Policy Consequences of Direct Legislation in the States
Theory, Empirical Models and Evidence*

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Abstract

Most theoretical models predict that institutions allowing for direct legislation should lead, on average, to policies more closely reflecting the wishes of the voters. While some agreement exists at the theoretical level about the expected policy consequences of direct legislation, empirical evidence has been scant so far. In this paper I discuss the reasons for this scantness of empirical evidence, namely the intricacies of the adequate empirical model to test the theoretical proposition, and suggest possible solutions to this problem. Re-analyzing a dataset with which some authors have found no evidence in support of the theoretical claim, I show that with better adapted empirical models we find results in synch with our theoretical expectations. Thus, policies in states that allow for direct legislation reflect on average more closely the voters' wishes.

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1 Introduction

Direct legislation has no good press, currently. Journalists (e.g., Schrag 1998 and Broder 2000), as well as some observers (e.g., Smith 1998) find much at fault with policies adopted in processes of citizen-lawmaking. Implicit in their assessment is the assumption that policies in states allowing for direct legislation differ from policies adopted in states not allowing citizens to propose and vote on ballot measures.

Both the descriptive and theoretical literature on direct legislation largely concurs with this view. Nevertheless, the effect of institutions of direct legislation on policies has proven more controversial on the empirical level. In this paper I argue that this controversy is largely due to missspecfications in the empirical models used to assess the effect of direct legislation. More precisely, most theoretical models suggest that policies in states allowing for direct legislation should be biased toward the preferred policy of the median voter.[1] This bias obviously depends on what the policy would have been in the absence of institutions of direct legislation. Hence, when comparing policies across states with or without direct legislation it is necessary to control for the voters' preferences, since these determine the direction of the bias.

But most empirical specifications used in the literature to assess the effect of direct legislation fail to take this into consideration. Hence, the results obtained are often biased. While for some empirical specifications it is relatively easy to determine the bias, in others this proves elusive. Thus, empirical models taking into account the theoretically derived prediction of the effect of direct legislation should address these problems directly.

I propose in this paper two empirical models which derive directly from the theoretically implied predictions on the effect of direct legislation. Both of them are based on one restrictive assumption. In the empirical evaluation both yield, however, very similar results and support the theoretical implication on the effect of direct legislation.

The paper proceeds as follows. In the next section I review very briefly the literature on the policy effects of direct legislation. In section three I present first the theoretically implied empirical model, before discussing specifications that have been employed in the literature. I demonstrate that almost all of these specifications are based on erroneous assumptions which inevitably result in biases in the estimated co-

[1] Obviously, the notion of median voter only applies in contexts where the policy outcome reflects a one-dimensional policy space. Hug and Tsebelis (2000) and Tsebelis (2000) propose ways in which the general theoretical ideas discussed in this paper apply in multidimensional spaces.

efficients which are of relevance. In section four I use these faulty models and compare their results on the basis of a set of policies that have been studied by Lascher, Hagen and Rochlin (1996). I demonstrate that these different faulty models come to widely divergent conclusions, and none of them provides clear evidence in support of the theoretically implied prediction of the effect of direct legislation. In section five I derive from the theoretically implied empirical model two specifications, each relying on one simplifying assumption, solving, however, the problems of the previous specifications. I use these two empirical specifications to re-analyze the set of policies studied by Lascher, Hagen and Rochlin (1996) and find largely consistent support for the contention that direct legislation biases policy outcomes toward the policy preferred by the median voter. Section seven concludes and charts future research avenues.

2 The policy effects of direct legislation

There is a large consensus in the literature that institutions for direct legislation affect policy outcomes.[2] Only few authors, like for instance Cronin (1989, 232), argue that policies do not differ between political entities allowing for direct legislation, and those that do not. From the early incisive writings of Key and Crouch (1939), which were largely ignored by subsequent authors, it also seemed clear that the policy effects of direct legislation may be of two different sorts. First, the policy effects may be direct, in the sense that policies are adopted by voters, which would have failed in the normal legislative process. Second, institutions allowing for direct legislation may have indirect effects when the legislature adopts policies which it wouldn't have adopted without these institutions. Often such indirect effects emerge, when interested groups threaten legislatures with their own proposals, which they might try to realize through direct legislation.[3]

These two types of effects appear especially clearly in recent theoretical work. Steunenberg (1992), Gerber (1996 and 1999), Moser (1996), Matsusaka and McCarty (1998) and Hug (1999) all show, that the overall consequences of direct legislation comprise both direct and indirect effects.[4] From these theoretical models two other

[2] Gerber and Hug (forthcoming) discuss in much more detail the issues involved in this question.

[3] Matsusaka (2000, 658) adds a possible signaling effect, namely when ". . . election returns from initiative contests. . . convey information to representatives about citizen preferences that they later incorporate into policy."

[4] Strictly speaking, the incomplete information models of Steunenberg (1992) and Gerber (1996 and 1999) do not cover both types of effects. In Steunenberg's (1992) model initiatives always occur if the

important elements transpire. First, based on their theoretical implications it appears clearly that sorting out direct from indirect effects empirically is difficult. This, because these two effects interact and thus form the result of a strategic interplay among various actors. Second, the theoretical models also demonstrate that the policy effect of direct legislation, whether direct or indirect, is dependent on at least the preferences of the legislature and the voters.[5] The general thrust of the theoretical results is that under most conditions, policies adopted under direct legislation will be biased toward the preferences of the median voter.[6] For example, if the legislature would like to spend $ 1 million on a school building but the voters prefer spending $ 2 million, then direct legislation will, on average, lead to higher expenditures. Conversely, if the same legislature is on a spending spree and wishes to construct a school-palace for $ 10 million, direct legislation will lead to policies closer to the preferred spending level of frugal voters. Consequently, in one case direct legislation will lead to higher expenditures, while in another expenditures will be lower, due to direct citizen-lawmaking. This shows, that the effect of direct legislation is contingent on the preference configurations of the most important actors, namely voters and the legislature.

In summary, the theoretical literature suggests that institutions for direct legislation have both direct and indirect effects, but that these two types of effects are difficult to separate empirically. In addition, the direction of these effects is dependent on the voters preferences, in the sense that policies will reflect more closely these preferences under direct legislation than in the absence of these institutions.

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status quo is different from the voters' preferences, which implies that only direct effects are considered. In Gerber's (1996 and 1999) model votes never occur in equilibrium, since the legislature anticipates voter and interest group reactions. Thus, the predicted policy effects are only of the indirect nature.

[5]Most models comprise also an interest group or an opposition, which triggers direct legislation. Given that various such groups may fulfill this role, their preferences will empirically be of less relevance.

[6]It has to be noted that two models find that under very specific conditions, voters may be worse off under direct legislation. Matsusaka and McCarty (1998) show that if the legislature attempts to be a perfect agent of the voters, without knowing the latter's preferences, it may try to preempt ballot measures by adopting policies which are detrimental for the voters. This occurs, however, only if the legislature wishes to buy off an extreme interest group. Similarly, Hug (1999) also finds that if the legislature's and the voters' interest are close, direct legislation may lead to policies less preferred by the voters, because the legislature wishes to avoid ballot measures. In both models, however, this detrimental effect is dependent on the voters' preferences. For instance proposition 3 in Matsusaka and McCarty (1998, 16) states that policy outcomes may be more extreme under direct legislation, where "extremeness" is implicitly defined by the distance between the adopted policy and the expected value of the voters' preferences. Thus, both positive and negative effects of direct legislation from the perspective of voters are contingent on the latter's preferences.

