

# Public policy and the initiative and referendum: a survey with some new evidence

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**Abstract** This paper surveys the extensive literature that seeks to estimate the effect of the initiative and referendum on public policy. The evidence on the referendum uniformly finds that requiring voter approval for new spending (or new debt) results in lower spending (or lower debt). The initiative process is associated with lower spending and taxes in American states and Swiss cantons, but with higher spending in cities. The initiative is consistently associated with more conservative social policies. Policies are more likely to be congruent with majority opinion in states with the initiative process than states without the initiative, suggesting that direct democracy allows the majority to counteract the power of special interests in policy making.

**Keywords** Public policy · Initiative and referendum · Direct democracy · Representation

## 1 Introduction

As voters across the globe simmer with populist discontent, direct democracy in the form of initiatives and referendums has become a popular way to give ordinary citizens more influence on policy. Voters have stunned pundits and political elites in a recent series of referendum elections: The United Kingdom's 2016 vote to exit the European Union; Colombia's 2016 vote to reject a peace deal with the FARC rebels; Greece's 2015 vote against the European Union's debt bailout plan; and Italy's 2016 rejection of a constitutional restructuring. In the United States, the number of approved state-level initiatives (47) reached a historical high in 2016, forcing onto the agenda issues that elected officials would rather ignore, such as marijuana legalization, the minimum wage, animal rights, and capital punishment. Voters in Switzerland, with a direct democracy history stretching back

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to the Middle Ages, have been tackling one controversial issue after another, from United Nations membership and limits on construction of mosque minarets, to immigration restrictions and a guaranteed income for all citizens. In the first decade of the twenty first century, 298 national referendums were held across all regions of the world: Africa (35), the Americas (44), Asia (30), Europe (167 referendums), and Oceania (22).<sup>1</sup>

The surge in citizen lawmaking has many causes, among them an increasingly educated citizenry and communication technologies that allow ordinary people to become informed about complicated policy issues (Matsusaka 2005b), but it is also fueled by concerns that governments are overly influenced by special interests and elite opinion. Initiatives and referendums from this perspective are tools that allow the people to regain control of their governments. Yet many questions swirl around direct democracy: Are the voters competent to make public decisions? Or are they too uninformed, emotional, and susceptible to pressure group influence? Should important decisions be left to experts? Or is it healthy to allow voters to override the experts, and keep policy from straying too far from the public's wishes? These questions are particularly important as direct democracy expands worldwide and as reformers search for tools to cure what they see as the ailments of today's democracies.

But before any of these normative questions can be addressed, one needs to understand how the initiative and referendum actually work in practice. What effect, in fact, does popular voting have on the policies that are adopted? Fortunately, more than two decades of concerted research has provided a wealth of evidence on this question. This essay provides a critical survey of that evidence, with several goals in mind. The first goal is to bring together in one place a reference to as much of the existing evidence as I am aware of. By providing a comprehensive list of published research, I hope to enable researchers and policy analysts to find the information they are seeking. A second goal is to summarize the main conclusions from the literature. Because of the selective way that the literature typically is reviewed, sometimes claims are made about the state of knowledge that, to my reading, are inaccurate in important ways. Perhaps most pervasive is the tendency to describe the literature's findings as "mixed". While there surely are conflicting findings, as in any mature literature, some conclusions have been replicated so consistently that it seems safe to treat them as robust patterns. Also, I hope to clarify that some of the "mixed" findings are the result of combining apples with oranges; if institutional differences are taken into account, some apparent discrepancies vanish. A third goal is to highlight areas where the findings appear to genuinely conflict. I note some interesting empirical puzzles that remain to be resolved, and identify what might be productive directions for future research.

The main lessons that emerge from this survey are:

1. The evidence is strong that mandatory referendums on new borrowing or new spending result in lower levels of borrowing and spending, respectively. According to the median estimates, a mandatory referendum on spending is associated with 8% lower spending, and a mandatory referendum on borrowing is associated with 16% less debt. This finding is consistent with theoretical predictions.
2. The initiative process is associated with more conservative fiscal and social policies in American states and Swiss cantons. The initiative process is associated with more spending in cities. Received theory does not give a strong prediction one way or another regarding these differences, so the consistency of the patterns is somewhat puzzling.

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<sup>1</sup> Cross-county data are from Kaufman et al. (2010).

3. Existing evidence, while limited, indicates that the initiative process makes policy more congruent with majority opinion. This accords with theoretical predictions, and implies that special interests are not able routinely to subvert the process for their own benefit.
4. While some studies pay close attention to issues of causality and offer reasonably convincing conclusions, the literature is thin on studies that employ modern methods of causal inference. The findings from the emerging literature that employs modern methods of causal inference typically confirm the findings from the older literature.

To the best of my knowledge, this essay is the first attempt to provide a comprehensive critical survey of the literature on public policy and the initiative and referendum.<sup>2</sup> I use the adjective “critical” here because in surveying the literature, I attempt to assess the work and draw conclusions based on how convincing I find the underlying evidence. That is, this is deliberately not a survey that “counts” every paper equally in drawing conclusions. One of my motivations for writing this essay was dissatisfaction with the practice in the literature of lumping together careful, rigorous evidence that reaches one conclusion with less careful, less rigorous evidence that reaches a different conclusion, and then declaring the existence of a controversy. To avoid this, I exercise judgment in weighing different studies, and explain why (in my opinion) some evidence is more persuasive than others.

### 1.1 Institutional background

Direct democracy is an umbrella term used to describe those forms of democracy that involve the people directly making law as opposed to having laws made by elected representatives. The idea of citizens participating directly in important public decisions is probably as old as human societies, and historical examples go back to the ancient Greeks and Romans, and the Swiss *Landsgemeinde* in the Middle Ages. In terms of modern democracies, Americans used town meetings to make policy decisions even before the United States was formed, and the state of Rhode Island held a referendum on adopting the U.S. Constitution in 1788. The highest-octane form of direct democracy, the initiative process, originated in Swiss cantons in the 1830s, was incorporated at the federal level in the Swiss constitution of 1848, and then crossed the Atlantic to American states and cities beginning in the 1890s.

The form of direct democracy and the terminology used to describe it vary across and even within countries. For the purposes of this essay, I define the *referendum* to be a process by which citizens vote on a policy proposed by government officials, and the *initiative* to be a process by which citizens vote on a policy proposed by the citizens themselves. Both processes result in “ballot propositions” or “ballot measures”, policy proposals that appear on the ballot for approval or disapproval.

Referendums<sup>3</sup> can be further divided into three main types:

- *Mandatory referendum*<sup>4</sup> A mandatory referendum is a vote on a government policy proposal that is required by law for the proposal to go into effect. For example: Constitutional amendments require voter approval in most American states, and at the federal and cantonal levels in Switzerland. Many American states require bond issues

<sup>2</sup> For broader surveys of direct democracy, see Lupia and Matsusaka (2004) and Matsusaka (2005a).

<sup>3</sup> Following standard practice, and the *Oxford English Dictionary*, I use *referendums* as the plural rather than *referenda*.

<sup>4</sup> Also called “compulsory” and “obligatory” referendum.

to be approved by voters. Most Swiss cantons require new spending programs to be approved by voters. California requires voter approval for any new taxes or tax increases at the state or local level. Switzerland requires a national vote to join an international organization. There are also idiosyncratic examples, such as the city of San Diego's requirement of voter approval for all new property developments in certain parts of the city.

- *Petition referendum*<sup>5</sup> A petition referendum is a vote on a government proposal that takes place as a result of a citizen petition. Typically, citizens have a certain number of days to collect a predetermined number of signatures from fellow citizens and, if successful, the electorate votes to keep or reject the policy. The petition referendum plays an important role in Switzerland, where citizens are allowed to veto laws at the federal and cantonal levels, and in Italy where citizens can repeal existing laws. The petition referendum is widely available at the state and local level in the United States.
- *Advisory referendum* An advisory referendum is a vote on a government proposal that is called at the request of the government. Its results are not binding formally on the government. Recent examples are Brexit in the United Kingdom and the FARC Treaty in Colombia.

The initiative process also relies on petitions to bring a proposal to a vote, but unlike the petition referendum, the initiative allows citizens themselves to propose the policy that will be put to a vote. The sponsors must collect a predetermined number of signatures within a specific time frame to qualify their proposal for the ballot. For example, California requires signatures equal to 8% of the votes cast in the previous gubernatorial election in order to propose a state constitutional amendment, and Switzerland requires a flat 100,000 signatures to propose an amendment to the federal constitution. Perhaps the most famous initiative historically is California's tax-cutting Proposition 13 in 1978 that sparked a national tax revolt.

Tables 1 and 2 provide summary information on direct democratic institutions in the United States and Switzerland, the two most active direct democracy countries in the world. Table 1 summarizes initiative and referendum provisions in the American states. All but one of the 50 states have some form of direct democracy. Twenty-four states require voter approval (mandatory referendum) on debt issues,<sup>6</sup> 49 states require voter approval on constitutional amendments, 23 states allow petition referendums, and 24 states allow initiatives. The first state to adopt the initiative process was South Dakota in 1898, and the first vote was held in Oregon in 1904.

Figure 1 shows the ebb and flow of initiative use over time at the state level in the United States. The figure displays the number of initiatives on the ballot, and the number that were approved. Initiative activity surged in the early twentieth century during the Progressive movement, receded midcentury, and then resurged in the 1970s, triggered in part by Proposition 13. Over the 1904–2017 period, a total of 2551 initiatives appeared on state-level ballots. California and Oregon have voted on more initiatives than any other state, followed by Colorado, North Dakota, and Washington.<sup>7</sup> No systematic data are available on the number of local initiatives; the total number is probably an order of magnitude greater than the number of state initiatives.

<sup>5</sup> Also called "optional" or "popular" or "veto" referendum.

<sup>6</sup> And at least 10 other states have flat limits on the aggregate amount of debt, meaning that exceeding the debt limit requires popular approval in the form of a constitutional amendment.

<sup>7</sup> Summary information from Initiative and Referendum Institute (2017).

**Table 1** Initiative and referendum in the American states

State	Referendum			Initiative
	Petition	Mandatory: debt	Mandatory: constitutional amendment	
Alabama	–	–	X	–
Alaska	X	X	X	X
Arizona	X	–	X	X
Arkansas	X	X	X	X
California	X	X	X	X
Colorado	X	X	X	X
Connecticut	–	–	X	–
Delaware	–	–	–	–
Florida	–	X	X	X
Georgia	–	–	X	–
Hawaii	–	–	X	–
Idaho	X	X	X	X
Illinois	–	X	X	X
Indiana	–	–	X	–
Iowa	–	X	X	–
Kansas	–	X	X	–
Kentucky	–	X	X	–
Louisiana	–	–	X	–
Maine	X	X	X	X
Maryland	X	–	X	–
Massachusetts	X	–	X	X
Michigan	X	X	X	X
Minnesota	–	–	X	–
Mississippi	–	–	X	X
Missouri	X	X	X	X
Montana	X	X	X	X
Nebraska	X	–	X	X
Nevada	X	–	X	X
New Hampshire	–	–	X	–
New Jersey	–	X	X	–
New Mexico	X	X	X	–
New York	–	X	X	–
North Carolina	–	X	X	–
North Dakota	X	–	X	X
Ohio	X	–	X	X
Oklahoma	X	X	X	X
Oregon	X	–	X	X
Pennsylvania	–	X	X	–
Rhode Island	–	X	X	–
South Carolina	–	–	X	–
South Dakota	X	–	X	X
Tennessee	–	–	X	–

**Table 1** continued

State	Referendum			Initiative
	Petition	Mandatory: debt	Mandatory: constitutional amendment	
Texas	–	X	X	–
Utah	X	–	X	X
Vermont	–	–	X	–
Virginia	–	X	X	–
Washington	X	–	X	X
West Virginia	–	–	X	–
Wisconsin	–	–	X	–
Wyoming	X	X	X	X

This table lists initiative and referendum provisions in the American states. A referendum is a vote for or against a proposal from the government: “petition” means it comes to the ballot by citizen petition; “mandatory” means it is automatically placed on the ballot. Two types of mandatory referendums are listed, for bond issues and for constitutional amendments. An initiative is a vote on a citizen-proposed policy. Classifications were constructed by consulting state constitutions. A state was classified as having a mandatory referendum on bond issues if it required a vote to issue bonds under any circumstance or if it required a vote to exceed constitutional restrictions. States that implicitly require a constitutional amendment to borrow but do not mention popular approval for borrowing are not classified as having a mandatory referendum

Table 2 summarizes direct democracy provisions in Swiss cantons. All but one canton either requires a referendum or allows a referendum by petition on new spending programs above a certain threshold. All 26 of the cantons allow initiatives. Two cantons employ direct democracy in the form of town meetings.

