of states had an initiative on the ballot at some point, and 11 % of states approved a measure. Only 5 % of states had a measure on the ballot that failed; the small number of such cases is an obstacle to precise es-

[14] As is usual in the direct democracy literature, I classify Illinois as a noninitiative state. Illinois' initiative process can be used only to amend structural and procedural subjects contained in Article IV of the state constitution, which has to do with the legislature. The state does not permit initiatives concerning any of the issues considered in this study, e.g. in 1994 the Illinois Supreme Court prevented an initiative concerning term limits from appearing on the ballot.

[15] Initiatives requiring parental notification were included in the "parental consent" category. The results are similar if they are excluded. The "term limits" category does not include initiatives that allowed a representative to take a non-binding term limits pledge; only initiatives that limited terms by law. For term limits, I counted laws placing term limits on state legislators even though the ANES question asks about term limits on congressmen; see discussion in the appendix.

[16] In states with an indirect initiative process, after citizens collect enough signatures to place a measure on the ballot, the legislature has the option to adopt the proposal without sending it to the voters. I was able to identify three initiatives that became law in this way, without going to the voters, and include those in the category of initiatives that were approved: Alaska approved capital punishment in 1986, Michigan prohibited public funding of abortion in 1987, and Michigan required parental consent for abortions by minors in 1990.

timation of the communication channel. Data on existing state policies was assembled from a variety of online sources, verified by consulting state law when necessary. Demographic variables were taken from the Census Bureau.[17]

The empirical analysis here is cross-sectional, largely due to data limitations. This means that the key effects are identified based on comparisons across states and issues at a point of time, rather than from changes within a given state over time.[18] Even when time series data are available, it is often difficult to estimate institutional effects because many institutions are sticky and do not change over time (for example, only one state has changed its initiative status during the last 40 years). The lack of time series data also prevents investigation of the speed at which policy adjusts to changes in public opinion; the present study can investigate only the extent to which policy reflects prevailing opinion at a given point of time.

4 Empirical results

Table 2 provides an overview of policy choices across states and issues, distinguishing whether or not the initiative is available, and whether or not it is used. When the initiative is not available, only 46.9 % of state-issues reflect the majority's preference. Since choosing a policy by flipping a coin would yield 50 % congruence, public opinion does not seem to be a strong determinant of policy choice in noninitiative states on these issues. Congruence is 61.3 % in states where the initiative is available; within these states congruence is higher when a proposition was approved (78.0 %) than when a proposition was not approved (56.1 %). In terms of the ideological orientation of the policy choices, states with the initiative are more likely to choose the "conservative" policy outcome than states without the initiative: 66.7 % compared to 47.3 %. The difference again is especially pronounced when an initiative was actually approved, with a conservative outcome 82.0 % of the time in states with a successful initiative compared to 61.8 % of the time in initiative states without a successful initiative.

Table 2 State policy outcomes by initiative status

Initiative status	Percent congruent	Percent conservative	Observations
INITIATIVE NOT AVAILABLE: State does not permit initiatives	46.9	47.3	243
INITIATIVE AVAILABLE: State permits initiatives	61.3	66.7	207
DIRECT: Initiative available and ballot proposition approved	78.0	82.0	50
INDIRECT: Initiative available but no ballot proposition was approved	56.1	61.8	157

Note. The unit of observation is an issue-state. The first column reports the percent of observations in which policy is congruent with the views of a majority of people in the state, and the second column reports the percent of observations in which the state's actual policy is the conservative outcome

[17] Additional information on data sources is contained in the appendix to Matsusaka (2010).

[18] Note that the direct effects are not identified entirely from cross-state differences; the identification also exploits variation across issues.