3 The empirical models

While these insights from the theoretical literature are largely agreed upon, empirical evidence in their support has long proven to be elusive, or under sharp attack. Strictly speaking the theoretical models imply an empirical model of the following type:[7]

$$|PO_i - X_{m_i}| \;=\; f(dl_i, X_i) \tag{1}$$

where PO_i is a measure of a particular policy adopted in entity i, X_{m_i} is the median voter's preferred policy in this area, dl_i is an indicator variable for the presence of direct legislation and X_i a possibly empty set of control variables.

An early empirical investigation appears in Pommerehne (1978a and 1978b), who proposes an empirical study based on data covering roughly 100 Swiss cities. On the basis of aggregate measures the author estimates a median voter model, predicting the demand for public goods, or more precisely for spending levels.[8] In essence, Pommerehne's (1978a and 1978b) model is of the following type

$$PO_i \;=\; \beta_0 + \beta_{X_m} \times X_{m_i} + \epsilon_i \tag{2}$$

Since no direct measurement for the median voter's preferences are available, X_{m_i} stands here for a set of variables related to the median voter's demand for public goods.[9] This model is estimated for two sets of cities, namely those allowing for referendums on certain expenditures, and those which do not allow for such referendums. Pommerehne (1978a and 1978b) finds that his empirical model performs much better (σ_ϵ is smaller) for the set of cities allowing fiscal referendums, than for those which do not have this institution. He takes this as evidence that the voters' wishes

[7] Matsusaka (forthcoming) suggests using the square of the differences, but this alternative specification would not alter significant any of the derivations and statements that follow.

[8] The median voter model in this context is in some sense a translation of Black's (1958) median voter theorem to the empirical level. More precisely, work in this tradition, starting with Bowen (1943), suggests that voting may allow for determining the citizens' "demand" for public goods. This literature (e.g., Holcombe 1989 for a review) argues that the median income is a superior predictor in a demand function for public goods. Romer and Rosenthal (1979) question thoroughly the theoretical and empirical foundations for this claim.

[9] Below I will argue that this short-cut is problematic, since we seldom have a perfect preference measure. Hence, our variables measure with error the quantity of interest to us, namely the median voter's preferred policy outcome.

are better respected in cities with a fiscal referendum than in the remaining cities.[10] To assess whether this empirical setup is warranted, the question is whether equation 2 can be derived from equation 1. Rearranging equation 1 for two possible cases yields the following:

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\begin{aligned}
& if PO_i - X_{m_i} > 0 \\
& \qquad PO_i = f(dl_i, X_i) + X_{m_i} \\
& if PO_i - X_{m_i} \leq 0 \\
& \qquad PO_i = -f(dl_i, X_i) + X_{m_i}
\end{aligned}
\tag{3}
$$

It follows in a straightforward manner that equation 2 follows from equation 3, only if $f(dl_i, X_i)$ is zero, which reduces both equations to equation 2. But obviously if the theoretical models are correct $f(dl_i, X_i)$ is not equal to zero, which implies that dl_i, as omitted variable in equation 2, becomes part and parcel of ϵ_i. In addition, since dl_i is equal to 1 for direct legislation states and 0 otherwise, this omitted variable induces heteroskedasticity of a very predictable type. Estimating equation 2 based on the whole set of observations, however, has as additional problem that dl becomes a classical omitted variable. Theoretically we know that its effect is different from zero, and we may suspect that dl is correlated with some included variables. Hence, both slope estimates and, as a consequence, the residuals will be biased.

Pommerehne (1978a and 1978b) partly circumvents this problem by estimating two sets of equations, namely one for direct legislation municipalities, and the other for the remaining cities. Inspecting 3 it follows rather easily that σ_ϵ should be smaller for direct legislation states than for the remaining states. This is exactly what Pommerehne (1978a and 1978b) used as criterion to assess the effect of direct legislation. But, obviously the estimates obtained by Pommerehne are based on an erroneous specification. Thus, the residuals are also biased estimates of the error terms, which suggests that assessing the variance of the residuals is problematic.

This approach fell in some disrepute after the more general critique leveled by Romer and Rosenthal (1979) against the so-called median voter model. But their argument focused on the usefulness of using a particular summary of the income dis-

[10] He also assesses the elasticities of the demand function which correspond, given his linear specification, to the slope coefficients. As will become apparent below, this interpretation is problematic (see Matsusaka forthcoming).

tribution (the median) to predict demand for public goods, instead of any other distributional summary. Even though the forceful critique of Romer and Rosenthal (1979) also directly refers to Pommerehne's (1978a) work, their critique does not deal directly with the question discussed here. Nevertheless, Pommerehne's (1978a and 1978b) crude approach yields only correct assessments on the effect of direct legislation under very restrictive assumptions.[11]

Probably in part due to Romer and Rosenthal's (1979) critique of the median voter model, subsequent authors refrained from relying on Pommerehne's pioneering work. Almost all empirical models attempting to estimate the effect of direct legislation since then, employed the following model or a variation thereof (e.g., Chicoine, Walzer and Deller 1989):

$$PO_i = \beta_{X_m} \times X_{m_i} + \beta_{dl} \times dl_i + \epsilon_i \tag{4}$$

The effect of direct legislation is then tested for by assessing the size and sign of the estimated β_{dl}. But a comparison between equation 4 and equation 3 quickly highlights a shortcoming of this approach. The estimate of β_{dl} is only unbiased under the assumption that either $PO_i - X_{m_i} > 0$ or $PO_i - X_{m_i} \leq 0$ for all i's. If this condition is violated, the estimate for β_{dl} is biased toward zero.

The empirical model reported in equation 4 has been extensively used in studies of American states and municipalities, as well as of Swiss cantons and cities. Interestingly enough, despite the inherent bias in the estimated effects of direct legislation, most, if not all, studies find a strong and significant effect for direct legislation. Hence, Matsusaka (1995) finds that American states with direct legislation have lower spending levels between the 1960s and the 1990s. Feld and Matsusaka (2000) find similar effects for Swiss cantons.[12] Interesting in this context is Matsusaka's (2000) finding that in the first half of the 20th century direct legislation states had higher spending levels than the states without the initiative process. This finding, compared to the results obtained in Matsusaka (1995) clearly illustrates the logic of equation 3. As Matsusaka (2000) illustrates with qualitative material, the preferences of the voters in the first half of the 20th century were quite different from those of their fellow citizens at the end of the last century. Hence, most states started off in a situation characterized by one

[11] It has to be noted here, that the derivation of equation 2 from 3 relies in part on the assumption that X_{m_i} is a perfect measure of the median voter's preferred policy outcome.