Figure 2 shows the number of national votes in Switzerland over time, including both initiatives and referendums. The figure shows a somewhat dormant process until the 1970s, when activity shot up, peaking in the 1990s. The surge in initiative activity beginning in the 1970s and continuing to the present took place both in the United States and in Switzerland, suggesting that it might have been caused by secular changes in the world rather than developments specific to either country.

Looking across nations, referendums are common, and much more prevalent than initiatives. A recent survey of legal institutions found that 57% of countries required national referendums for some issues, and 24% allowed initiatives on national issues. In terms of use, since 1980 a remarkable 82% of countries have held a national referendum to resolve at least one public issue.<sup>8</sup>

## 2 Theory

Almost all theoretical work on the policy effects of direct democracy employs a spatial model, following the pioneering work of Romer and Rosenthal (1979a), which studied the mandatory referendum.<sup>9</sup> The model was extended to include initiatives by Gerber (1996)

<sup>8</sup> Numbers are from the Direct Democracy Database maintained by the International Institute for Democracy and Electoral Assistance, available at <https://www.idea.int/data-tools/data/direct-democracy>.

<sup>9</sup> Kessler (2005) and Besley and Coate (2008) are two interesting studies using non-spatial models.

**Table 2** Direct democracy in Swiss cantons. Adapted from Feld and Matsusaka (2003), and updated by consulting cantonal web sites

Canton	Mandatory referendum: new spending	Petition referendum: new spending	Initiative	Town meeting
Aargau	–	Yes	Yes	–
Appenzell ER	–	Yes	*	Yes
Appenzell IR	Yes	Yes	*	Yes
Basle City	–	Yes	Yes	–
Basle County	–	Yes	Yes	–
Bern	–	Yes	Yes	–
Fribourg	Yes	–	Yes	–
Geneva	–	Yes	Yes	–
Glarus	Yes	–	*	Yes
Grisons	Yes	Yes	Yes	–
Jura	Yes	Yes	Yes	–
Lucerne	Yes	Yes	Yes	–
Neuchatel	Yes	Yes	Yes	–
Nidwalden	Yes	Yes	Yes	–
Obwalden	Yes	–	Yes	–
St. Gallen	Yes	Yes	Yes	–
Schaffhausen	Yes	Yes	Yes	–
Schwyz	Yes	–	Yes	–
Solothurn	Yes	Yes	Yes	–
Thurgau	Yes	Yes	Yes	–
Ticino	–	Yes	Yes	–
Uri	Yes	Yes	Yes	–
Valais	–	Yes	Yes	–
Vaud	–	–	Yes	–
Zug	–	Yes	Yes	–
Zurich	Yes	Yes	Yes	–

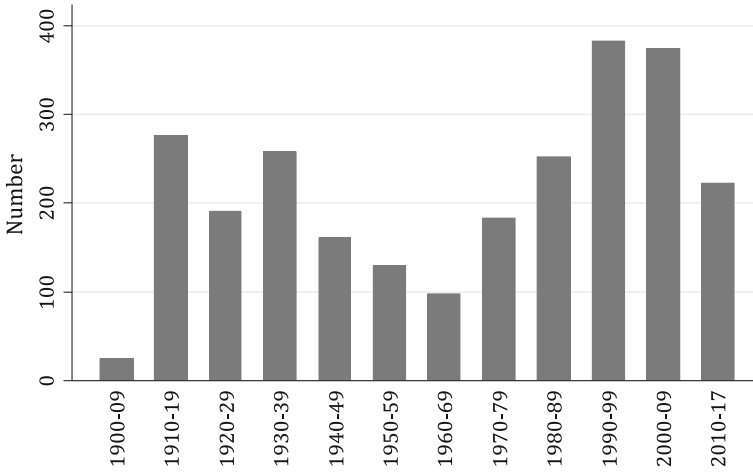
This table lists initiative, referendum, and town meeting provisions in Swiss cantons

\* In cantons with a town meeting form of government, citizens can make proposals at the meeting

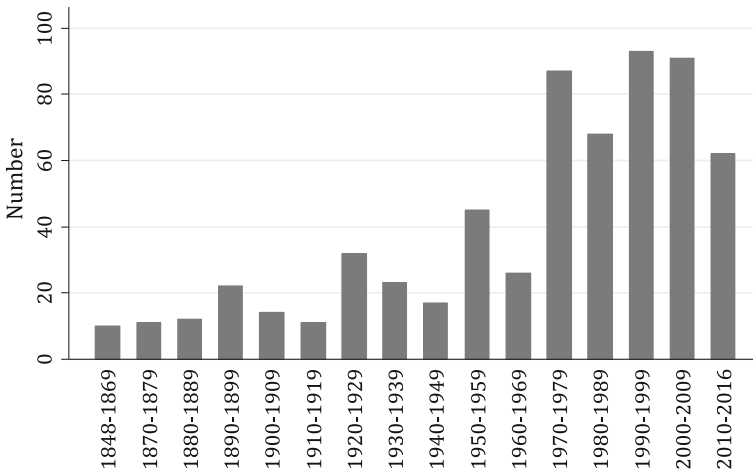
and Matsusaka and McCarty (2001). Here I present a simple version of these models that produces several insights that are important for empirical work.

In thinking about the effect of direct democracy, one has to start by asking: compared to what? A prediction that direct democracy (say) reduces taxes presupposes a baseline level of taxes that hypothetically would occur without direct democracy. The standard baseline is the policy that would prevail under a pure representative democracy. Also, it is important to recognize that the initiative and referendum never completely replace representative government, but are always grafted on top of representative institutions. One insight from the theoretical literature is that direct democracy's effect on policy comes to a large degree by changing the behavior of representatives.

A scalar policy  $x$  is to be chosen, with  $x = 0$  the status quo point. There are two actors: voters, represented as a unitary agent (e.g., the median voter); and the government, also



**Fig. 1** State initiatives in United States. *Source:* Initiative and Referendum Institute



**Fig. 2** Swiss National initiatives and referendums. *Source:* Swiss Federal Chancellery

represented as a unitary agent. The voter has a single-peaked utility function  $u(x)$  with an “ideal point” (peak) at  $x = V$ , and the government has a single-peaked utility function with an ideal point at  $x = G$ . In a world with no direct democracy, the government would choose its ideal point, and the policy would be  $x = G$ . In a pure median voter world, the government would have the same preferences as the voter,  $G = V$ , would set policy at the voter’s ideal point, and direct democracy would be irrelevant. When  $G \neq V$ , which both theory and empirical research suggests can happen, direct democracy can affect the policy outcome.<sup>10</sup>

<sup>10</sup> There are many reasons why median voter outcomes might not prevail: The pressure group models of Stigler (1971) and Peltzman (1976) show how policy responds more to preferences of organized groups; the Downsian model fails to produce convergence to the median when the issue space is multidimensional, there are more than two candidates, candidates are policy motivated, or there is a valence dimension; and the



## 2.1 Mandatory referendum

Consider the preference configurations in Fig. 3. Here the voter's and government's ideal points are to the right of the status quo. With no referendum, the government chooses its ideal point  $x = G$ . When a referendum is required, the government proposes policy  $x_G$  that is subject to voter approval. If the government's proposal is rejected, the policy reverts to the status quo,  $x = 0$ . The voter will approve the government's proposal only if it yields a higher utility than the status quo.

If Case 1a, the voter prefers any proposal in the region  $(0, x_{max}^{MR})$  compared to the status quo. This acceptance zone is narrow enough to constrain the government: instead of proposing  $x_G = G$ , which the voter would reject, the government proposes  $x_G = x_{max}^{MR}$ , which the voter accepts. A referendum requirement in this case would change policy. In Case 1b, the mandatory referendum has no effect: the zone is too wide to constrain the government. The government proposes its ideal point, which the voter accepts. Case 1c flips the ordering of government and voter preferences so that the government's ideal point is closer to the status quo than the voter's ideal point. Again, the mandatory referendum has no effect because the voter will accept the government's ideal point.

This example illustrates several properties of the mandatory referendum. First, although the referendum gives power to the voters, it does not enable them to bring policy all the way to their ideal point. Because the government retains control of the agenda, it can act strategically to keep the policy near its own ideal point. Second, although the referendum does not allow the voter to fully control the outcome, in equilibrium the policy is (weakly) closer to the voter's ideal point than if a referendum was not required. Third, the referendum has an effect on policy even though no proposal is rejected in equilibrium. In Case 1a, the referendum restricts policy because of the threat it exerts; the government moderates its proposal in order to avoid being rejected. This is important for empirical research because it implies that the effect of the referendum cannot be inferred by studying only policies that actually go to a vote.

Finally, the model generates an important prediction concerning the direction of the policy effect. If the mandatory referendum is such that the status quo is  $x = 0$ —that is, rejection of the government's proposal results in nothing happening—then the mandatory referendum (weakly) reduces the overall level of  $x$  compared to what would prevail under pure representation. More concretely, this implies that requiring voter approval for new spending or new borrowing will reduce overall spending and borrowing, respectively.<sup>11</sup>

## 2.2 Petition referendum

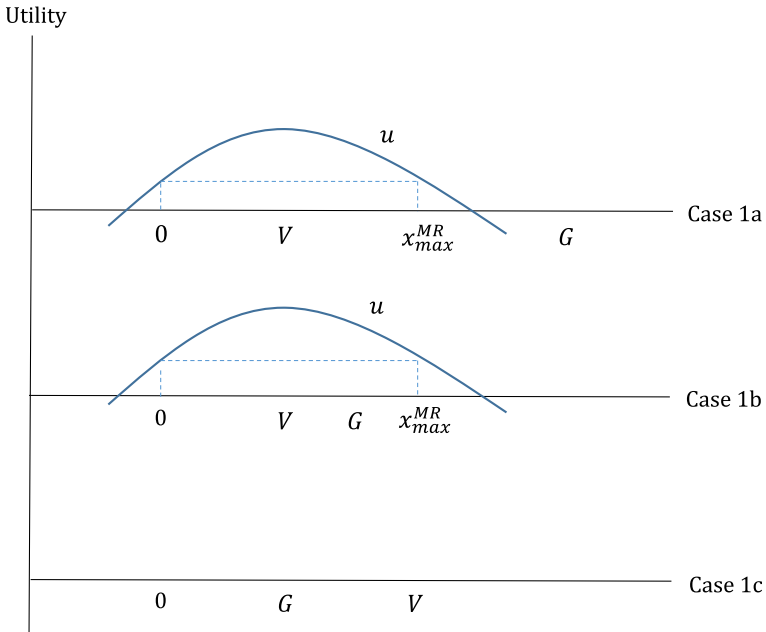
This form of direct democracy is similar to the mandatory referendum except that now the voter must pay a cost  $c > 0$  in order to call an election. The cost represents expenditure of time and money associated with collecting signatures on petitions.