Table 3 Linear regressions explaining probability of a congruent policy choice

	(A)	(B)	(C)	(D)	(E)	(F)
Dummy = 1 if initiative available	0.16***	0.09*	0.09*	0.09*	0.12*	0.10*
	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
DIRECT: Dummy = 1 if initiative available & at least one initiative was approved	...	0.26***	0.26***	0.26***	0.21**	0.31***
		(0.08)	(0.08)	(0.08)	(0.10)	(0.08)
INDIRECT/COMMUNICATION: Dummy = 1 if at least one initiative failed	...	...	−0.04	−0.44	0.02	−0.05
			(0.10)	(1.00)	(0.12)	(0.11)
INDIRECT/COMMUNICATION: % vote in favor of failed initiative	...	...	...	0.01	...	...
				(0.02)		
Public opinion: size of majority (%)	0.021***	0.021***	0.020***	0.021***	0.019***	0.018***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Education, % with high school diploma	−0.40	−0.31	−0.31	−0.30	−0.89	−0.80
	(0.68)	(0.67)	(0.67)	(0.67)	(0.86)	(0.72)
Income per capita	−0.77	−0.97*	−0.97*	−0.96*	−0.03	−0.99
	(0.57)	(0.57)	(0.57)	(0.57)	(0.70)	(0.62)
Dummy = 1 for Southern states	0.13**	0.13**	0.13**	0.13**	0.15**	0.14**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.08)	(0.06)
Observations	450	450	450	450	304	391
	All	All	All	All	w/o imputed opinion	w/o majority <60 %

Note. Each column report estimates from a linear regression to explain the probability of a congruent policy choice, defined as a policy outcome supported by a majority of citizens. Standard errors are in parentheses beneath the coefficient estimates. The regressions cover nine issues across 50 states, except where noted. Each regression included nine issue-specific dummy variables whose coefficients are not reported. Coefficients on income are multiplied by 100 for readability. Significance levels are indicated: $^{*}= 10\ \%$, $^{**}= 5\ \%$, $^{***}= 1\ \%$

4.1 Congruence

The next step is to investigate if the differences in Table 2 can be attributed to initiative status or other factors. Each column of Table 3 reports a linear regression of model (1) in which the dependent variable is a dummy variable equal to 1 for a congruent policy outcome (as defined in (4)). Column (A) establishes a benchmark by showing a difference in congruence between initiative and noninitiative states, controlling for other factors. The positive and significant coefficient of 0.16 on the initiative dummy indicates that policies are 16 % more likely to be congruent with majority opinion in initiative than noninitiative states.

The regressions include control variables that are standard in the literature. The most important variable in terms of explanatory power is public opinion; for each percentage point increase in the size of the opinion majority, the probability of a congruent outcome increases by 2.1 %, according to the point estimate. One possible explanation for this pattern is that a large majority offers more support toward meeting supermajority requirements for overriding executive vetoes, ratifying constitutional amendments, and other legislative procedures that empower a minority. Another possible explanation is that states with heterogeneous opinion (a small majority) are more difficult to represent because the majority view is more difficult to identify, leading to more "honest mistakes" by politicians when setting policy (Matsusaka and McCarty 2001). A third possible explanation is measurement error: the majority's position and hence congruence is most vulnerable to mismeasurement when the majority is close to 50 %.

The two demographic controls, education and income, are not statistically significant in column (A), and they display weak explanatory power throughout the article. Education and income could influence congruence if more educated and wealthy voters have different policy preferences, or a different ability to monitor elected officials. The dummy variable for Southern states is statistically different from zero at the 5 % level, and the coefficient indicates that Southern states are 13 % more likely to be congruent. It is conventional to include a Southern dummy in regressions of this sort, and the dummy almost always has predictive power, possibly because it captures unmeasured aspects of state culture.[19] Finally, this regression and all others throughout the article include separate dummy variables for each issue; those coefficients are not reported.

Column (A) establishes that initiative states are more congruent than noninitiative states, holding constant the control variables. The main task of the article is to shed some light on the reason for that difference. Column (B) reports a regression that separates direct and indirect effects using the approach in Eq. (2), one dummy variable for initiative availability and one dummy variable for states in which voters actually approved an initiative on the issue in question. Both initiative coefficients are positive and different from zero at the 10 % level of statistical significance or better, suggesting that both direct and indirect effects are important, but the coefficient for the direct effect is more than three times the magnitude of the coefficient for the indirect effect. The estimates imply that having the initiative process