[12] A careful review of most work in this tradition appears in Kirchgässner, Feld and Savioz (1999).

equation of 3 and have moved to the other one in the course of the century.[13]

The problem with this approach is that it does not allow the effect of direct legislation to be dependent on the voters' preferences. While Matsusaka's (1995 and 2000) work clearly shows that preferences differ across time, it is perfectly possible that they might also differ across space. As argued above, neglecting this conditional influence of direct legislation biases downward the estimated effect. An attempt to overcome this difficulty and to propose an adequate empirical model appears in Gerber's (1996 and 1999) work. She studies two policies, namely parental consent laws for teenage abortions and the death penalty. For both policies she aggregates preference measures for the fifty states, based on survey questions on these two policies. Since her dependent variables are dichotomous, namely presence or absence of these policies, she employs a logit setup. Thus, starting off from something similar to equation 1 she derives the following empirical model for dichotomous policies (omitting any control variables):

$$\frac{p(PO_i = 1)}{1 - p(PO_i = 1)} = e^{(\beta_0 + \beta_{X_m} \times X_{m_i} + \beta_{dl} \times dl_i \times X_{m_i} + \epsilon_i)} \quad (5)$$

In her empirical model appear the aggregated preference measure (X_{m_i}), the preference measure interacted with a dummy equal to one for all states with direct legislation, and various control variables.[14] She finds for both policies that the effect of preferences on the log-odds ratio is stronger in states with direct legislation than in all other states ($\beta_{dl} \neq 0$ and the signs of β_{X_m} and β_{dl} are identical).[15] Her empirical specification is adequate, because the preference variable can be considered as a measure of the latent construct implicit in logit-models. On this construct there exists a cutoff point above which a majority of voters prefers a given policy.[16] The empirical model then assesses how swiftly the predicted policy switches in direct legislation states and all others as the preferences move along this continuum. As is obvious from the logit

[13] Obviously, the direction of the movement depends on how we define PO_i and X_{m_i}.

[14] This empirical model corresponds almost completely to Bartels' (1991) model, which he uses to assess whether constituency opinions influence the voting behavior of congressmen in the area of defense. One variable, which according to Bartels (1991) might reinforce this relationship, is the closeness in the election of the congressman. Hence, he employs both constituency preferences as well as these preferences interacted with a measure for electoral closeness, among others, as independent variables.

[15] Employing the same empirical model Gerber and Hug (2001) find similar results for various policies aiming at protecting minorities.

[16] Given the fact that policies for teenage abortions and the prevalence of the death penalty differs from state to state, it is likely that a uniform question on these two policies used in the surveys elicits not a precise evaluation of the policies on books in the relevant states.

specification, if the overall effect of preferences on the log-odds ratio is bigger, this suggests that policy reacts more swiftly to changes in voter preferences.

Drawing directly or indirectly on this previous work, Lascher, Hagen and Rochlin (1996) as well as Camobreco (1998) attempt to build on Gerber's (1996 and 1999) work, as well as Matsusaka's (1995) for policies measured on continuous scales. They employ as dependent variables a series of policies, e.g. school expenditures, or tax revenues. As in Gerber (1996 and 1999) these authors employ a preference measure and interact this preference measure also with a dummy for direct legislation states. Their results are mixed at best and in essence provide no evidence for the theoretically grounded hypothesis that voters' preferences are better reflected in policies in direct legislation states. Matsusaka (forthcoming) demonstrates, however, that their empirical model is inadequate. The reason why can be seen rather easily by inspecting a simplified version of their model:

$$PO_i = \beta_0 + \beta_{X_m} \times X_{m_i} + \beta_{dl} \times dl_i \times X_{m_i} + \epsilon_i \qquad (6)$$

Comparing equation 6 with equation 1 clearly suggests, that there is no way to get from one equation to the other. Matsusaka (forthcoming) implicitly demonstrates this fact and rightly argues that the results reported by Lascher, Hagen and Rochlin (1996) and Camobreco (1998) do not relate to the theoretical question these authors pose. Hence their conclusions about the non-effect of direct legislation are questionable.

4 What can faulty empirical models tell us?

So far we explored a series of empirical models that have been proposed to study the effect of direct legislation on policy outcomes. The model that reflects most closely the theoretical model implied by most formal work, namely Gerber's (1996 and 1999) logit specification, however, only applies to dichotomous policies. Simply transposing her specification to a model for a continuous policy clearly fails, as Matsuaka (forthcoming) demonstrates. But how do the different empirical specifications discussed above fare in the case of the set of policies studied by Lascher, Hagen and Rochlin (1996)? These authors use measures for eight policies adopted at the state level, as well as a summary index covering these policies (Table 1).[17] These policies

[17]Descriptive statistics for these policies, as well as for all other variables employed in this paper appear in the appendix. For more precise definition of the variables, I refer to the sources mentioned in

Table 1: Policies and their measurement

label	policy and source
adc80	"scope of Aid to Families with Dependent Children" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
consume2	"enactment of various consumer protection laws" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
crimjus2	"use of different approaches to criminal justice" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
exppupil	"educational spending per pupil" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
eraboles	"years from passage of the Equal Rights Amendment (ERA)" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
gambling	"extent to which legalized gambling is allowed" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
medicar2	"scope of the Medicaid program" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
lowry2	"tax progressivity" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)
zpolicy	"summary index of the eight policies" (Erikson, Wright and McIver 1993, 75-78, Lascher, Hagen and Rochlin 1996, 765)

are aid for families with dependent children (adc80), consumer protection laws (consume2), criminal justice (crimjus2), school expenditures per pupil (exppupil), equal rights amendment (eraboles), adoption of gambling (gambling), medicare (medicar2), taxes (lowry2) as well as the summary index (zpolicy). Each of these policies Lascher, Hagen and Rochlin (1996) regress on the percentage of high school graduates (HS grad), income, the percentage of urban population, and a measure of state ideology derived from Erikson, Wright and McIver (1993). Lascher, Hagen and Rochlin (1996) consider the latter variable as a preference proxy for the eight policies and the summary index, while the other variables appear as controls in their models. Hence, following equation 6 they interact their ideology measure with a direct legislation dummy, and introduce the latter as additional regressors.

I use this specification by Lascher, Hagen and Rochlin (1996) as starting point for an assessment of the various faulty empirical models. As Lascher, Hagen and Rochlin (1996) these analyses only cover 47 states, since according to Erikson, Wright and McIver (1993) the ideology measure for Nevada, Hawaii and Alaska is unreliable, and these states are thus omitted. Table 2 summarizes the crucial elements of each specification.[18] Column 1 of table 2 recapitulates in some sense Lascher, Hagen and Rochlin's (1996) result, by only reporting the adjusted r^2 of a replication analysis of their eight policies and their summary index.[19] I report this problematic statistic, because it is the

table 1.

[18]Complete results can be obtained from the author and will appear shortly on his web-page.

[19]I report Lascher, Hagen and Rochlin's (1996) results, as well as my replication thereof in columns 1 and 2 of tables 3 to 11 for each of these policies.