Figure 4 illustrates one configuration of preferences. The difference between the mandatory and petition referendum is that the acceptance zone is wider for the petition referendum. To see this, note that the voter's utility from triggering a referendum that

Footnote 10 continued

shirking models of Barro (1973) and Ferejohn (1986) show that elections put pressure on representatives to follow voter preferences, but not enough to cause them to entirely forego their own policy preferences.

<sup>11</sup> Note that a mandatory referendum on spending cuts would have the opposite effect—leading to higher spending.



**Fig. 3** Model of mandatory referendum

reverts to the status quo is  $u(0) - c$  because of the petition cost that must be paid to override the status quo. The same utility level can be produced with a policy  $x$  that solves  $u(x) = u(0) - c$ , indicated as  $x_{max}^{MR}$  in the figure. Because  $x_{max}^{MR} < x_{max}^{PR}$ , the government can deter a petition referendum with a less accommodating policy choice than when the referendum is mandatory. Other than this difference in degree, the qualitative policy predictions are the same for the petition referendum as for the mandatory referendum.

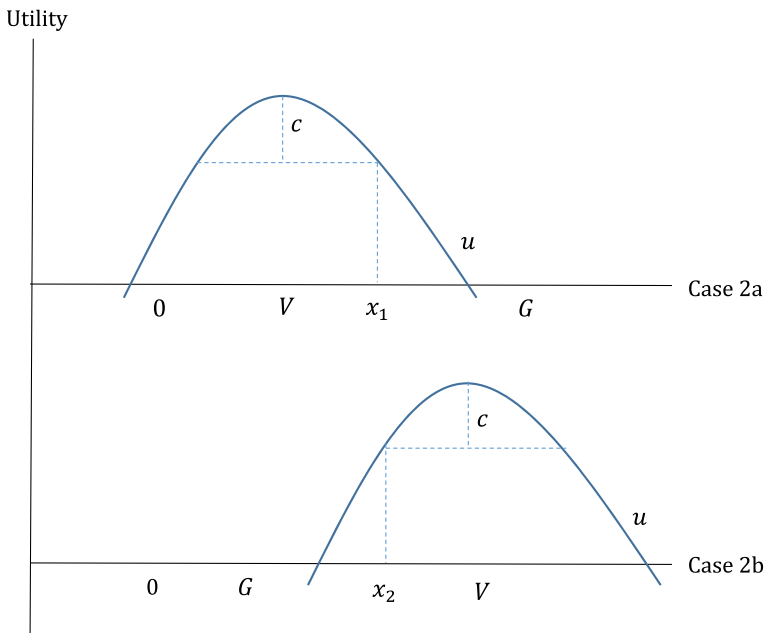
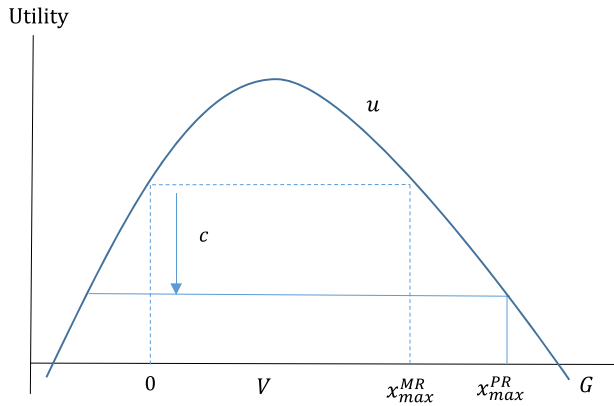
### 2.3 Initiative

The initiative differs from the referendum in that it allows the voter to make proposals; it removes the government’s monopoly control of the agenda. To capture this process, we model the sequence of actions as follows: first, the government chooses a policy, taking into account the possibility of a future initiative and, second, the voter at a cost of  $c > 0$  can choose to propose an initiative with the policy at the voter’s ideal point. Again, the cost includes time and effort allocated to petitioning.

Consider the examples in Fig. 5. Suppose, for a moment, that the government were to choose its ideal point  $x = G$ . Then the voter would launch an initiative and override the policy if  $u(V) - c > v(G)$ . If it would be optimal for the voter to proceed with an initiative, then the government—in anticipation—would choose an accommodating policy that deters the initiative. Specifically, the government would choose  $x_1$  that solves  $u(V) - c = u(x_1)$ . In case 2a, where  $V < G$ , the equilibrium policy outcome is  $x = \min \{x_1, G\}$  when the initiative is available. The analysis is symmetric in Case 2b where  $G < V$ : the equilibrium policy outcome is  $x = \max \{x_2, G\}$ .<sup>12</sup>

<sup>12</sup> For example, Case 2a might represent a tax increase, and Case 2b might represent an increase in the minimum wage.

**Fig. 4** Model of petition referendum



**Fig. 5** Model of the Initiative

Several implications follow, some of which echo the analysis of the mandatory referendum. First, the initiative influences policy, but it does not bring the outcome to the voter’s ideal point. The distance between the final policy and the voter’s ideal point is increasing in the cost of initiating a proposal. In practice, the cost of drafting a proposal and collecting signatures can be substantial, for example, well over \$1 million in California and Ohio. Second, while the initiative does not give the voter everything the voter wants, it does bring policy closer to the voter’s ideal point than if the initiative was unavailable. Third, the effect of the initiative is indirect in the sense that policy changes come about not by the voter approving a proposition but by the government adjusting policy in anticipation

of a proposition. Empirically, this implies that the effect of the initiative cannot be inferred by examining only those propositions that actually appear on the ballot.

Finally, and in contrast to the mandatory referendum, no directional prediction concerning the effect of the initiative emerges without introducing auxiliary assumptions. In Case 2a, the final policy is (weakly) smaller when the initiative is available. In Case 2b, the final policy is (weakly) larger when the initiative is available. Thus, there is no theoretical reason to expect that, say, the initiative reduces taxes versus increases taxes. The direction of the effect depends on the relative ideal points of the government and voter. To produce a directional prediction requires a way of measuring the ideal points of the voter and government, or an auxiliary theory that implies a particular configuration of preferences (see below for examples). The dependence of the effect of the initiative on the relative ideal points of the key actors is important in assessing the empirical literature.

## 2.4 Discussion

The model sketched here leads to several theoretical implications that are relevant for empirical research.

1. The effect of the initiative and referendum is indirect, potentially to a large degree. Policy may change not because voters approve a proposition, but because the threat of a proposition causes the government to choose a different policy. Put differently, the initiative and referendum matter simply by being available, even if they are not used. The important implication for empirical research is that one cannot measure the effect of the initiative and referendum solely by examining measures that come to a vote.<sup>13</sup>
2. The effect of the referendum is to curtail the policy that is subject to the referendum. Thus, there is an unambiguous directional prediction: when referendums are required on new spending and new debt, the overall levels of spending and debt will be lower than otherwise.
3. The effect of the initiative is to push policy toward the ideal point of the voters, but this can cause the outcome to be more to the “left” or “right” than it would be absent the initiative. Empirically, this means that there is not a directional prediction for the effect of the initiative, absent knowledge of voter and government preferences.
4. The effect of initiatives and referendums are not the same. This means that combining separate indicators of initiative and referendum rights into a “direct democracy” index lacks theoretical justification, and there is no coherent way to interpret such indexes under existing theory.

The model sketched herein is simplified to illustrate certain basic forces. One important limitation is that in equilibrium no initiatives or referendums go to a popular vote; they are always deterred by accommodation from the government. This counterfactual implication is sensitive to the assumption of complete information. If there is some uncertainty about the election outcome, then some proposals will end up on the ballot (Matsusaka and McCarty 2001). With uncertainty, the indirect effect highlighted in implication 1 above continues to appear, but a direct effect also is possible; and implications 2 and 4 continue to hold. Implication 3, however, changes: the property that the initiative always pushes policy (weakly) toward the position of the voter may or may not hold. Intuitively, if there is uncertainty about the election outcome, the government may accommodate an extreme

<sup>13</sup> Matsusaka (2014) develops an empirical strategy to quantify the sizes of the direct and indirect effects, and finds evidence suggesting that the direct effect is larger than the indirect effect, at least for American states.

interest group by adopting an even more extreme policy than it would otherwise have chosen in order avoid the risk of an election, moving policy away from the median voter's position.<sup>14</sup>

### 3 Empirical approaches

The goal of the literature covered in this survey is to estimate the effect of direct democracy on policy outcomes. Establishing causality is difficult in the social sciences, and especially so when studying institutions that (almost by definition) are highly stable over time. Much of the early literature was produced before development of modern methods of causal inference; it seeks to establish causality using a combination of methods, including correlations, extensive controls for potential confounding factors, theoretical justifications, and anecdotal and qualitative evidence. Strengths and weaknesses of the arguments are discussed below.

The main strategy in the literature is, roughly speaking, to compare policy outcomes in jurisdictions with and without direct democracy. Jurisdictions without direct democracy then serve as the counterfactual—the policy outcome if the jurisdiction does not allow direct democracy—and the policy difference is interpreted as the effect of direct democracy. The literature's workhorse empirical model is a regression of the form

$$Y_{it} = \alpha + \beta D_{it} + \gamma X_{it} + u_{it}, \quad (1)$$

where  $i$  indexes a jurisdiction (state, canton, city),  $t$  indexes time, and  $\alpha$ ,  $\beta$ , and  $\gamma$  are parameters to be estimated. The variable  $Y_{it}$  is a policy measure, such as the amount of government spending. The variable of interest is a dummy  $D_{it} = 1$  if jurisdiction  $i$  at time  $t$  has direct democracy available, and  $D_{it} = 0$  otherwise (or  $D_{it}$  can be a vector describing several dimensions of the institution). The vector  $X_{it}$  includes control variables. The coefficient  $\beta$  is intended to capture the effect of direct democracy on policy, although in some cases it is better seen as simply the mean difference between jurisdictions with and without direct democracy, conditional on  $X_{it}$ . When the policy is represented by an indicator variable, such as permitting versus banning capital punishment, logit or probit versions of (1) are often used. A few studies frame their analysis in terms of policy adoption, and estimate hazard models (also called “duration” or “event history” models depending on the field).

Regression (1) presents several challenges that the literature has addressed in various ways. The regression assumes that the effect of direct democracy is the same for every jurisdiction and every time. This is a strong assumption because the implementing details of initiative and referendum laws vary across jurisdictions in ways that could influence their impact. For example, jurisdictions might have different signature requirements, making it easier in some places to qualify measures for the ballot (variation in  $c$  in terms of the model). Typically, researchers address this concern by conditioning the direct democracy effect on institutional details of importance. For example, in Matsusaka (1995), the initiative is characterized by a dummy variable for availability and the dummy variable interacted with the signature requirement; in Feld and Matsusaka (2003), the mandatory referendum on spending is characterized by a dummy indicating availability and a spending threshold term interacted with the dummy. Our understanding of which

<sup>14</sup> Matsusaka and Ozbas (2017) show how this property emerges under fairly general conditions.

institutional features are most important is reasonably well advanced: see Bowler and Donovan (2004) for an extensive analysis of the initiative process.