[19] I estimated numerous exploratory regressions using other control variables, none of which changed the findings in a material way or were reliably statistically significant. Among the demographic variables considered were different measures of education, population, urbanization, race, and ethnicity. I also explored alternative variable specifications, such as log of income instead of its level, and allowed the initiative effects to interact with the opinion variables. I experimented with other measures of political culture, but none had explanatory power. In a working paper version of the study, I included a variable for the age of the state, which does have explanatory power in some specifications; I excluded this variable in the present version because its interpretation is unclear and its inclusion does not alter the main findings related to initiative status. I also ran exploratory regressions with control variables representing legislative structure and professionalism, which did not alter the main results.

available increases the probability of congruence by 9 %, and when voters actually approve an initiative, the probability of congruence increases by 26 %. Although the coefficient for the direct effect is larger than the coefficient for the indirect effect, the difference is not quite statistically significant ($p = 0.12$).[20]

Column (C) reports a regression that allows for a separate indirect "communication" effect by including a dummy variable for states that had an initiative on the ballot that failed, as indicated in (3). This variable allows for the possibility that the election returns from a ballot measure, even if unsuccessful, might influence the legislature's behavior. The estimates provide little evidence of an indirect channel of this type. The coefficient on the dummy for failed initiatives is quantitatively small and far from statistical significance. Taken at face value, these estimates imply that the initiative influences congruence by allowing threats, but placing a measure on the ballot that is unsuccessful does not increase (or reduce) the effectiveness of the threat. One caveat is that there are not many observations that fit into the indirect communication category (only 5 % of the total), so it might be difficult to detect a communication effect if it does exist (although an effect does appear in Table 4). The fact that there are so few cases in this category itself suggests that this channel of influence is of secondary importance for the issues studied here.

The specification in (C) treats all failed initiatives as the same. However, an initiative that fails by a small margin might convey different information than one that fails by a large margin. To allow for this possibility, the regression in column (D) adds a variable equal to the percentage of votes in favor of the failed initiative (with potential values in the range $[0, 49.9]$).[21] This specification fares no better: neither coefficient related to failed initiatives is statistically different from zero.

The last two regressions investigate robustness of the results. As discussed above, issue-specific opinion data had to be imputed for some states because of small sample sizes in the ANES. There is no reason to expect a bias in the resulting congruence measures, but they could be noisy. Column (E) reports a regression with the same specification as column (C) except that observations with imputed public opinion are excluded. The results remain qualitatively the same. A related concern is that congruence is susceptible to mismeasurement when public opinion is evenly divided. Column (F) reports a regression with the same specification as (C) except that observations where the majority was less than 60 % are excluded. Again, the results remain qualitatively the same.[22]

I also explored but do not report estimates that distinguish between legal rules for the initiative process in different states. Each state implements its initiative process in a different way, and those variations in implementation may influence congruence. To examine this, I allowed the initiative effect to vary with the signature requirement for qualifying a

[20] I also estimated an equation with state fixed effects (necessarily omitting the initiative availability variable). The coefficient on the direct effect dummy variable was 0.29, with $p < .01$.

[21] If there was more than one failed initiative in a state on an issue, the average approval rate was used.

[22] Another robustness concern is whether any one particular issue out of the nine is driving the results. Based on regressions that delete issues one by one, it can be determined that the term limits issue contributes more to the results than any other single issue. If term limit observations are deleted from the sample, the regression continues to show an overall statistically significant indirect effect and a positive direct effect, but the standard error increases so that the coefficient on the direct effect is not different from zero at conventional levels of significance. The lack of significance could be due to having many fewer observations that use the direct channel once the term limits observations are removed. A similar pattern holds for the policy regressions in Table 4, although the direct effect remains significant in some specifications. A plausible conclusion is that the sample displays robust evidence of an overall initiative effect, but the evidence for the direct channel depends to a large extent on the term limits observations.