Table 2: Exploring eight policies with different faulty methods

	replication adj. r^2	dl=0 adj. r^2	dl=1 adj. r^2	dl=0 residuals sd	dl=1 residuals sd	dl b (s.e.)	dl b (s.e.)	dl $\times$ sig b (s.e.)	F-test joint sign.
adc80	0.66	0.77	0.32	48.15	52.99	2.72	17.47	-2.14	
						(18.47)	(36.07)	(4.49)	
consume2	0.38	0.49	0.32	2.41	3.39	-0.69	1.41	-0.3	
						(1.06)	(2.04)	(0.25)	
crimjus2	0.16	0.20	0.05	92.36	137.25	-27.25	121.35	-21.6**	*
						(41.93)	(77.4)	(9.63)	
exppupil	0.72	0.76	0.74	27.78	21.67	-5.26	-14.87	1.4	
						(9.25)	(18.03)	(2.24)	
eraboles	0.45	0.47	0.43	1.87	1.91	-1.39**	-0.29	-0.16	*
						(0.68)	(1.32)	(0.16)	
gambling	0.62	0.63	0.56	116.2	93.11	66.73*	40.32	3.84	
						(38.66)	(75.57)	(9.40)	
medicar2	0.42	0.47	0.29	12.78	13.27	-0.88	1.43	-0.34	
						(4.77)	(9.32)	(1.16)	
lowry2	0.23	0.31	0.00	3.75	4.14	-1.27	4.07	-0.78**	**
						(1.44)	(2.64)	(0.33)	
zpolicy	0.76	0.80	0.66	0.46	0.50	-0.12	0.45	-0.08**	
						(0.17)	(0.33)	(0.04)	
n	47	26	21	(26)	(21)	47		47	

indicator on which Pommerehne (1978a and 1978b) relies in his analyses when comparing cities with direct legislation to those without. The corresponding comparison for the American states appears in columns 2 and 3 of table 2. In column 2 I report the adjusted r^2 for the estimation based on the 26 states with no direct legislation, while in column 3 appear the adjusted r^2 for the 21 direct legislation states. Since the adjusted r^2 are systematically higher in column 2 than in column 3 we would conclude, based on Pommerehne's (1978a and 1978b) approach, that the theoretical model fails to find empirical support.

Such a quick decision neglects, however, two crucial points. First, Pommerehne's (1978a and 1978b) approach is a very crude approximation of the theoretical model, since it relegates the effect of direct legislation into the disturbances. Second, the r^2 as a goodness of fit measure suffers from various shortcomings. One of them is that we compare this fit measure over two sub-samples, which are, in addition, of unequal size. This is especially problematic, since the slope coefficients are estimated with more information for the states with no direct legislation. A way around this problem is to estimate the slope coefficients for all states together and then to compare the standard deviation of the residuals.[20] The results for these analyses appear in columns 4 and 5 of table 2. In all cases, except for the education expenditures, the standard deviation of the residuals is larger for direct legislation states. Thus, again we might be tempted to reject the theoretical model. But two reasons speak against such a hasty decision. First,

[20]Obviously, such an estimation raises again the specter of the direct legislation as an omitted variable.

given the unequal size of the two subsamples, it is obvious that the slope coefficients reflect more strongly the relationship found in the states with no direct legislation. Second, even if this were not too much of a problem, we are still faced with the problem that Pommerehne's (1978a and 1978b) approach does not reflect the theoretical model, and, given the omitted variable, leads to biased results.

Apart Pommerehne's (1978a and 1978b) empirical model we might also consider the specification most frequently used in studies on expenditures, taxes and debt levels, which rely on estimating the coefficient for a direct legislation dummy. Using exactly the same specification as Lascher, Hagen and Rochlin (1996), but dropping the interaction term between preferences and the direct legislation dummy, we get the estimates for the slope coefficient of the dummy reported in column 6 of table 2. Overall the estimated slope coefficients are negative, with two exceptions, namely those for adc80 and gambling. Similarly, only for two policies is this coefficient significant at the 0.1 level, namely for the ERA-policy and gambling. Again, this is hardly overwhelming support for the theoretical model. Following our discussion of the empirical models above, we know, however, that these estimates are biased toward zero. A further complication comes from more recent theoretical models (e.g. Matsusaka and McCarty 1998 and Hug 1999), which suggest that the effect of direct legislation diminishes as the costs of submitting a ballot proposal increase. Consequently, as the signature requirement for submitting a proposal increases, the effect of direct legislation should diminish.[21] This implies that we should not only include a dummy for direct legislation, but also the signature requirement for the direct legislation states. The estimated coefficients for the central variables of this model appear in columns 7 and 8 of table 2. In column 9 I report the results of a joint significance test.[22] For three policies we find a jointly significant effect, namely for criminal justice, ERA and taxes (lowry2). In addition, for three policies, namely criminal justice, taxes as measured by Lowry and the summary indicator (zpolicy) the signature requirement is significant and of the opposite sign as the direct legislation dummy. This is the case for all policies, except ERA and gambling. Thus, when taking into account more recent theoretical results which stress the effect of the signature requirement, we get some additional support for the theoretical model, but overall this support still remains weak. Again, this should not surprise,

[21] Matsusaka and McCarty (1998), Hug (1999), Gerber and Hug (2001), and Matsusaka (2000) find empirical support for this theoretical claim.

[22] Throughout this paper I use two-tailed tests of significance, where $**$ indicates significance with $p < 0.05$, while $*$ denotes a significance level of $p < 0.10$.

since as emphasized repeatedly, the effects estimated with this empirical model are biased toward zero.

5 Appropriate tests of the theoretical model

Strictly speaking equation 3 would be the appropriate empirical model to be used to assess the effect of direct legislation. There is, however, one additional complication, to which we alluded at several instances, namely that we generally do not have perfect measures for the median voter's preferences (X_{m_i}). But if we have to estimate X_{m_i}, we cannot derive equation 3 directly from equation 1. We may presume that X_{m_i} is related to an array of variables P_i presumably linked to the preferences of median voter:

$$X_{m_i} = f(P_i) \tag{7}$$

Assuming (falsely) linear relationships for both equations 1 and 7 we get the following system of equations:

$$\begin{aligned} X_{m_i} &= \beta_0 + \beta_1 \times P_i + \epsilon_i \\ |PO_i - X_{m_i}| &= \gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i \end{aligned} \tag{8}$$

Assuming further that $\epsilon_i \sim N(0, \sigma_\epsilon^2)$ and $\theta_i \sim N(0, \sigma_\theta^2)$ we may use equation 8 to derive the log-likelihood function for PO in the following way

$$\begin{aligned} & if PO_i - X_{m_i} > 0 \\ PO_i &= \beta_0 + \beta_1 \times P_i + \epsilon_i + \gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i \\ &= \beta_0 + \beta_1 \times P_i + \gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \epsilon_i + \theta_i \\ & if PO_i - X_{m_i} \leq 0 \\ PO_i &= \beta_0 + \beta_1 \times P_i + \epsilon_i - \gamma_0 - \gamma_1 \times dl_i - \gamma_2 \times X_i - \theta_i \\ &= \beta_0 + \beta_1 \times P_i - \gamma_0 - \gamma_1 \times dl_i - \gamma_2 \times X_i + \epsilon_i - \theta_i \end{aligned} \tag{9}$$