Another challenging issue pertains specifically to the initiative process: theory predicts that the direction of the effect ( $\beta$ ) depends on the configuration of preferences. For example, if the government prefers more spending than the voter, theory predicts that the initiative reduces spending, while if the government prefers less spending than the voter, theory predicts that the initiative increases spending. Even if measures of government and voter preferences are available, simply including them in  $X_{it}$  does not solve this problem because it is their *relative* positions that matter. Nevertheless, as will be shown below, much of the literature finds a systematic directional effect of the initiative, independent of the configuration of preferences, which is something of a puzzle. Efforts to control directly for the configuration of preferences are discussed under “congruence” below.

A fundamental concern with regression (1) is that because availability of direct democracy is not randomly assigned—jurisdictions choose whether or not to adopt it or are “endowed” with it for historical or cultural reasons—the risk of spurious correlation is significant. Regression (1) in effect considers direct democracy jurisdictions to have been “treated” and uses the other jurisdictions as the “untreated” control group. This yields valid causal estimates only if the conditional potential values of the treated and control group are the same. In less formal language, regression (1) produces causal estimates only if the direct democracy jurisdictions would have had the same policy as the non-direct democracy jurisdictions in the absence of the direct democracy “treatment”. While the risk of spurious correlation is present in every study, even those with fully random assignment (Leamer 2010), it is much more than hypothetical in this context because direct democracy availability varies across jurisdictions in systematic ways (for example, initiative states are more likely than non-initiative states to be in the Western part of the United States), creating a real possibility that direct democracy and non-direct-democracy states may differ in ideology, political culture, and other factors that drive policy choices, but are not easily controlled in regressions. The details of how various studies attempt to rule out spurious correlation is critical in interpreting their findings.

## 4 Empirical evidence

This section reviews the empirical literature connecting the initiative and referendum to policy outcomes. The literature can seem chaotic on first impression; one purpose here is to organize the evidence and highlight the existence of common patterns. While I reviewed every paper of which I am aware in preparing this summary, I chose to exclude a few studies that left too many questions about execution to give confidence in the findings.<sup>15</sup>

<sup>15</sup> Specifically: First, for the most part I have excluded working papers, on the principle that their findings have not yet undergone peer review. This is with regret, since some of these studies employ interesting and reasonably convincing methods of causal identification. Second, I have excluded studies that compare mean policy outcomes between jurisdictions without any control variables, because theory strongly suggests that controls for preferences need to be included. Third, I have excluded studies that estimate the effect of the initiative using interaction terms, but do not present estimates of the net effect of the initiative, or do not provide enough evidence to infer the net effect. Fourth, I omitted studies that rely entirely on a direct democracy index because it is not possible to separate initiative and referendum effects and thus lack theoretical coherence, as discussed in Sect. 4. Finally, I have excluded a small number of studies with findings that are known to be spurious based on subsequent research or that employ methods that are problematic.

## 4.1 Fiscal policy: referendum

Table 3 lists 16 studies that have examined the relation between the referendum and fiscal policy. Almost all of these studies focus on mandatory referendums. Recall that theory predicts that a mandatory referendum on new actions results in a lower level of the action. The evidence is strikingly consistent with this prediction.

Panel A of Table 3 lists 10 studies that examine referendums on public spending. The studies cover New York school districts, Swiss cantons, Swiss cities, and cross-national samples. In all cases, the referendums are mandatory. The referendums apply to new spending in Switzerland, to the annual budget in New York school districts, and to any topic in the cross-national sample. All of the studies find lower spending or taxes when voter approval is required, with the estimates ranging from  $-2$  to  $-19\%$ . The median estimate is  $-8\%$ , a sizable number. For New York school districts, failure of a referendum does not result in zero spending but rather a default budget that incorporates an increase from the previous year's spending, so theory does not necessarily predict lower spending; nevertheless, the evidence shows less spending, albeit of a smaller magnitude. Blume et al. (2009) and Blume and Voigt (2012), which find lower spending in countries with a mandatory referendum at the national level, also find lower welfare spending in those countries, suggesting in part how the lower spending is achieved.

Panel B of Table 3 lists six studies that examine referendums on borrowing. The studies cover American states, Swiss cities, and cross-national samples. The referendums in question are mandatory except for the Swiss studies, which do not distinguish between mandatory and petition referendums. All of the studies find that debt and deficits are lower when borrowing must be approved by the voters. The differences for debt range from  $-10$  to  $-25\%$ , with a median value of  $-16\%$ . Again, the magnitude is substantial. Feld and Kirchgässner (1999), which finds 25% less debt in Swiss cities with a mandatory referendum on a deficit, reports that the smaller deficit is accomplished by spending less and collecting more revenue.<sup>16</sup>

A natural concern with this evidence is the possibility that jurisdictions with referendums are more fiscally conservative to begin with, and that their lower spending and borrowing are manifestations of their conservatism rather than the referendum itself. For example, one might conjecture that jurisdictions with fiscally conservative citizens are more likely to adopt mandatory referendums (although one could also argue that profligate jurisdictions are more likely to do the same in order to curb excessive spending and borrowing). The studies address this issue to varying degrees by employing demographic and political controls that are likely to be correlated with citizen preferences. Among the more convincing efforts are Funk and Gathmann (2011, 2013b), which estimates Swiss canton-level voter preferences using data on federal election results; the studies find that referendum cantons are indeed more fiscally conservative, but that the difference in spending remains even after controlling for cantonal preferences. A different, but also relatively convincing approach, is employed in Nguyen-Hoang (2012), which uses a difference-in-difference framework that exploits a change in New York state law that forced some but not all school districts to adopt a mandatory referendum. Also, it should be kept in mind that most referendum provisions were in place long before the sample period, in some cases more than a century earlier; even if the adopters had fiscally conservative

<sup>16</sup> Four studies estimate the relation between borrowing and mandatory referendums on spending (Blume et al. 2009; Blume and Voigt 2012; Luechinger and Schaltegger 2013; Burret and Feld forthcoming). They generally fail to find a statistically significant relation.

**Table 3** Empirical studies of the referendum and fiscal policies

Study	Jurisdiction	Period	N	Referendum subject	“Effect” of referendum
<i>Panel A. Expenditure and tax revenue</i>					
Ebdon (2000)	New York school districts	1990	465	Annual budget	– 5.5% expenditure*
Nguyen-Hoang (2012)	New York school districts	1990–2000	3817	Annual budget	– 2% expenditure*
Feld and Matsusaka (2003)	Swiss cantons	1980–1998	494	New spending	– 19% expenditure*
Freitag and Vatter (2006)	Swiss cantons	1990–2000	275	New spending	Lower revenue*
Funk and Gathmann (2011)	Swiss cantons	1890–2000	2395	New spending	– 8 to – 12% expenditure*; – 7% revenue
Funk and Gathmann (2013b)	Swiss cantons	1950–2000	1272	New spending	– 14% expenditure*; – 12% revenue*
Blume et al. (2009)	Countries	1990s	62	Unclear	Lower expenditure*
Blume and Voigt (2012)	Countries	2008	94	Unclear	Lower expenditure*
Galetta and Jametti (2015)	Swiss cities	1993–2007	1782	New spending	– 8% expenditure*
Burret and Feld (forthcoming)	Swiss cantons	1980–2011	832	New spending	Lower expenditure*, lower revenue*
<i>Panel B. Debt and deficits</i>					
McEachern (1978)	U. S. states	1974	50	New debt	Lower debt*
Bohn and Inman (1996)	U. S. states	1970–1991	987	New debt	+ 44% surplus*
Kiewiet and Szakaly (1996)	U. S. states	1961–1990	1421	New debt	– 10% debt*
Feld and Kirchgässner (1999)	Swiss cities	1990	131	Deficit <sup>a</sup>	– 25% debt*
Feld and Kirchgässner (2001)	Swiss cities	1990	137	Deficit <sup>a</sup>	– 15 to – 17% debt*
Feld et al. (2011)	Swiss cities	2004	134	New debt <sup>a</sup>	– 13 to – 19% debt*

This table summarizes published studies estimating the relation between fiscal policy and availability of the referendum. N is the number of observations in the main equation. “Referendum subject” is the fiscal object (e.g., spending level) that is subject to voter approval; “unclear” means the study did not specify the subject of the referendum. The referendum was mandatory except where indicated with a superscript <sup>a</sup> in which case the study did not distinguish between mandatory and petition referendums. Referendum “effect” is the difference in fiscal policy between referendum and non-referendum jurisdictions, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1), expressed as a percentage. The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better



preferences, the current citizens are entirely different people and may have completely different preferences than their forebears. Even though the evidence of individual studies leaves some questions regarding causality, the fact that such a robust pattern appears across so many studies covering different jurisdictions and time periods—and is consistent with received theory—lends support to a causal interpretation.

## 4.2 Fiscal policy: Initiative

When it comes to the initiative process, theory does not produce an unconditional prediction for the direction of the effect. If voters are more fiscally conservative than government officials, the initiative is predicted to drive spending down, while if voters are more fiscally liberal than government officials, the initiative drives spending up. In short, the initiative can raise or lower spending. We might expect, then, not to find a consistent connection between the initiative and fiscal policy outcomes.

It is somewhat surprising, therefore, that most of the evidence finds systematically different expenditure levels between jurisdictions with and without the initiative. Table 4 lists the extant published literature. Panel A focuses on fiscal policy in American states and Swiss cantons over the last half century, perhaps the best-known set of findings. Exploiting cross-sectional variation in initiative availability across American states, several studies find that initiative states spend and tax about 5% less than non-initiative states on average. Because all Swiss cantons allow initiatives, the Swiss studies compare fiscal policy in cantons with low versus high signature requirements, based on the idea that initiatives are easier to use and, hence, more effective when signature requirements are low. Swiss cantons with low signature requirements spend and tax less than those with high signature requirements, on average. These differences cannot be explained by different citizen ideology in initiative and non-initiative states or cantons, despite extensive efforts to control for ideology (Matusaka 2004; Funk and Gathmann 2011).

Panel B of Table 4 focuses on cities. Here we see a consistent pattern as well, but it goes in the opposite direction: cities with the initiative spend more than cities without it. The evidence from American cities is based primarily on cross-sectional variation. The evidence from German cities is produced by a variety of methods: Blume et al. (2011) uses difference-in-difference methods to compare German cities that were given initiative rights in 1990s with cities that already had initiative rights; Asatryan (2016) uses variation across cities in signature requirements; and Asatryan et al. (2017a) uses regression discontinuity methods based on the fact that signature requirements vary based on population thresholds.<sup>17</sup> In Germany, local initiatives are prohibited by law from considering budgetary matters, but they are allowed to propose or cancel programs.

These findings are somewhat puzzling in light of received theory.<sup>18</sup> If government and voter preferences were drawn at random and government preferences tended to track voter preferences on average, we would expect  $G = V$  on average. Policy differences would appear between initiative and non-initiative jurisdictions that happened to have  $G \neq V$ , but

<sup>17</sup> To be precise, Asatryan (2016) uses the signature requirement as an instrument for the use of initiatives. For the purposes of this survey, I interpret those findings to be based on variation in signature requirements, although those specific results are not reported in the article.

<sup>18</sup> The two papers in Panel C of Table 4 using international evidence provide somewhat contradictory evidence, but the papers contain little basis for determining whether the differences are explained by different sample periods, different definitions of initiatives, or something else. These papers also contain fairly weak controls for citizen ideology, culture, and similar factors that might generate spurious correlations, so it does not seem productive to speculate at length about those findings.