Table 4 Linear regressions explaining probability of conservative policy choice

	(A)	(B)	(C)	(D)	(E)	(F)
Dummy = 1 if initiative available	0.16***	0.07	0.10**	0.10**	0.07	0.06
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
DIRECT: Dummy = 1 if conservative initiative approved	…	0.33***	0.29***	0.29***	0.40***	0.32***
		(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
INDIRECT/COMMUNICATION: +1 if conservative initiative failed, −1 if liberal initiative failed, else zero	…	…	−0.31***	−0.46	−0.31***	−0.32***
			(0.08)	(0.095)	(0.09)	(0.09)
INDIRECT/COMMUNICATION: +%vote in favor of failed conservative initiative, −%vote in favor of failed liberal initiative, else zero	…	…	…	0.003	…	…
				(0.002)		
Public opinion: % in favor of conservative policy choice	0.018***	0.017***	0.016***	0.016***	…	…
	(0.003)	(0.003)	(0.003)	(0.003)		
State/citizen ideology: Erikson et al. in (E), Berry et al. in (F)	…	…	…	…	−0.020***	−0.49***
					(0.005)	(0.11)
Education, % with high school diploma	0.32	0.48	0.53	0.53	−0.29	−0.09
	(0.70)	(0.69)	(0.67)	(0.67)	(0.75)	(0.71)
Income per capita	−1.74***	−2.00***	−2.11***	−2.11***	−0.95	−2.42***
	(0.62)	(0.61)	(0.60)	(0.60)	(0.78)	(0.59)
Dummy = 1 for Southern states	0.20***	0.21***	0.21***	0.21***	0.10	0.23***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
Observations	350	350	350	350	336	350

Note. Each column report estimates from a linear regression to explain the probability of a conservative (versus liberal) policy choice. Standard errors are in parentheses beneath the coefficient estimates. The regressions cover seven issues (excluding English-only and estate tax) across 50 states. Each regression included seven issue-specific dummy variables whose coefficients are not reported. Coefficients on income are multiplied by 100 for readability. Significance levels are indicated: *= 10 % , **= 5 % , ***= 1 %

measure for the ballot, and depending on whether the state allows initiatives to amend the constitution or pass statutes. I also introduced two index variables developed by Bowler and Donovan (2004), capturing legislative insulation and qualification difficulty. None of these institutional features had a reliable connection with congruence, or led to different conclusions regarding the importance of the three channels.

To summarize, initiative states are 16 % more likely to choose congruent policies than noninitiative states on these issues. The main reason for the higher congruence in initiative states is the approval of propositions that appear on the ballot, not the threat of propositions. Yet even without having an initiative on the ballot, initiative states have 9 % higher congruence, so the threat effect appears to be real. The regressions provide no evidence in support of an indirect communication channel that operates through failed ballot propositions.

4.2 Conservative versus liberal outcomes

The preceding section explores how the initiative affects the congruence between policy and public opinion. This section explores how the initiative affects the ideological direction of policy choices. Since the outcome is dichotomous, the choices can be described as "conservative" or "liberal" in most cases. There is no a priori reason to expect the initiative process to push policy in one direction or the other, on average. However, empirical studies of direct democracy routinely find that initiative and noninitiative states choose systematically different policies, even when their ideological orientations are not different.[23] It is therefore interesting to examine whether initiative and noninitiative states choose different policies for the issues examined in this article, and if so, which channel of influence can best account for the different policies.

Table 4 reports estimates from regressions in which the dependent variable is equal to 1 if a state chooses the conservative policy and equal to 0 if a state chooses the liberal policy, that is, model (1) with dependent variable (5). The control variables are the same as in Table 3, except that public opinion is measured as the percentage of the population favoring the conservative outcome. The regression in column (A), with a dummy variable for initiative availability, establishes that a difference between initiative and noninitiative states exists to be explained. The point estimate of 0.16 is statistically different from zero at better than the 1 % level, and implies that initiative states are 16 % more likely to choose the conservative policy than noninitiative states. Although the initiative coefficient here is similar to the coefficient in the benchmark regression in Table 3, it is worth noting that there is no mechanical connection between the dependent variables (congruence versus conservative policy choice) in the two tables. One could imagine a data pattern in which the initiative increases congruence but *reduces* the likelihood of a conservative policy. Taken together, the column (A) regressions in Tables 3 and 4 imply that initiative states have more congruent policies than noninitiative states, and this happens because they choose more conservative policies; or, put differently, noninitiative states have less congruent outcomes because their policy choices are too liberal compared to majority opinion in the state. Public opinion is an important explanatory variable: a one percentage point increase in support for the conservative outcome is associated with a 1.8 % greater chance of a conservative outcome being chosen. Income is also highly significant, with wealthy states more likely to choose liberal outcomes.