Depending on the assumptions made for the joint distribution of ϵ and θ, the likelihood function may be pieced together. Based on the assumption that $\epsilon_i \sim N(0, \sigma_\epsilon^2)$

and $\theta_i \sim N(0, \sigma_\theta^2)$ it follows easily that $\epsilon + \theta \sim N(0, \sigma_\epsilon^2 + \sigma_\theta^2 + 2\sigma_{\epsilon,\theta})$ and $\epsilon - \theta \sim N(0, \sigma_\epsilon^2 + \sigma_\theta^2 - 2\sigma_{\epsilon,\theta})$. Hence the log-likelihood function looks something like the following:

$$\begin{aligned} llik &= \sum_i a_i * f_N(PO - \beta_0 - \beta_1 \times P_i - \gamma_0 - \gamma_1 \times dl_i - \gamma_2 \times X_i, \sigma^2_{\epsilon+\theta}) \\ &\quad + \sum_i (1 - a_i) * f_N(PO - \beta_0 - \beta_1 \times P_i + \gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i, \sigma^2_{\epsilon-\theta}) \end{aligned} \tag{10}$$

where $a_i = 1$ if $PO_i - X_{m_i} > 0$ and else $a_i = 0$. Since the distribution functions in the two pieces of the log-likelihood function differ, an additional parameter has to be estimated, which corresponds to a multiple of the covariance of ϵ and θ. Apart this complication, the estimation of the parameters through maximizing equation 10 is almost trivial.

The estimation of the implicit system of equation 8 is only possible because of the manifestly erroneous assumption of linear relationships. Given that the absolute distance between the policy outcome and the median voter's preference is to be explained, the dependent variable can only take positive values. Hence, a correct specification would take this into account, for instance by assuming the following:

$$\begin{aligned} X_{m_i} &= \beta_0 + \beta_1 \times P_i + \epsilon_i \\ |PO_i - X_{m_i}| &= e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i} \end{aligned} \tag{11}$$

Assuming again that $\epsilon_i \sim N(0, \sigma_\epsilon^2)$ and $\theta_i \sim N(0, \sigma_\theta^2)$ we may use equation 11 to arrive at the following equation

$$|PO_i - \beta_0 - \beta_1 \times P_i - \epsilon_i| = e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i} \tag{12}$$

Equation 12 may again be disassembled into two pieces:

$$\begin{aligned} &if PO_i - X_{m_i} > 0 \\ &\qquad PO_i = e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i} + \beta_0 + \beta_P \times P_i + \epsilon \\ &if PO_i - X_{m_i} \leq 0 \\ &\qquad PO_i = -e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i + \theta_i} + \beta_0 + \beta_P \times P_i + \epsilon \end{aligned} \tag{13}$$

Since one of the error terms (θ_i) enters equation 13 multiplicatively, while the other (ϵ_i) enters it additively, no simple likelihood function can be pieced together. Only by setting one of these two error terms to zero is it possible to derive a log-likelihood function.[23] Setting $\theta_i = 0 \forall i$ yields the following expression, which again can easily be used to derive a likelihood function:

$$\begin{aligned} & if PO_i - X_{m_i} > 0 \\ & \qquad PO_i = e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i} + \beta_0 + \beta_P \times P_i + \epsilon \\ & if PO_i - X_{m_i} \leq 0 \\ & \qquad PO_i = -e^{\gamma_0 + \gamma_1 \times dl_i + \gamma_2 \times X_i} + \beta_0 + \beta_P \times P_i + \epsilon \end{aligned} \tag{14}$$

Hence, it appears that correctly estimating the parameters of interest in equations 1 and 7 is only possible by adopting one of two manifestly false assumptions. First, we may assume that the equation linking explanatory variables to the difference between policy outcome and the median voter's preference is linear. Under this assumption we can estimate at the same time the effect of preferences and direct legislation on policy outcomes. Second, we may acknowledge that the outcome equation is nonlinear, but then assume that our various proxy variables completely accurately predict the median voter's prefered policy outcome.[24]

6 Policy consequences of direct legislation

Thus, the above discussion suggests two possibilities to estimate the effect of direct legislation, both based on a simplifying assumption. The first is to assume that the substantive interesting part of equation 8 is a linear relationship. The second relies on the false assumption that our proxies for the preferences of the median voter perfectly predict the median voter's preferred policy outcome.

[23] This is implicitly the strategy adopted by Bartels (1991), who regresses his survey-based preference measure on a series of constituency characteristics, and then employs the predicted values from this auxiliary regression as predictor in the voting equation of congressmen. While this approach addresses the issue of measurement error in the preference variable, it is far from certain that purging the variable in the way proposed by Bartels (1991) solves the matter highlighted here.

[24] This assumes that $\epsilon_i = 0 \forall i$. Obviously, we could also assume that $\theta_i = 0 \forall i$, but it seems much less plausible that the inferred preferences of the median voter plus some control variables accurately predict the policy outcome in a state, as measured by our dependent variables.

Employing Lascher, Hagen and Rochlin's (1996) dataset I explore these two empirical models with different specifications.[25] The results appear in tables 3 to 11. In the first column of each table I report the results taken from Lascher, Hagen and Rochlin's (1996) article. Column 2 reports my replication of their results using exactly the same specification.[26] In columns 3, 4 and 5 I report the results from three specifications where I assume that the outcome equation is linear.[27] The three specifications differ by the additional control variables that I introduce. The first (column 3) is largely a replication of Lascher, Hagen and Rochlin's (1996) specification, though with the more appropriate empirical model. Based on the discussion in the previous section I add in the next specification the signature requirement as additional variable for the direct legislation states.[28] Finally, in the third specification I add two additional control variables.[29] In columns 6, 7 and 8 I report the results from the same three specifications, however, under the assumption that our proxies for the median voter's preferred policy are perfect predictors.

As a reminder, Lascher, Hagen and Rochlin (1996) interpret their results in the same fashion as Bartels' (1991) and Gerber (1996 and 1999) interpret theirs obtained in a nonlinear framework of a probit, respectively logit estimation. Thus, if their interpretation were correct, we would expect a significant effect both for the preference measure and the preference measure interacted with the direct legislation dummy. In

[25] I estimated the same models also for affirmative action policies in public contracting and comparable worth policies (Santoro and McGuire 1997). The results are largely similar, and in addition allow for an analysis covering all 50 states. For brievity's sake, I refrain from reporting these results in this version of the paper, but they will appear shortly on my webpages.

[26] Given the very different scales of the independent variables employed by Lascher, Hagen and Rochlin (1996), some estimated coefficients are difficult to interpret. For this reason I divided their income measure by 1000, so that the estimated coefficients for this variable become more easily interpretable. Similarly, they only report the 0.05 significance levels. Given the small samples involved, I report in all analyses both the 0.1 ($*$) and 0.05 ($**$) significance levels. For the results that I directly quote from Lascher, Hagen, and Rochlin (1996), I can only derive and report the 0.05 significance level.