**Table 4** Empirical Studies of the Initiative and Expenditure and Revenue

Study	Jurisdiction	Period	N	“Effect” of initiative
<i>Panel A. American states and Swiss cantons</i>				
Matsusaka (1995)	U.S. states	1960–1990	343	Less expenditure and revenue*
Merrifield (2000)	U. S. states	1981, 1986, 1991	147	Less expenditure*
Besley and Case (2003)	U. S. states	1960–1998	1817	Less tax revenue(*), mixed on expenditure
Matsusaka (2004)	U. S. states	1970–2000	1488	Less expenditure and revenue*
Primo (2006)	U. S. states	1969–2000	1504	Less expenditure*
Feld and Matsusaka (2003)	Swiss cantons	1980–1998	494	Less expenditure*
Freitag and Vatter (2006)	Swiss cantons	1990–2000	275	Less revenue*
Glazer and McGann (2008)	U. S. states	2002	50	Less expenditure and revenue*
Funk and Gathmann (2011)	Swiss cantons	1890–2000	2395	Less expenditure and revenue*
Burret and Feld (forthcoming)	Swiss cantons	1980–2011	832	More expenditure and revenue
<i>Panel B. Cities</i>				
Zax (1989)	U. S. cities	1982	1305	More expenditure*
Farnham (1990)	U. S. cities	1982	735	None
Matsusaka (2004)	U. S. cities	1982–1997	13,583	More expenditure and revenue*
Primo (2010)	U. S. cities	2000	611	More expenditure*
Blume et al. (2011)	German cities	1970–2005	84	More expenditure*
Asatryan (2016)	German cities	2002–2009	53,715	More expenditure and revenue*
Asatryan et al. (2017a)	German cities	1983–2011	4666	More expenditure and revenue*
<i>Panel C. Countries</i>				
Blume et al. (2009)	Countries	1990s	62	More expenditure*
Blume and Voigt (2012)	Countries	2008	94	Less expenditure
<i>Panel D. Early twentieth century</i>				
Matsusaka (2000, 2004)	U. S. states	1902–1942	192	More expenditure and revenue*
Funk and Gathmann (2011)	Swiss cantons	1890–1959	1555	Less expenditure*

This table summarizes published studies estimating the relation between fiscal policy and availability of the initiative process. N is the number of observations in the main equation. Initiative “effect” is the difference in fiscal policy between initiative and non-initiative jurisdictions, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1). The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better. An asterisk in parentheses indicates that the difference is statistically significant for some but not all specifications

they would not differ habitually. An important challenge for the literature is to provide a convincing explanation for systematic differences in policy outcomes.

One possibility is that the reported differences are spurious. The mostly likely cause of a spurious correlation is unobserved variation in citizen preferences; the demographic and economic proxies for citizen preferences employed in most studies might be too coarse to capture the underlying variation. However, as discussed above, Funk and Gathmann (2011) provides careful and convincing estimates of citizen preferences in Switzerland that seem to rule out spurious preference-based correlation. Similarly, Matsusaka (2004) considers an array of preference information for American states, showing that preferences are similar across initiative and non-initiative states and that between-state differences persist even controlling for preferences. One can never completely rule out spurious correlation, but existing evidence casts significant doubt on the most likely possibilities.

The explanation I would tentatively advance is that during the 1970–2000 period voters became more fiscally conservative than their governments, and voters in initiative states took matters into their own hands and pushed policy in a conservative direction. Temporary divergences between voters and their representatives can emerge if public opinion shifts, but government officials change their views more slowly (Matsusaka 2004, Ch. 6). Validating that explanation is beyond the scope of this survey, but some pieces of evidence lend it support. Peltzman (1992) shows that voters behaved like fiscal conservatives during this period, and Matsusaka (2004) documents a preference for fiscally conservative policies in opinion surveys. Tolbert (1998) finds that initiative states were more likely to adopt tax and expenditure limits than non-initiative states, again suggesting that citizens perceived legislators to be too fiscally liberal. Voter preferences may have moved to the right during the period as people tired of the “big government” era that began in the 1930s and peaked in the 1960s, or as pro-spending special interests gained more influence in legislatures; some theories suggest that governments systematically prefer more spending than the median voter.<sup>19</sup> Government policies are likely to respond sluggishly to changes in public opinion because incumbents do not adjust their policy preferences to mirror constituent preferences; as Poole (2007) put it, legislators “die in their ideological boots.” Because legislators for the most part have fixed opinions, policy changes only when incumbents are replaced by new representatives whose preferences are more closely aligned with voters (McCarty et al. 2015; Fedaseyeu et al. 2016), and this transition can take some time owing to the well-known advantages of incumbency.

If the lower spending in initiative than non-initiative states and cantons is in fact caused by temporary divergences between voters and government officials ( $V < G$ ), then we might expect to see the gap narrow and possibly reverse as the underlying preferences evolve. Matsusaka (2000, 2004) explores this idea by estimating spending differences early in the twentieth century. (Panel D of Table 4 lists studies that focus on differences in the early twentieth century.) In the early twentieth century, there is reason to believe that voters were more fiscally liberal than their governments. Massive migration from farms to cities around the turn of the nineteenth century rapidly transformed the population from rural to urban, but states did not redistrict their legislatures to adjust for population changes—this was before the one-person one-vote principle was adopted—so that the rural

<sup>19</sup> For example, the large literature on fiscal externalities argues that legislators prefer excessive spending because pork-barrel projects provide concentrated benefits to their constituents, while the costs are spread over the taxpayers at large (Buchanan and Tullock 1962; Weingast et al. 1981; Gilligan and Matsusaka 1995, 2001; Bradbury and Crain 2001; Baqir 2002). The bureaucratic budget-maximizing model of Niskanen (1971) also implies a propensity for government to spend more than voters prefer.

areas came to be “overrepresented” in state legislatures. The dominant rural interests were not sympathetic to the new spending programs favored by city dwellers, such as old-age insurance, welfare programs for the poor, worker’s compensation, and urban infrastructure, such as clean water and sewage systems. Because  $G < V$  appears to have been a general pattern during the period, theory predicts that citizens would use the initiative process to adopt the new programs. Consistent with this hypothesis, Matsusaka (2000, 2004) shows that initiative states spent *more* than non-initiative states during the early twentieth century. Moreover, many examples can be found of initiatives targeted specifically at the new programs desired by urban voters. Voters also used the initiative to force states to redraw district lines to address the overweighting of rural voters. Funk and Gathmann (2011) presents some evidence for Swiss cantons prior to 1959 that does not show the same pattern; I am not sufficiently familiar with Swiss history to know if the country experienced the same urban–rural divide as the United States did during this period.

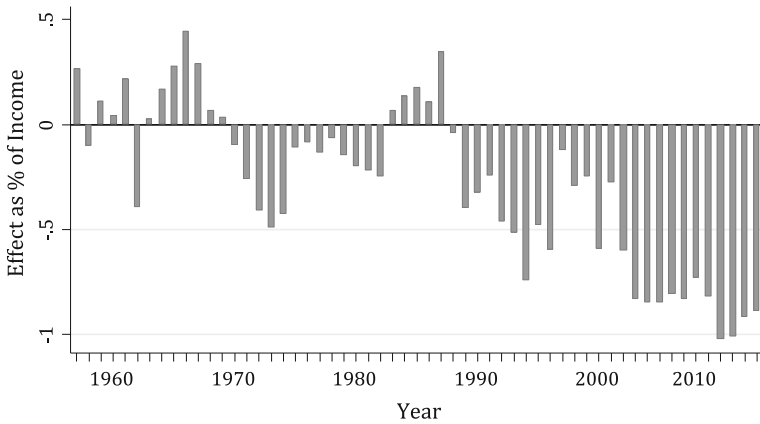
To the extent that government and voter ideal points drift over time, we should see the difference between initiative and non-initiative states drift over time as well. Figure 6 reports some evidence on this conjecture. The figure also serves to update the literature on initiative effects in American states for the post-2000 period, about which no evidence has yet been reported. The figure displays the conditional difference in spending by initiative and non-initiative states over the 1957–2014 period, constructed by regressing combined state and local direct general expenditure as a percentage of state income on federal aid as a percentage of income, the logarithm of population, a dummy variables for Southern states, years, and year-specific initiative availability.<sup>20</sup> The figure reports the coefficients on the year-specific initiative dummies, essentially showing the conditional difference in spending between initiative and non-initiative states over time.<sup>21</sup>

Initiative states spent more than non-initiative states in most of the early years of the sample. The relation reversed in the 1970s (during the tax revolt) and continued into the early 1980s, when it reversed again. Finally, in the late 1980s, spending in initiative states fell below that of non-initiative states, and the gap has widened in the twenty first century. The new evidence in the figure is the historically wide gap that emerged beginning in 2005, and currently stands at about 1% of income. Considering that state and local spending is about 17% of income, on average, the gap represents almost a 6% difference in spending between initiative and non-initiative states.

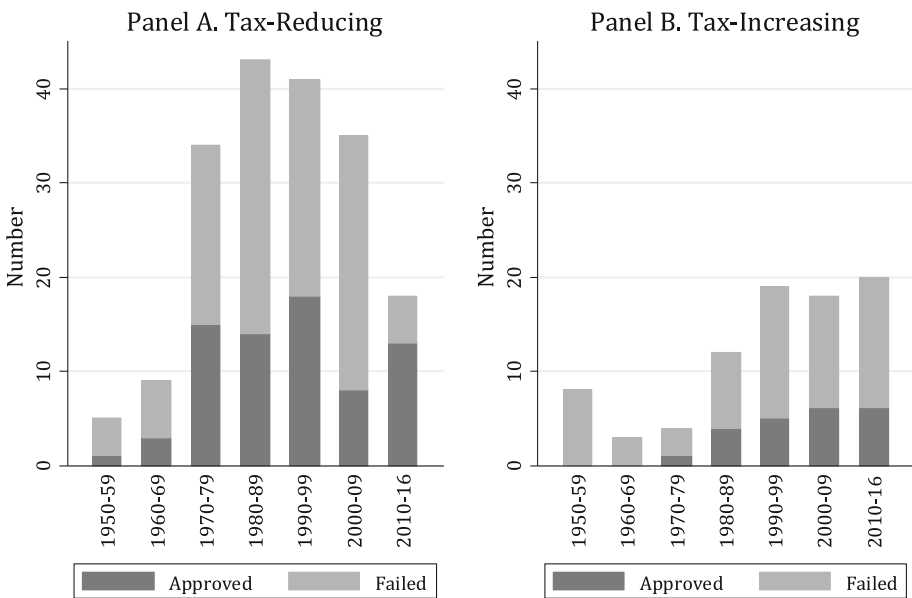
Is it reasonable to think that government and voter preferences diverged in a way that can account for Fig. 6? A healthy amount of anecdotal information points in the same direction as the statistical evidence. In the United States, historical accounts trace the rise of a “tax revolt” in the 1970s to California’s Proposition 13. Figure 7 reports the number of tax cutting and tax limiting initiatives in the United States by decade across the 24 states that allow initiatives. One can see that tax-cutting and tax-limiting initiatives surged in the 1970s, both in terms of overall volume and the number approved, and remained high for several decades. If  $G = V$ , there would be little reason for voters to push for lower taxes, and we would not expect to see voters override their representatives through initiative in order to cut taxes. The number of tax-increasing initiatives was small, and focused primarily on tobacco taxes. Evidence on the number of initiatives should be viewed with caution because they do not account for the importance of the measures; California’s

<sup>20</sup> Fiscal data from *State and Local Government Finances*, published by the Census.

<sup>21</sup> Following the literature, I omit Alaska and Wyoming, which are outliers because of significant severance tax revenue.