The regression in column (B) begins the process of investigating why initiative states choose more conservative policies than noninitiative states by adding a dummy variable for state-issues in which a conservative ballot measure was approved by the voters.[24] As before,

[23] For a detailed comparison of citizen ideology in initiative and noninitiative states, see Matsusaka (2004).

[24] I do not consider approved liberal initiatives because the sample contains only two of them.

the coefficient on this variable indicates the direct effect of the initiative, while the coefficient on initiative availability indicates the indirect effect. The coefficients are positive for both direct and indirect effects, but the indirect effect is measured too imprecisely to distinguish from zero. The direct effect is significant at better than the 1 % level, and indicates that an initiative state in which a conservative proposition was approved is 33 % more likely to have a conservative policy than a noninitiative state. The direct and indirect effect coefficients are different from each other at better than the 5 % level ($p = 0.02$). Since noninitiative states choose the conservative policy 47 % of the time (see Table 2), the point estimate implies that an initiative state with a successful conservative initiative chooses a conservative policy 87 % of the time, all else equal. Gerber et al. (2001) and Kousser et al. (2008) show that many initiatives approved by voters do not go into effect because of a court challenge, repeal by the legislature, or failure of the executive to implement it. The estimates in column (B) suggest that attempts to derail approved initiatives are not particularly effective for the issues studied here.

The regression in column (C) attempts to separate the indirect effect into a threat component (given by the initiative availability dummy) and a communication component. To do this, a new variable is introduced that takes the value of $+1$ if a conservative initiative failed at the polls and -1 if a liberal initiative failed at the polls. Several interesting patterns emerge. First, the coefficient on the direct channel remains large, showing a 29 % increase in the probability of a conservative outcome when a conservative initiative was approved. After decomposing the indirect effect into a threat and communication channel, both coefficients are now significantly different from zero. The coefficient on initiative availability implies that the threat of an initiative increases the probability of a conservative law by 10 %. The coefficient on failed initiatives is negative, indicating that failure of a conservative initiative reduces the probability of an initiative state adopting a conservative law by 31 % (and conversely for failure of a liberal initiative). A natural interpretation is that legislatures take an unsuccessful conservative initiative as evidence in support of the liberal position, and move policy in that direction.

To push this finding a little farther, the regression in column (D) takes into account the vote margin for failed initiatives. As discussed above, a conservative initiative that fails with 49 % in favor sends a different message than one that fails with 1 % in favor. The regression adds a variable equal to the approval percentage for each failed initiative, with a positive sign for conservative initiatives and a negative sign for liberal initiatives (hence the variable conceivably takes on values in $[0, 49.9]$ for conservative initiatives and $[-49.9, 0]$ for liberal initiatives). As can be seen, this alternative specification does not materially affect the coefficients on initiative availability and the direct effect dummy. The coefficient on the new variable has the expected sign—a 1 % increase in votes supporting an unsuccessful conservative initiative is associated with a 0.3 % higher probability of a conservative law—but it cannot be distinguished from zero at conventional levels of significance. This could mean that the votes received by an unsuccessful initiative do not matter, but it seems more likely that there are too few observations in this group to generate precise estimates.

The regressions in columns (E) and (F) explore the robustness of the findings to alternative measures of public opinion. The ANES-based issue-specific measure of public opinion is replaced by general ideology measures: column (E) uses the state ideology index of Erikson et al. (1993) and column (F) uses the "citizen ideology" measure of Berry et al. (1998). Both ideology measures have good explanatory power, but neither changes the substantive findings with respect to the initiative variables.[25]

[25] If all three opinion variables are included in the regression at the same time, the coefficient on the ANES measure is significant at the 1 % level, the coefficient on the Erikson et al. measure is significant at the 5 %

The findings on policy choice can be summarized as follows. Initiative states are 16 % more likely to choose a conservative policy than noninitiative states, and this makes policy more congruent with public opinion in initiative than noninitiative states. The initiative has both indirect and direct effects on the policy choice, but the direct effect stemming from approval of a measure is more important than the indirect threat effect. And finally, failed initiatives seem to have an indirect effect on policy choices, but in the opposite direction than is sometimes believed: when voters reject a conservative initiative, the state is less likely to adopt a conservative policy than if the initiative process was not available.