[27] As transpires from the tables, I fail to report estimations for some models. The reason for this is that the estimations failed to converge in a reasonable time-frame. The fault lies with my impatience and probably also with the sensitivity of the estimation procedure to the starting values. Scheduled Monte-Carlo simulations should give us a much more robust assessment of the proposed estimation procedure. A point to which I will come back below.

[28] The direct legislation dummy reflects whether a state allowed for initiatives in 1990, and thus excludes Mississippi from the initiative states. The signature requirement is zero for all non-initiative states and equal to the lowest percentage of signatures required for a ballot measue across statutory and constitutional initiatives.

[29] The two control variables relate to characteristics of the legislature, namely the turnover rate in the state's house of representatives (Council of State Governments 1997) and the professionalization of the state legislature (Squire 1992). Both controls are also used by Gerber (1996 and 1999) and Gerber and Hug (2001).

Table 3: Explaining AFDC

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init	init	init	init	init
				initp	initp		initp	initp
					controls			controls
	b	b	b	b	b	b	b	b
	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)
HS grad	4.40	4.76**	2.68**	-	2.59**	2.71**	2.80	3.11
	(1.68)	(1.43)	(0.75)		(0.83)	(0.90)	(0.76)	(0.75)
income	0.00	6.50	12.20**		17.30**	10.60**	14.02**	5.93
	(0.00)	(10.06)	(6.10)		(6.70)	(6.08)	(6.65)	(6.18)
urban	0.01	2.16	-22.50		-28.28**	-15.34	-35.09*	-30.91*
	(0.44)	(37.43)	(23.61)		(26.32)	(22.29)	(24.47)	(23.21)
libop	8.35*	8.39**	4.99**		5.40**	4.84**	4.25**	3.46**
	(1.71)	(1.76)	(0.76)		(0.81)	(0.77)	(0.75)	(0.78)
init	78.2**	-79.55**						
	(32.70)	(32.64)						
init x libop	-6.12**	-6.00**						
	(2.02)	(2.04)						
constant		-17.99	49.26		22.83	56.21	25.62	52.69
		(101.37)	(51.02)		(55.36)	(53.10)	(53.26)	(57.36)
init			-5.38		-8.17	-0.15	-0.58	-0.62
			(8.45)		(18.84)	(0.18)	(0.51)	(0.53)
initp					0.68		0.05	0.04
					(2.26)		(0.06)	(0.07)
proflegi					-20.88			0.92*
					(31.72)			(0.66)
turnhous					-0.05			-0.01
					(0.35)			(0.01)
constant 2			49.25**		54.48**	3.92**	3.96**	3.94**
			(6.37)		(13.65)	(0.13)	(0.12)	(0.27)
σ_ϵ		48.41	29.49		29.10	26.83	26.94	26.91
			(2.78)		(2.90)	(0.03)	(0.07)	(0.07)
$2\sigma_{\epsilon,\theta}$			4.26		2.89			
			(7.15)		(7.26)			
llik			-221.21		-221.88	-221.30	-221.49	-221.45
n	47	47	47		47	47	47	47

addition, the sign for these coefficients should be identical, since the presumption is that the direct legislation reinforces the effect of preferences on the policy outcome.

As the first two columns in tables 3 to 11 show, for no policy does the predicted pattern appear for the estimates. While the effect of the preference measure on policy is statistically significant for seven policies, the coefficient for the interaction is only statistically significant for one policy, namely gambling. In addition in this case, the effect of the ideology scale on the adoption of gambling is strong and positive among the whole set of states, while it is reduced among the direct legislation states by almost half. Consequently, based on these results, Lascher, Hagen and Rochlin (1996) rightly claim that there is no evidence that direct legislation has the effect of a “gun behind the door.”

Unfortunately, as discussed above, their results are based on an erroneous empirical model. Thus, it is interesting to explore what insights we would glean from the results obtained from the corrected empirical models discussed above. Obviously, in both empirical models the crucial tests shifts away from an interaction term to the effect of the direct legislation dummy. More precisely, the theoretical model predicts, that its

Table 4: Explaining consumer policy

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	0.25**	0.20**	0.32**	-	-0.00	0.10**	0.07**	0.05*
	(0.11)	(0.09)	(0.05)		(0.00)	(0.04)	(0.04)	(0.04)
income	0.00	0.20	-0.30		-0.37	0.94**	1.55**	1.63**
	(0.00)	(0.63)	(1.45)		(1.36)	(0.36)	(0.26)	(0.26)
urban	0.04	2.50	-0.00		0.33	0.25	-1.10	-1.55*
	(0.03)	(2.35)	(0.00)		(0.05)	(1.37)	(1.13)	(1.20)
libop	0.15	0.12	0.06		0.06	0.22**	0.25**	0.25**
	(0.11)	(0.11)	(0.05)		(0.05)	(0.05)	(0.05)	(0.05)
init	0.46	0.66						
	(2.05)	(2.05)						
init x libop	0.09	0.10						
	(0.13)	(0.13)						
constant		-0.84	-1.24		-2.93	1.51	-0.54	0.19
		(6.36)	(3.51)		(3.40)	(2.78)	(2.59)	(1.82)
init			1.23**		-0.73	0.37*	-0.62**	-0.51*
			(0.56)		(1.09)	(0.26)	(0.37)	(0.35)
initp					0.28**		0.12**	0.12**
					(0.13)		(0.02)	(0.02)
proflegi					-0.41			-0.94**
					(2.05)			(0.55)
turnhous					-0.02			-0.01*
					(0.02)			(0.01)
constant 2			2.36**		2.87**	0.65**	0.78**	1.20**
			(0.38)		(0.74)	(0.17)	(0.15)	(0.20)
σ_ϵ		3.04	1.62		1.61	1.74	1.59	1.52
			(0.14)		(0.14)	(0.01)	(0.01)	(0.01)
$2\sigma_{\epsilon,\theta}$			-0.26		-0.14			
			(0.39)		(0.38)			
llik			-91.29		-93.58	-92.78	-88.62	-86.47
n	47	47	47		47	47	47	47

effect should be negative, namely reducing the distance between the median voter's preferred policy and the policy outcome. Looking at columns 3 and 6 in tables 3 to 11, where exactly the same variables as those employed by Lascher, Hagen and Rochlin (1996) appear, we find that for all policies the estimated effect of direct legislation is negative, except for the first specifications in each set in tables 4, 10 and 11, all specifications in table 5, and in one out of six specifications in tables 6, 7 and 9.

While these deviations from the expected pattern seem at the outset important, a closer looks shows considerable support for the theoretically expected effects, especially when considering the specification including the signature requirement. For all policies, with the exception of criminal justice policies (table 5) we find the expected pattern of coefficients. Strongly statistical significant results in support of the theoretically predicted effects appear in table 4, 7, 9, and 10.