**Fig. 6** Initiative ‘Effect’ by Year: 1957–2015. *Note* The figure plots the difference in expenditure between initiative and noninitiative states in the United States, controlling for federal aid, population, region, and year, based on a regression. A positive number indicates that initiative states spent more than noninitiative states. Expenditure is combined state and local direct general expenditure. Alaska and Wyoming are excluded. The years 2001 and 2003 are omitted because spending data are unavailable



**Fig. 7** Tax initiatives in American states. *Data Source:* Initiative and Referendum Institute

Proposition 13 had an immense impact that far outweighs a large number of relatively minor tax-cutting propositions in the 1990s.

To fully account for the existing evidence, we would also need an explanation for why spending is higher in initiative than non-initiative cities. I am unaware of a study that has offered a convincing reason why  $V > G$  would be the norm in cities. One might speculate that commercial interests such as developers and local businesses are particularly

influential in city halls, and they favor lower spending and taxes than the majority of city residents. Positing a pro-business bias in city halls might help to explain the recent raft of successful minimum wage measures in cities across the United States, but does not fully explain why business interests would favor less spending than residents, rather than just different types of spending. Perhaps the pattern is connected to the demographic transition that occurred from 1960 to 2000 as wealthier citizens left the cities for the suburbs; this out-migration might have shifted city preferences to the left faster than government preferences shifted.

Another issue of interest in the literature is the effect of the initiative on fiscal centralization, meaning the fraction of expenditure and taxes that is attributable to the state as opposed to local governments. Studies that estimate centralization directly are listed in Panel A of Table 5. We saw earlier that state-level governments spend and tax less, at least in the last half-century. The question is whether this results in a lower scale of government overall, or if it represents fiscal devolution, that is, by cutting state spending, are voters pushing government decisions more to the local level or are they simply shrinking the government? The initial study of this question (Matsusaka 1995) found significant decentralization associated with the initiative process in American states, but that pattern has not appeared in Swiss data. The evidence for Switzerland shows either a statistically insignificant and quantitatively small connection between centralization and initiative signature requirements, or more centralization when signature requirements are low.<sup>22</sup>

Panel B of Table 5 provides a different perspective on centralization by considering the connection between state-level initiatives and local government spending. The general finding, based on data exclusively from the United States, is that cities spend more if they are located in a state with the initiative process, all else equal. Inference is complicated by the fact that cities are more likely to have the initiative in states that also have the initiative (e.g., all cities in California have the initiative), raising the possibility that city- or state-level initiative dummies might proxy for each other in standard regressions. Primo (2010) contains a systematic analysis of this issue, allowing city spending to depend on both city and state initiative availability. He finds that city-level initiatives are associated with more spending, but does not find a statistically significant independent relation between state-level initiatives and local spending.

Table 6 lists a variety of other studies that examine the connection between the initiative and specific expenditure and tax categories. As one subdivides expenditure and revenue into finer categories the risk of accidental data mining rises. The literature is not extensive for any individual category, so the findings should be received with caution. Only Asatryan (2016) and Asatryan et al. (2017b) use contemporary methods to identify causality. Three studies find a positive association between welfare spending and the initiative, but in only one of them (Feld et al. 2010) is the difference statistically significant. Two studies find that initiatives cities spend less on public employees than non-initiative cities, and the differences are significant. Finally, Feld et al. (2010) finds less spending on non-welfare functions in initiative than non-initiative cantons, and Lewis et al. (2015) finds no significant difference in spending on “collective” versus “particularized” functions.

Panel B of Table 6 lists studies that examine the composition of public revenue. Three studies find that initiative states and cities in the United States raise less money from taxes and more from fees and charges for services than non-initiative states do. The differences

<sup>22</sup> Other evidence suggests a negative connection between the mandatory referendum and expenditure centralization, statistically significant in Feld et al. (2008), but not significant in Funk and Gathmann (2011).

**Table 5** Empirical studies of the initiative and fiscal centralization

Study	Jurisdiction	Period	N	“Effect” of initiative
<i>Panel A. Fiscal centralization</i>				
Matsusaka (1995)	U. S. states	1960–1990	343	Less expenditure centralization*
Matsusaka (2004)	U. S. states	1970–2000	1488	Less expenditure centralization*
Feld et al. (2008)	Swiss cantons	1980–1998	494	Less expenditure, more revenue centralization
Funk and Gathmann (2011)	Swiss cantons	1890–2000	2310	More expenditure centralization*
<i>Panel B. Local spending and state-level initiative</i>				
Matsusaka (1995)	Local governments in a state	1960–1990	343	More expenditure*
Matsusaka (2004)	Local governments in a state	1970–2000	1488	More expenditure
Primo (2010)	U. S. cities	2000	611	More expenditure

This table summarizes published studies estimating the relation between fiscal centralization and availability of the initiative process. Centralization is the ratio of state spending (revenue) to combined state and local spending (revenue), or the analogous measure for cantons and cities. N is the number of observations in the main equation. Initiative “effect” is the difference in fiscal policy between initiative and non-initiative jurisdictions, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1). The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better

are usually, but not always statistically significant. I conjectured in Matsusaka (1995) that this pattern might reflect a preference among voters for requiring those who use public services to pay for them, that is, to reduce cross-subsidization on the revenue side, and thus reduce redistribution. Asatryan et al. (2017b) find that German cities rely more on business taxes and less on property taxes when the initiative is easy as opposed to difficult to access.

Finally, Panel C of Table 6 lists studies that examine the connection between the initiative and debt and deficits. Most studies find small and statistically insignificant differences. The exception is Asatryan (2016), which finds smaller deficits in German cities where the initiative is more accessible.

### 4.3 Social policy

Table 7 lists the literature on social policies and direct democracy. All published work studies American states and the initiative process. Policies are treated as dichotomous—a state either has a certain law or does not. Consequently, all estimates are probit/logit versions of Eq. (1) or the analogous hazard/duration models.<sup>23</sup>

One pattern is that initiative states adopt more conservative social policies than non-initiative states. Specifically, initiative states are more likely to require parental notification for an abortion by a minor, adopt more restrictive abortion rules, use capital punishment, declare English the official language, and ban same-sex marriage. The differences are statistically different from zero in all studies. The one policy area that may or may not fit the pattern is casino gaming on tribal lands, which initiative states are more likely to allow

<sup>23</sup> While hazard models have their virtues, the underlying assumption that policy making is a one-way trip—all states eventually adopt a policy and never reverse themselves—is contrary to fact. For example, there have been numerous reversals in death penalty and same-sex marriage policies over time.

**Table 6** Empirical studies of the initiative and the composition of spending and taxes

Study	Jurisdiction	Period	N	“Effect” of initiative
<i>Panel A. Spending categories</i>				
Blume et al. (2009)	Countries	1990s	55	More welfare spending
Feld et al. (2010)	Swiss cantons	1980–1998	494	More welfare spending*
Blume and Voigt (2012)	Countries	2008	94	More welfare
Matsusaka (2009)	U. S. cities	2000	652	Less public employment*, lower public employee wages*
Asatryan (2016)	German cities	2002–2009	53,715	Less spending on public employees*
Feld et al. (2010)	Swiss cantons	1980–1998	494	Less nonwelfare spending*
Lewis et al. (2015)	U. S. states	1982–2011	1418	Less “collective” versus “particularized” spending
<i>Panel B. Revenue sources</i>				
Matsusaka (1995)	U. S. states	1960–1990	343	Less taxes*, more fees*
Matsusaka (2004)	U. S. states	1970–2000	1488	Less taxes*, more fees
Matsusaka (2004)	U. S. cities	1982–1997	13,583	Less taxes, more fees*
Asatryan et al. (2017b)	German cities	1980–2011	299	Higher business tax rates*, no change in property tax rates
<i>Panel C. Deficits</i>				
Matsusaka (1995)	U. S. states	1960–1990	343	Higher deficits
Blume et al. (2009)	Countries	1990s	45	No difference in deficits
Blume and Voigt (2012)	Countries	2008	94	Lower deficits*
Asatryan (2016)	German cities	2002–2009	53,715	Lower deficits

This table summarizes published studies estimating the relation between various fiscal policies and availability of the initiative process. N is the number of observations in the main equation. Initiative “effect” is the difference in fiscal policy between initiative and non-initiative jurisdictions, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1). The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better. “Collective” spending in Lewis et al. (2015) is spending on education, highways, law enforcement, parks, natural resources, and government administration; “particularized” spending concerns welfare, hospitals, health care, and corrections

than non-initiative state. Whether this pattern is an exception to the general conservative pattern for social issues, or whether voters view gambling as an economic rather than a social issue is not clear. These studies employ fairly convincing proxies for citizen preferences—in most cases the studies have issue-specific public opinion data—so the conservative policies in initiative states cannot be explained as a result of more conservative public opinion in those states.

To replicate some existing findings as well as to expand the set of social issues that have been studied, Table 8 reports new regressions on eight policy issues. For each issue, I collected the policy position in each state in 2005, as well as state-specific opinion data on that specific issue from the American National Election Studies (see Matsusaka 2010 for



**Table 7** Empirical studies of the initiative and social policies in American states

Study	Policy	Period	N	“Effect” of initiative
Gerber (1996, 1999)	Abortion, parental consent	1990	50	More likely to require*
Arceneaux (2002)	Abortion index	2000	40	More restrictive policies*
Gerber (1999)	Death penalty	1990	50	More likely to permit*
Boehmke (2005)	Death penalty	1972–1982	224	More likely to adopt*
Schildkraut (2001)	English, official language	1981–1998	630	More likely to adopt*
Hume (2011)	Marriage, same-sex	1998–2009	442	More likely to ban*
Lewis (2011)	Marriage, same-sex	1994–2006	~ 650	More likely to ban*
Boehmke and Witmer (2004)	Tribal gaming	1988–2000	578	More likely to allow*
Boehmke (2005)	Tribal gaming	1989–1999	364	More likely to allow*

This table summarizes published articles estimating the relation between social policy and availability of the initiative process. All articles study data from American states. N is the number of observations in the main equation. Initiative “effect” is the difference in the policy between initiative and non-initiative states, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1). The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better

details). The table reports coefficients from eight linear probability regressions, one for each issue, and one that pools all of the issues, with issue-specific dummy variables. The dependent variable is equal to one if the state had the “conservative” outcome. For example, the dummy was equal to one in the first regression if the state required parental consent or notification before a minor could have an abortion. The results are qualitatively similar using logistic regressions; I report linear probability regressions because the coefficients are easier to interpret.

Table 8 shows a positive relation between conservative social policies and availability of the initiative process for all eight issues, and the difference is statistically different from zero for six of them (the exceptions are a ban on partial birth abortions and English as the official language).<sup>24</sup> In the pooled regression at the bottom of the table, a precisely estimated positive relation emerges: across the eight issues, initiative states are 12% more likely than non-initiative states to take the conservative position on the issue ( $p = 0.003$ ). These differences are difficult to attribute to varying policy preferences between initiative and non-initiative states because the regression directly controls for the percentage of the population in each state that favors the conservative outcome according to opinion surveys (recall that opinion here is not a liberal-conservative thermometer or something similarly broad, but views on the precise policy that comprises the dependent variable). Opinion influences policy—the coefficients on opinion are always positive and usually statistically significant—but the difference between initiative and non-initiative states is on top of these opinion-driven differences.

The usual caveat applies about attributing a causal relation to the initiative coefficients in Table 8. It seems possible that some of the differences stem from other factors, such as

<sup>24</sup> The lack of statistical significance for English-only stands in contrast to the findings of Schildkraut (2001). The sample periods differ, but given the much larger number of observations in Schildkraut ( $N = 630$ ), my estimates are insignificant probably because of the small sample size ( $N = 50$ ).