5 Implications

A healthy scholarly literature has found that the initiative process changes outcomes across a number of different policy issues, and several studies have shown that the initiative makes laws more congruent with public opinion, and tilts them in a conservative direction.[26] However, little is known about *how* the initiative brings about policy changes, even though several theoretical channels of influence have been suggested and are often discussed. This article develops an empirical strategy for measuring the impact of three potential channels that have been emphasized in the literature: an indirect "threat" channel, an indirect "communication" channel, and a direct "override" channel. The research strategy is to compare policy choices in states that use only the direct channel (that is, actually approve an initiative), states that use the indirect channels (where the initiative is available but an actual measure is not approved), and states that do not permit initiatives at all, to disentangle the three effects. The study examines policy outcomes on nine high-profile policy issues across all 50 states.

A central finding is that the direct effect is most important, quantitatively and statistically. In all specifications, states that actually pass initiatives on specific issues choose policies that are more congruent with public opinion and more conservative than states where the initiative is available but not used or states where the initiative is unavailable. The point estimates suggest that having a successful initiative on the ballot increases the probability of congruence by 26 % compared to simply having the initiative process available, and increases the probability of a conservative policy choice by 33 %. The large direct effect of the initiative suggests that the ability of groups to undermine an approved ballot measure after the election (for example, through court challenge or legislative repeal) is limited, at least for the issues studied here.

There is also evidence that the initiative affects policy indirectly by providing an opportunity for threats, as suggested by game theoretic models. States that have the initiative process available but do not have any propositions on the ballot are 9 % more likely to choose a congruent policy than noninitiative states, and are 10 % more likely to choose a conservative policy. There is also some evidence, albeit weaker, of a communication or signaling effect from unsuccessful initiatives, but the estimates are imprecise due to the relatively small number of observations in this category. Unsuccessful initiatives do not have a distinguishable effect on congruence, but failure of a conservative initiative makes a state

level, and the coefficient on the Berry et al. measure is not significant ($p = 0.20$). The initiative coefficients are substantively unchanged.

[26] The conservative tilt emerges from studies that cover the last several decades. Research from the early twentieth century finds a liberal tilt associated with the initiative process (Matsusaka 2000).

more likely to adopt a liberal policy outcome, and vice versa. Contrary to the view that interest groups may be able to help themselves by sponsoring a measure even if it fails, the evidence suggests that they would be better off keeping their cards hidden than playing and revealing a losing hand.

These findings confirm a central insight of game theoretic models of the initiative process, that the process can influence policy by allowing a threat without an actual measure on the ballot (Gerber 1996; Matsusaka and McCarty 2001). However, the evidence implies that the threat effect is of secondary importance compared to the direct effect. The evidence rejects perfect information models in which the initiative matters *only* through the threat effect. This suggests that more research would be useful on incomplete information models and, more generally, models in which the initiative matters directly by overriding the legislature rather than by threat alone. Gerber (1998) offers some thoughts on why indirect effects may be difficult for groups to exploit, and further research along these lines seems to be in order.

The evidence also offers ideas for future empirical research. The finding that indirect channels are important for policy choices implies that empirical studies focusing only on the direct channel will miss an important part of the story. Studies that examine only initiatives that reach the ballot or only initiatives that are approved will not capture the threat effect. A natural question is whether the findings on direct versus indirect effects extend beyond the issues investigated in this study, and beyond the American states; a study using Swiss cantons would seem straightforward to implement. The finding that more than one channel is important suggests that it might be worthwhile to refine the empirical approach proposed in this article. The empirical strategy may also be useful in studying other political institutions that are believed to have both direct and indirect effects, such as the executive veto.