Thus our results suggest that for the consumer policies, the adoption of the equal rights amendment, taxes, as well as the summary policy index, direct legislation significantly leads to policies reflecting more closely the voters wishes. For the remaining policies, we still find estimated coefficients which are largely in synch with theory, but

Table 5: Explaining criminal justice

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	4.22	3.64	1.03	0.97	-	2.43*	2.45*	2.02
	(4.22)	(3.55)	(1.55)	(1.52)		(1.65)	(1.67)	(1.67)
income	0.00	10.57	22.60**	28.70**		6.82	4.61	7.99
	(0.02)	(25.08)	(12.30)	(13.40)		(13.11)	(15.22)	(15.79)
urban	0.60	53.25	-29.12	-43.67		-90.22**	-82.28*	-80.57*
	(1.11)	(93.29)	(52.38)	(52.84)		(48.85)	(56.08)	(56.41)
libop	4.27	3.54	7.03**	6.77**		6.31**	6.36**	6.88**
	(4.31)	(4.38)	(1.66)	(1.67)		(1.62)	(1.62)	(1.65)
init	15.30	16.79						
	(82.10)	(81.35)						
init x libop	3.05	3.21						
	(3.09)	(5.07)						
constant		-145.60	-18.03	-63.29		23.69	38.16	46.69
		(252.67)	(111.37)	(118.38)		(111.12)	(120.97)	(122.47)
init			48.61**	9.76		0.39**	0.50	0.60*
			(20.06)	(43.74)		(0.19)	(0.42)	(0.41)
initp				5.50			-0.02	-0.02
				(5.47)			(0.06)	(0.05)
proflegi								-0.83
								(0.88)
turnhous								-0.01
								(0.01)
constant 2			71.25**	70.33**		4.50	4.51	4.73
			(13.24)	(13.03)		(0.16)	(0.16)	(0.27)
σ_ϵ		120.66	70.02	71.80		61.41	61.35	60.31
			(5.34)	(5.48)		(0.16)	(0.16)	(0.16)
$2\sigma_{\epsilon,\theta}$			17.30	22.07				
			(15.97)	(15.82)				
llik			-260.70	-260.22		-260.21	-260.17	-259.37
n	47	47	47	47		47	47	47

most of them fail to be statistically significant, though of the expected sign. Comparing these results to those reported in Lascher, Hagen and Rochlin (1996), it appears that both a faulty empirical model, as well as the omission of a crucial independent variable, namely the signature requirement, may explain these striking differences. While Matsusaka (forthcoming) demonstrates that their empirical model cannot provide results which speak to the theoretically derived hypothesis, the results reported here show that their results are erroneous. Finding comfort in the fact that both empirical specifications proposed in this paper largely yield concurring results with respect to the estimates of central interest in the context, the models proposed here appear to perform adequately.

Table 6: Explaining education

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	0.48	0.79	-	-	1.70**	1.24**	1.55**	1.33**
	(0.93)	(0.75)			(0.37)	(0.05)	(0.29)	(0.34)
income	0.01**	16.06**			13.60**	14.49**	10.91**	17.71**
	(0.00)	(5.31)			(3.40)	(0.02)	(3.20)	(2.83)
urban	-0.10	-3.64			2.27	26.49	6.13	7.36
	(0.24)	(19.77)			(13.46)	(0.01)	(11.01)	(11.50)
libop	4.76**	4.38**			3.60**	3.15**	3.63**	2.87**
	(0.95)	(0.93)			(0.36)	(0.11)	(0.37)	(0.37)
init	33.00	-33.94						
	(18.10)	(17.24)						
init x libop	-2.34*	-2.09**						
	(1.12)	(1.08)						
constant		107.04*			58.28**	56.453*	89.21**	27.19**
		(53.54)			(27.09)	(0.00)	(22.00)	(25.16)
init					-8.60	0.01	-0.18	-0.01
					(8.60)	(0.76)	(0.59)	(0.47)
initp					0.59		0.01	-0.10
					(1.08)		(0.08)	(0.08)
proflegi					57.30**			2.43**
					(16.06)			(0.50)
turnhous					-0.09			-0.01
					(0.17)			(0.01)
constant 2					11.99**	3.03**	3.06**	2.73**
					(6.15)	(0.25)	(0.14)	(0.25)
σ_ϵ		25.57			11.49	15.46	15.39	12.95
					(1.06)	(0.02)	(0.10)	(0.03)
$2\sigma_{\epsilon,\theta}$					-3.92			
					(3.38)			
llik					-189.66	-195.40	-195.18	-187.08
n	47	47			47	47	47	47

Table 7: Explaining ERA

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init	init	init	init	init
				initp	initp		initp	initp
					controls			controls
	b	b	b	b	b	b	b	b
	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)	(s.e.)
HS grad	0.15**	0.12**	-0.01	-0.01	-0.01	0.11**	-0.01	-0.02**
	(0.07)	(0.06)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
income	0.00	0.23	0.00	-0.10	0.00	-0.36	-0.10	0.19*
	(0.00)	(0.40)	(0.10)	(0.10)	(0.10)	(0.30)	(0.17)	(0.14)
urban	-0.03**	-3.41**	0.83**	1.01**	0.83**	0.14	1.49**	-0.26
	(0.02)	(1.51)	(0.49)	(0.51)	(0.58)	(0.98)	(0.61)	(0.68)
libop	0.27**	0.24**	0.00	0.00	0.00	0.26**	-0.00	-0.03**
	(0.07)	(0.07)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)
init	-2.69**	-2.52**						
	(1.29)	(1.31)						
init x libop	-0.08	-0.08						
	(0.08)	(0.08)						
constant		-0.63	3.37**	3.56**	2.78**	3.70**	3.42**	2.12**
		(4.08)	(0.84)	(0.87)	(0.86)	(1.88)	(0.89)	(1.04)
init			-0.04	-0.52	-0.27	0.44**	-0.41**	-0.33**
			(0.15)	(0.44)	(0.26)	(0.23)	(0.20)	(0.18)
initp				0.06	0.04		0.04*	0.02
				(0.05)	(0.03)		(0.02)	(0.02)
proflegi					1.05			0.70
					(0.70)			(0.25)
turnhous					0.00			0.00
					(0.00)			(0.00)
constant 2			2.76**	2.82**	2.40**	0.38	1.08	0.95
			(0.15)	(0.15)	(0.24)	(0.16)	(0.04)	(0.08)
σ_ϵ		1.95	0.71	0.67	0.70	1.13	0.55	0.57
			(0.05)	(0.05)	(0.05)	(0.00)	(0.00)	(0.00)
$2\sigma_{\epsilon,\theta}$			0.44	0.38	0.47			
			(0.12)	(0.13)	(0.13)			
llik			-35.79	-35.07		-72.33	-38.98	-40.17
n	47	47	47	47	47	47	47	47