**Table 8** Linear probability regressions of conservative social policy

Law	Dummy = 1 if initiative state	Opinion, % conservative	Dummy = 1 if South	Constant	R <sup>2</sup>
Abortion, require parental consent	0.16 (0.13)	2.54** (0.95)	0.36** (0.16)	- 1.57** (0.71)	0.265
Abortion, prohibit partial birth	0.11 (0.13)	1.88 (1.22)	0.09 (0.16)	- 1.08 (0.82)	0.286
Abortion, prohibit public funding	0.14 (0.12)	1.98*** (0.66)	0.35** (0.15)	- 0.48 (0.33)	0.304
English, official language	0.29** (0.14)	0.72 (1.26)	0.49*** (0.16)	- 0.26 (0.92)	0.195
Death penalty, permit	- 0.05 (0.12)	3.85*** (1.01)	0.40*** (0.13)	- 2.35*** (0.77)	0.353
Job discrimination, permit versus gays	0.20* (0.10)	3.25*** (0.68)	0.27* (0.14)	- 0.64*** (0.23)	0.487
Same-sex marriage, ban	0.12 (0.08)	0.76 (0.46)	0.10 (0.11)	0.31 (0.31)	0.128
School prayer, permit	- 0.16 (0.13)	3.02** (1.32)	0.23 (0.17)	- 1.95* (1.13)	0.243
All issues pooled	0.12*** (0.04)	1.90*** (0.30)	0.30*** (0.05)	- 0.79*** (0.20)	0.209

Each row of the table reports coefficients from a linear probability regression using data from 50 states in 2005. The dependent variable is equal to one if a state adopted the law indicated in the first column. Standard errors are in parentheses beneath the coefficient estimates. The last row pools all eight issues, and includes issue-specific fixed effects

Significance levels are indicated: \* = 10 percent, \*\* = 5 percent, \*\*\* = 1 percent

state political culture. For example, Table 8 shows that Southern states are 30% more likely to choose conservative policies than non-Southern states across all issues, again, after controlling for public opinion on each issue. At the same time, Tables 7 and 8 do establish a pronounced propensity for initiative states to adopt more conservative policies than non-initiative states, apparently across a wide array of social issues. Given that the regressions control for public opinion on the issues, we cannot easily rule out the possibility that the initiative is in fact contributing to this difference.

#### 4.4 Electoral processes and government structure

Many initiatives that come before the voters are targeted at electoral processes and the structure of government. Table 9 lists published studies that examine the connection between the initiative and election rules and government structure. Except as noted, these studies use data from American states, focus on the initiative process, and rely on a single cross-section for identification. Most of these studies lack direct controls for preferences and are based on a single cross-section, so spurious correlation is a concern.

The most robust finding, and one that is quite likely to be causal, is that initiative states are more likely than non-initiative states to impose term limits on elected officials. Tolbert (1998) and Matsusaka (2006) find this pattern for legislative term limits, and Matsusaka (2008) finds the same pattern for gubernatorial term limits. The modern term limit movement began in 1990 with initiatives in California, Colorado, and Oklahoma; since then 21 of 24 initiative states adopted legislative term limits, and in every case, were adopted by an actual initiative. In contrast, two of the 26 non-initiative states adopted legislative term limits during the same period. Opinion surveys show majority support for

**Table 9** Empirical studies of the initiative and electoral processes and government structure

Study	Policy	Period	N	“Effect” of initiative
Tolbert (1998)	Term limits, legislature	1994	50	More likely to have*
Matsusaka (2006)	Term limits, index	2005	50	More likely to have*
Matsusaka (2008)	Term limits, executive	2003	50	More likely to have*
Pippen et al. (1992)	Campaign contribution limits	1984, 1998	100	More limits*
Matsusaka (2006)	Campaign finance index	2005	50	No material difference
Matsusaka (2008)	Voting and ballot index	2005	50	More open
Matsusaka (2006)	Redistricting, by commission	2005	50	More likely to have
Funk and Gathmann (2013a)	Proportional representation	1890–2000	2535	More likely to adopt*
Matsusaka (2008)	Executive veto	2003	50	More likely to have veto*
Matsusaka (2008)	Meetings of cabinet, open	2003	50	More likely to require
Tolbert (1998)	Legislature, supermajority	1994	50	More likely to require*
Tolbert (1998)	Tax and expenditure limits	1994	50	More likely to have*
Di Tella and Fisman (2004)	Wages, governors	1950–1990	929	Less sensitive to income, more sensitive to taxes*
Matsusaka (2006)	Wages, executive officers	2003	50	Lower*
Matsusaka (2009)	Wages, public employees	2000	652	Lower*
Gerber and Phillips (2005)	Urban growth boundaries	2002–2003	290	More restrictive*

This table summarizes published articles estimating the relation between election rules and government structures and availability of the initiative process. All articles study data from American states, except for Gerber and Phillips (2005) and Matsusaka (2009), which study American cities. N is the number of observations in the main equation. Initiative “effect” is the policy difference between initiative and non-initiative states, controlling for other variables, that is, the coefficient  $\beta$  in Eq. (1). The table reports the “best” estimate from the paper (in the judgment of the study authors, where available, otherwise in my judgment). An asterisk indicates that the difference is statistically different from zero at the 10 percent level or better

term limits in every state, so the pattern cannot be explained by different preferences in initiative versus non-initiative states. Theoretically, term limits are precisely the sort of issue for which the initiative process is expected to matter because voter and legislator ideal points are likely to differ sharply ( $G \neq V$ ).

In terms of election laws, the evidence generally is inconclusive. Two studies examine campaign finance laws; Pippen et al. (1992) reports that initiative states had more restrictive campaign finance rules than non-initiative states in 1998, and the difference is statistically significant, while Matsusaka (2008) finds no material or statistically significant difference for a campaign finance index in 2005. Matsusaka (2008) reports that initiative

states had more open voting and ballot access rules than non-initiative states in 2005, but again the difference is not statistically significant. Matsusaka (2006) finds that initiative states are more likely to use commissions for legislative redistricting, but the difference is not statistically significant.

The most conclusive finding regarding election laws comes from Funk and Gathmann (2013a), which shows that Swiss cantons were more likely to adopt proportional representation when initiative signature requirements were low rather than high. The finding is statistically significant, and many of the changes can be traced directly to actual initiatives. That sort of reform is another example where the existing government would have a different interest than the voters ( $G \neq V$ ) because changing the electoral system threatens to undermine the currently dominant political coalition.

In terms of government structure, Matsusaka (2008) finds that initiative states are more likely than non-initiative states to grant the governor a veto, and the difference is statistically significant. The same study reports that initiative states are more likely to require open cabinet meetings, but that difference is not statistically different from zero. Initiative states are more likely to tie the legislature's hands regarding tax policy: Tolbert (1998) finds that initiative states are more likely to require a legislative supermajority to raise taxes, and are more likely to impose tax and expenditure limits.

In terms of public employee compensation, two studies find that initiative states pay their public workers (executive branch and public employees in general) less on average, and the differences are statistically significant. Di Tella and Fisman (2004) find that governors' salaries in initiative compared to non-initiative states depend less on state income growth and more on tax revenue growth.

The final policy considered in Table 9 is land-use regulations. Gerber and Phillips (2005) compare urban growth boundaries in 290 California cities in 2002–2003. They find that boundaries adopted by initiatives are more restrictive than the boundaries adopted by city councils.

## 5 Congruence

A central research issue is whether direct democracy makes policy more or less responsive to citizen preferences. The Progressives who brought the initiative and referendum to the United States in the early twentieth century clearly expected to make policy more responsive to the people:

I believe that the initiative and referendum should be used, not as substitutes for representative government, but as methods of making such government really representative. Action by the initiative and referendum ought not to be the normal way of legislation; but the power to take it should be provided in the constitution, so that if the representatives fail truly to represent the people on some matter of sufficient importance to rouse popular interest, then the people shall have in their hand the facilities to make good the failure. – Theodore Roosevelt<sup>25</sup>

The Progressive view has been contested by what might be called the “special interest subversion” view, arguing that the initiative and referendum in fact will empower special interests, because they have the resources to dominate elections and the proposal process.

<sup>25</sup> Theodore Roosevelt, “A Charter for Democracy,” speech to the Ohio State Constitutional Convention, February 21, 1912.

Despite the importance of that debate, evidence that speaks to it in a convincing way is surprisingly limited. Part of the reason is the difficulty of measuring congruence between policy and preferences.

We can frame the question in terms of the model developed above by defining “congruence” as  $CONG = -|x - V|$ , where  $x$  is the policy outcome and  $V$  is the voter’s preference. In the abstract, we can suppose that a relation exists between congruence and direct democracy of the form:

$$CONG_i = a + bD_i + cX_i + u_i, \quad (2)$$

where  $i$  indexes an observation (e.g., a state in a given year),  $D$  is an indicator variable for availability of direct democracy, and  $X_i$  is other variables that affect congruence. The Progressive argument is that  $b > 0$ : direct democracy allows the majority to achieve its policy preferences more often than not. The special interest subversion argument is that  $b < 0$ : direct democracy allows special interests to subvert the policy preferences of the majority.

The primary challenge in estimating Eq. (2) is the lack of data on citizen preferences needed to construct  $CONG$ . Information is abundant on public opinion in general terms, such as an ideology index, but public opinion data on specific policies at the level of observation  $i$  are rare. Several studies have attempted to evaluate congruence using general opinion data and a regression of the form:

$$x_i = \alpha + \beta \cdot O_i \cdot D_i + \delta \cdot O_i + \lambda \cdot D_i + \mu \cdot X_i + u_i, \quad (3)$$

where  $O_i$  is an opinion index (e.g., a liberal-conservative thermometer or demographic and economic variables believed to be correlated with preferences). These studies focus on the parameter  $\beta$ , arguing that a larger value of  $\beta$  implies that policy is more congruent with opinion in direct democracy than non-direct democracy jurisdictions. While seemingly plausible at first glance, several studies have shown that in fact one cannot draw conclusions about congruence from such regressions (Romer and Rosenthal 1979b; Erikson et al. 1993, Ch. 4; Matsusaka 2001)—because no theoretical connection exists between  $\beta$  and congruence. (See the “Appendix” for a formal explanation.)

Given these challenges, the literature has gone down two paths to assess the connection between congruence and direct democracy. Matsusaka (2004) takes an indirect approach. After documenting that American initiative states tax and spend less than non-initiative states on average, the study examines survey data in which citizens express their views on fiscal policy. The opinion data show (for the period in question) that a majority of citizens preferred less public spending and lower taxes to more spending and higher taxes, suggesting that  $V < G$ . If so, one can conclude that less spending and lower taxes in initiative states is closer to majority opinion, consistent with the Progressive view.

Two studies calculate congruence directly. These studies, again focused on American states, collect state-specific opinion data on specific policies and, for each state, identify whether the policy choice is congruent with majority opinion in the state, in effect calculating  $CONG = -|x - V|$  directly, where  $V$  is the preferred policy choice of a majority of citizens in a state. These studies examine policies with dichotomous outcomes, allowing each state law to be classified as congruent or not congruent. Formally, suppose that the policy in question is  $x \in \{yes, no\}$  (e.g., the state does or does not employ capital punishment) and that the majority’s opinion is  $V \in \{yes, no\}$ . Then congruence is calculated as

$$CONG_i = \begin{cases} \text{yes} & \text{if } x = V; \\ \text{no} & \text{if } x \neq V. \end{cases} \quad (4)$$

For example, if a majority of citizens in a state support use of the death penalty, then the policy is congruent if the state permits the death penalty, and not congruent if the state does not permit it.