It seems appropriate to end with caveats. The study focuses on nine issues that were the subject of questions in the American National Election Studies. Most of these are social issues, and in some cases are included because they are emerging issues. The influence of the initiative on such issues may be different than its influence on more perennial issues such as taxes and spending. While the evidence here represents a first step in disentangling the direct and indirect effects of the initiative, some caution is in order before generalizing beyond these issues and time period studied. The study also focuses on policies that prevail at a particular point in time. As other research has shown, the effect of the initiative may vary over time, both in magnitude and direction (Matsusaka 2004). Finally, confidence in the interpretation of the direct effect coefficient as a causal effect will be enhanced if variation in initiative use can be linked to exogenous factors. A small literature exists that may provide instruments for initiative use (Matsusaka and McCarty 2001; Barankay et al. 2003; Eder et al. 2009) that allow stronger causal interpretations.

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Appendix

Opinion data were collected from three sources: the "state ideology index" (= percentage liberal minus percentage conservative) came from Erikson et al. (1993); and "citizen ideology" and "government ideology" came from Berry et al. (1998), calculated as an average

over the period 1990–1999, as updated in Berry et al. (2007). Issue-specific opinion data were drawn American National Election Studies (ANES), as indicated in Table 1. When a question was asked in multiple years, I combined all responses for a state into a single sample. This pooling approach has been used by Erikson et al. (2003) and Brace et al. (2002); see Erikson et al. (2007) for an overview and justification. For about two-thirds of the state-issues this yielded more than 60 individual responses. For the remaining one-third, the ANES had fewer and in some cases zero individual responses. For these state-issues, I imputed opinion based on the state's general ideology, using coefficients from a regression that employed data from the other states. Specifically, for each issue n, the basic procedure was to estimate a regression $O_{ns}^{ANES} = \alpha + \beta O_{ns}^{BERRY} + u_{ns}$ for those states with reliable opinion information (typically defined as states with 60 or more observations), where O_{ns}^{ANES} is the ANES opinion score for state s and O_{ns}^{BERRY} is the state's general ideology index as constructed by Berry et al. (1998). Then, for states with missing ANES information, ANES scores were imputed using the estimated values of α and β and the state's index value from Berry et al. (2007). Matsusaka (2010) contains more details.

Experts on the ANES will note that my use of the survey goes beyond its intended purposes. Except for the 1988–1992 Senate study, the ANES is designed to be representative at the national, not the state level. This raises questions about the validity of my opinion estimates, particularly for small states where all responses might come from a single region of the state. If responses in a predominantly rural state are drawn exclusively from the state's single metropolitan area, measured opinion may be skewed. Jones and Norrander (1996) report evidence suggesting that the ANES can be aggregated reliably at the state level, at least with large enough sample sizes, but even so, these estimates of citizen preferences are likely to contain significant noise and possibly bias. Brace et al. (2007) argue that the main concern with this type of pooling measure is reliability, the failure to detect real relationships. Thus, there is an argument that the data create a bias against finding a significant result. See the contributions to the Summer 2007 issue of *State Politics and Policy Quarterly* for discussion of the pros and cons of different measures. I have endeavored to estimate my results using alternative measures to ensure that they are not dependent on one particular formulation.

An important feature of the study's measure of congruence is that it is robust to potentially large measurement errors. This is because when calculating congruence, the size of the majority does not matter: congruence is the same if a state's opinion is 55 % or 95 % in favor of a policy. An error in measuring opinion does not affect congruence unless the error is large enough to cause the majority to flip from one side of the issue to the other. For the policies studied, opinion is usually lopsided in favor of one position, meaning that an "in favor" state is unlikely to be erroneously classified as an "opposed" state, and conversely. For the same reason, measurement error in the imputed observations is less troubling than it might seem at first. In short, even though state opinion may be measured with significant error, this should not have a large effect on measured congruence.

A limitation of the ANES opinion data is that the questions asked do not always perfectly match the policy question studied. For example, the ANES question on term limits asks about congressional term limits while the law studied concerns state legislative term limits. The assumption is that individuals favoring term limits for Members of Congress would also favor term limits for state legislators, but this is clearly an approximation. Presumably, errors of this sort would introduce noise into the data that will not create a bias, but rather make it more difficult find statistically significant results.

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