The first study to go down this path was Matsusaka (2010), which calculates congruence across 10 issues in the American states in 2005. Public opinion on each of the 10 issues is collected from the American National Election Studies survey, which provides state-level opinion information. Interestingly, the study finds that overall congruence is only 59%, meaning that across all 50 states and 10 policies, the prevailing law reflects majority opinion only 59% of the time. This number is surprisingly low given that congruence would be 50% if policies were selected by flipping a coin without any regard to voter preferences. More to the point of this survey, congruence was 18–19% higher in initiative than non-initiative states. The pattern is robust to controlling for other factors, as in (2), and statistically significant.

Lax and Phillips (2012) takes the same path, but considers an even larger set of policies. Instead of using direct opinion survey evidence, the study imputes state-level opinion from national surveys using multilevel regression and post stratification methods, allowing a significant increase in the scope of the analysis (39 policy issues). This study also finds a low level of congruence overall: 49%. It does not provide a direct comparison between congruence in initiative and non-initiative states, but does present a regression of congruence (2) with a variety of institutional variables, one of them being availability of the initiative process. The coefficient on the initiative variable is small and statistically insignificant. A confounding aspect of this finding is that the regression includes an indicator for term limit states, finding that term limit states are 15% more likely to choose congruent policies than non-term limit states. The problem is that, as discussed above and recognized in Lax and Phillips (2012), the term limit states are almost the same as the initiative states, and the initiative is a primary determinant of whether a state has term limits or not, so the term limits variable is a proxy for the initiative variable.

In order to sort this out, I re-examined the original data used in the Lax and Phillips (2012) study, kindly provided to me by Jeffrey Lax. In an unconditional comparison of the means, initiative states were 3% more congruent than non-initiative states (the difference is not statistically significant).<sup>26</sup> In a regression like (2), including issue fixed effects and various controls, but not a term limits dummy, the difference in congruence between initiative and non-initiative states ranges from – 1 to 1% (never statistically significant). If the sample is restricted to issues in common with the Matsusaka (2010) study, initiative states are 11–15% more congruent, depending on how initiative status is defined. It appears that initiative states are more congruent over the issues covered in Matsusaka (2010), but no more or less congruent over the additional issues studied in Lax and Phillips (2012). No immediate distinction between the two sets of issues is obvious, but a systematic analysis remains to be done.

To summarize, the existing evidence on policy congruence is limited, but what exists suggests that initiative states are more likely (or no less likely) than non-initiative states to

<sup>26</sup> This pattern holds whether initiative states are defined if they allow (1) constitutional amendment initiatives, or (2) constitutional amendment or statutory initiatives. Following the literature, I classify Illinois as a non-initiative state (its initiative process is limited so that it cannot be used to address any of the policy issues in the dataset); Illinois' classification does not change the results.

adopt policies favored by a majority of citizens. The evidence is thus consistent with the Progressive view that the initiative allows the majority to counteract the influence of special interests on policy. None of the evidence supports the view that the initiative enhances the power of special interests, making non-majoritarian outcomes more likely.

## 6 Discussion

The literature on policy effects of the initiative and referendum has expanded rapidly over the last 20 years. Some conclusions now seem safely established, while a number of issues remain unresolved, and some new questions have emerged.

In terms of relatively “safe” conclusions, perhaps the strongest is that the mandatory referendum has a material inhibiting effect on policies: if voter approval is required for spending or debt increases, it is likely that spending and debt will be lower on average. This finding suggests that purely representative governments tend to spend and borrow more than voters prefer, on average.

Another conclusion is that the initiative pushes policy towards the outcome preferred by a majority of citizens, but the direction of that effect varies depending on the relative policy positions of the government officials and voters. In American states and Swiss cantons over the last 50 years or so the initiative has reduced the size of government on average. In contrast, the initiative seems to have driven up spending in cities, suggesting that representatives at the state and canton level tend to spend more than voters would like, but that representatives at the local level tend to spend less than voters would like. An important open question is what causes the preferences of government officials and ordinary citizens to diverge, and why do they appear to diverge in the same way at the same time across multiple jurisdictions?

The evidence consistently finds that initiative states adopt more conservative social policies than non-initiative states in the United States, and this finding holds across a wide array of policies. This pattern suggests that elected representatives tend to prefer more socially liberal policies than the majority of voters prefers. Why this is the case is another interesting unanswered question.

One of the more important tasks going forward is to produce more estimates of the effect of the initiative and referendum that exploit recent innovations in causal analysis. Much of the literature was produced prior to the so-called revolution in causal inference, and relies on research methods that are more correlational than causal. Improved causal estimates have a high value because the underlying institutions—availability of the initiative and referendum—tend to move slowly over time and their adoption usually is endogenous. It is natural to wonder if the observed correlations are spurious, and whether they might be driven jointly by underlying omitted factors. So far, the literature has attempted to address this issue in a variety of ways. One is to be explicit about the potential omitted variables (especially public opinion), and attempt to measure them directly. Another approach is to introduce anecdotal and corroborative evidence that points to causal relations. A third approach is to structure analysis using rigorous theory that identifies clear channels through which effects might run. All of these methods have scientific value and help build the case, and should continue to be employed. More research would be welcome that employs the newer methods of causal inference, such as difference-in-differences (Nguyen-Hoang 2012; Burret and Feld forthcoming 2018), regression discontinuity (Asatryan et al. 2017a), instrumental variables (Asatryan 2016; Funk and Gathmann 2011),

and so forth. The findings thus far using contemporary research methods, both published and in working papers, tend to reinforce the findings from the earlier literature, but more needs to be done by way of causal inference.

Another topic that should receive more attention is advisory referendums. In the last few years, advisory referendums have been at the center of global politics, with the United Kingdom's Brexit vote in 2016, Colombia's vote on a peace agreement with the FARC guerrillas in 2016, and Greece's vote on its debt bailout terms in 2015, as the most prominent examples. Advisory referendums are called at the discretion of the government, with the apparent goal of legitimizing a decision that the government favors and, in most cases, the government expects that voters will support the government's position. Yet voters sometimes upset expectations by rejecting a proposal, and governments generally feel obligated to abide by the outcome even though the vote is advisory only. Older historical examples include Chilean voters' unexpected rejection of a referendum in 1988 that led to the end of the military dictatorship of Augusto Pinochet, and French voters' rejection of a referendum in 1969 that led to the resignation of President Charles De Gaulle. The literature would benefit from more research to understand why governments call these referendums, why they exert so much influence even though they are purely advisory, and how they influence the quality of democratic governance.

Finally, the literature has been largely silent about normative issues. Most research simply focuses on documenting the effects of the initiative and referendum. More analysis on the normative implications of these findings would be worthwhile. A natural argument is that the initiative and referendum are valuable elements of the democracy toolkit because they allow the majority to rule. However, all democracies include safeguards to limit the power of the majority when it might threaten minority rights (so-called "majority tyranny"). The initiative and referendum may achieve their majoritarian outcomes by overriding these safeguards. We know little at this point about how often that happens: do the initiative and referendum bring about majoritarian policies in situations where those policies ought to prevail, or in situations where they ought to be blocked? Another long-running debate concerns when voters are competent to make policy decisions, and when decisions should be left to government officials. Initially, many countries planned to adopt the European Constitution by referendum, but following voter rejections in France and the Netherlands in 2005, governments stopped asking for popular approval, and proceeded instead by unilateral action. It seems possible that the scope of the European Union would have been more limited if the choice had been up to voters rather than government officials. Whether that would have been a good or bad thing is debatable, but the point is that political elites are likely to make different decisions than ordinary citizens on some issues. We see this for social issues in the United States, where representatives typically choose more liberal policies than the voters choose. In a democracy, under what conditions (if any) is it better to delegate decisions to political elites and allow them to override the opinion of the majority? Credible answers to these questions will make it easier to reach conclusions about the overall merits of direct democracy, and the advisability of extending direct democracy to more issues and governments.

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## Appendix

This appendix explains why the congruence of jurisdictions with and without direct democracy cannot be compared from regressions of the form of Eq. (3). The discussion is adapted from Matsusaka (2001); see also Achen (1977) and Golder and Stramski (2010).

In a slight change in notation from the text, define congruence as

$$CONG_i = -|x_i - x_i^*|, \tag{5}$$

where  $x_i$  is the policy outcome that prevails in jurisdiction  $i$ , and  $x_i^*$  is the policy outcome that the voter would like to prevail (in the text  $V = x^*$ ). The preferred policy  $x^*$  could be the median voter’s ideal point, the majority position, or some other measure.

Now suppose that  $x^*$  cannot be observed, but that the researcher has access to a public opinion variable  $P$  that is correlated with  $x^*$  according to  $x^* = f(P)$ , where  $f$  is an increasing function. For example, the policy is the income tax rate and  $P$  is an ideology index or a vector of demographic variables. Critically, while we know that  $x^*$  and  $P$  are correlated, we do not know the precise functional form of  $f$ . Because  $P$  and  $x$  are measured on different scales (5) cannot be implemented. Consider instead a regression of the form

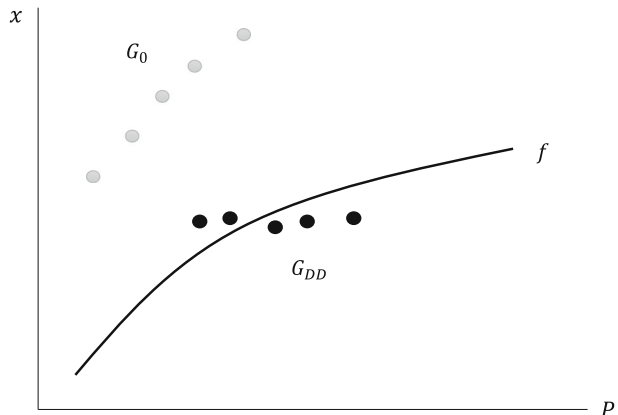
$$x_i = a + bP_i + u_i, \tag{6}$$

where  $a$  and  $b$  are coefficients to be estimated. The coefficient on the proxy for constituent preferences,  $b$ , is sometimes referred to as “responsiveness” in the literature. The question is: what is the formal connection between responsiveness  $b$  and congruence? The answer is: none, in general.

Consider Fig. 8. In a perfectly congruent world, the policy would be  $x = x^*$ , and all observations would lie on the  $f$  function:  $x = f(P)$ . In such a case, there would be a positive relation between outcomes and the preference proxy, and regression (6) would yield  $b > 0$ .

Now consider comparisons of congruence between jurisdictions with and without direct democracy. Denote the two groups we would like to compare as  $G_{DD}$  and  $G_0$ . We would like to measure the mean of  $CONG = -|x - f(P)|$  for each group, but  $f$  is not observable. Suppose instead that equation (6) is estimated separately for each group, producing responsiveness coefficients  $b_{DD}$  and  $b_0$  (or, as is more common in practice, a single regression is estimated with an interaction term that allows the coefficient on preferences to

**Fig. 8** Hypothetical policy-preference data



vary by group). What can we learn about relative congruence from a comparison of the two coefficients?

Figure 8 shows a hypothetical case. The cluster of points  $G_{DD}$  represents opinion-outcome observations for one group and the cluster labeled  $G_0$  represents observations for the other group. Note that in this example, (1) the policy outcomes for group  $G_0$  are less congruent (more distant) with public preferences than the outcomes for group  $G_{DD}$ , but (2) if regression (6) is estimated separately for the two groups, we would find  $b_{DD} < b_0$  (or, in an interaction framework with  $G_0$  as the null and  $G_{DD}$  as the interaction, we would find a negative coefficient on the interaction term). In this case, the regression estimates of  $b$  are inversely related to congruence. It is straightforward to construct examples in which the regression estimates of  $b$  are positively correlated with congruence. The implication is that the coefficient  $b$  is not an indicator of congruence, and therefore regressions (6) do not permit comparison of congruence between the two groups.

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