Table 8: Explaining gambling

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	1.77 (3.65)	-1.30 (3.11)	6.77** (1.76)	1.42 (1.36)	1.50 (1.42)	6.35** (1.65)	1.66 (1.37)	5.80** (1.76)
income	-0.01 (0.01)	11.05 (21.94)	-1.30 (13.10)	22.50** (11.10)	20.40* (11.50)	-0.83 (13.79)	21.69* (11.83)	1.23 (15.62)
urban	0.54 (0.96)	-34.26 (81.60)	27.08 (50.22)	-41.97 (42.65)	-33.66 (41.56)	19.53 (49.52)	-33.50 (43.69)	-6.50 (58.42)
libop	24.7** (3.70)	23.65** (3.83)	17.04** (1.63)	7.88** (1.65)	8.09** (1.66)	17.32** (1.69)	7.91** (1.66)	16.05** (1.76)
init	-84.80 (71.10)	-67.89 (71.16)						
init x libop	-10.10** (4.40)	-9.83** (4.44)						
constant		570.06** (221.01)	44.92 (124.15)	102.37 (105.89)	115.86 (114.29)	76.21 (119.12)	90.28 (109.08)	78.47 (118.19)
init			-10.60 (19.72)	-60.68* (35.14)	-45.67 (31.65)	-0.08 (0.22)	-0.55 (0.44)	-0.19 (0.57)
initp				3.07 (4.12)	0.91 (4.04)		0.03 (0.05)	0.00 (0.07)
proflegi					-16.21 (59.73)			-0.29 (0.86)
turnhous					-0.57 (0.65)			-0.01 (0.01)
constant 2			95.98** (13.04)	126.39** (12.10)	142.30** (24.40)	4.54** (0.14)	4.83** (0.10)	4.86** (0.29)
σ_ϵ		105.54	57.54 (4.22)	59.24 (4.66)	58.19 (4.56)	62.97 (0.17)	54.12 (0.14)	62.16 (0.17)
$2\sigma_{\epsilon,\theta}$			-11.30 (14.92)	10.23 (12.66)	8.59 (12.52)			
llik			-260.92	-253.97	-253.85	-261.39	-254.28	-260.79
n	47	47	47	47	47	47	47	

Table 9: Explaining medicare

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	0.22	0.32	0.03	-0.21	-0.19	0.24	0.43**	-0.15
	(0.47)	(0.40)	(0.16)	(0.17)	(0.19)	(0.27)	(0.25)	(0.17)
income	0.00	-1.41	2.80*	5.10**	3.30*	2.69*	3.98**	5.49**
	(0.00)	(2.80)	(1.50)	(1.60)	(1.90)	(1.71)	(1.93)	(1.61)
urban	0.07	11.91	10.93*	8.04	14.09**	16.31**	1.44	0.42
	(0.12)	(10.42)	(5.63)	(6.09)	(6.19)	(7.82)	(8.19)	(5.33)
libop	1.64**	1.87**	0.56**	0.42**	0.56**	1.31**	1.19**	0.48**
	(0.48)	(0.49)	(0.19)	(0.20)	(0.19)	(0.21)	(0.17)	(0.19)
init	-10.40	-11.34						
	(9.20)	(9.09)						
init x libop	-0.74	-0.76						
	(0.57)	(0.57)						
constant		138.02**	104.77**	99.77**	111.35**	93.80**	77.61**	96.87**
		(28.23)	(11.70)	(12.19)	(13.12)	(13.87)	(14.60)	(12.72)
init			-1.82	-10.48**	-9.03**	0.07	-0.85**	-1.14**
			(2.03)	(4.36)	(4.41)	(0.17)	(0.45)	(0.38)
initp				1.18*	0.88		0.11**	0.14**
				(0.54)	(0.59)		(0.04)	(0.04)
proflegi					10.80			0.67
					(7.17)			(0.55)
turnhous					-0.01			-0.00
					(0.08)			(0.01)
constant 2			13.38**	13.75**	11.49**	2.44**	2.39**	2.51**
			(1.38)	(1.41)	(2.92)	(0.12)	(0.13)	(0.23)
σ_ϵ		13.48	5.01	5.79	5.88	6.98	7.24	6.28
			(0.48)	(0.56)	(0.53)	(0.01)	(0.01)	(0.02)
$2\sigma_{\epsilon,\theta}$			-2.73	-1.18	-0.99			
			(1.49)	(1.60)	(1.63)			
llik			-155.90	-154.79	-154.15	-158.04	-159.75	-153.06
n	47	47	47	47	47	47	47	47

Table 10: Explaining taxes

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	0.18	0.22	0.02	0.01	-	0.11**	-0.01	0.09*
	(0.14)	(0.12)	(0.05)	(0.05)		(0.04)	(0.05)	(0.06)
income	0.00	-0.70	0.30	0.40		-0.23	0.37	0.65*
	(0.00)	(0.85)	(0.50)	(0.50)		(0.48)	(0.34)	(0.52)
urban	0.00	2.36	2.18	1.29		0.21	0.18	-3.27**
	(0.04)	(3.17)	(1.55)	(1.77)		(1.80)	(1.63)	(1.98)
libop	0.37**	0.37**	0.33**	0.33**		0.37**	0.35**	0.35**
	(0.15)	(0.15)	(0.06)	(0.06)		(0.06)	(0.05)	(0.06)
init	-3.37	-3.75						
	(2.81)	(2.77)						
init x libop	-0.16	-0.18						
	(0.17)	(0.17)						
constant		-7.31	-2.64	-2.61		-3.90	-0.08	-8.56**
		(8.59)	(5.62)	(5.50)		(3.69)	(2.00)	(4.30)
init			0.21	-1.14		0.17	-0.62**	-1.13**
			(0.65)	(1.47)		(0.21)	(0.29)	(0.60)
initp				0.19			0.10**	0.17**
				(0.19)			(0.02)	(0.06)
proflegi								0.26
								(0.84)
turnhous								-0.01*
								(0.01)
constant 2			3.44**	3.42**		1.09**	1.22**	1.26**
			(0.43)	(0.43)		(0.15)	(0.15)	(0.24)
σ_ϵ		4.10	1.61	1.62		2.28	2.15	2.12
			(0.18)	(0.19)		(0.00)	(0.01)	(0.01)
$2\sigma_{\epsilon,\theta}$			-0.94	-0.87				
			(0.60)	(0.61)				
llik			-103.24	-102.72		-105.35	-102.70	-101.97
n	47	47	47	47		47	47	47

Table 11: Explaining summary policy index (zpolicy)

	OLS		mle (linear outcome equation)			mle (exponential outcome equation)		
	lascher et al	replication	init	init initp	init initp controls	init	init initp	init initp controls
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
HS grad	0.04**	0.04**	0.02**		-0.01	0.02**	0.01**	0.05**
	(0.02)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)
income	0.00	0.64	0.10**		0.30**	0.14**	0.17**	0.06
	(0.00)	(0.10)	(0.10)		(0.10)	(0.05)	(0.06)	(0.08)
urban	0.00	0.32	0.27		-0.03	0.39**	0.06	-0.34
	(0.00)	(0.38)	(0.20)		(0.21)	(0.20)	(0.26)	(0.35)
libop	0.11**	0.11**	0.11**		0.09**	0.10**	0.11**	0.08**
	(0.02)	(0.02)	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)
init	-0.51	-0.51						
	(0.33)	(0.33)						
init x libop	-0.03	-0.03						
	(0.02)	(0.02)						
constant		-1.72	-0.68		-0.30	-1.61**	-0.95**	4.65
		(1.04)	(0.55)		(0.50)	(0.39)	(0.48)	(9.30)
init			0.07		-0.38**	0.19	-0.39	-0.02
			(0.08)		(0.15)	(0.18)	(0.38)	(0.05)
initp					0.06**		0.08*	0.01
					(0.02)		(0.04)	(0.01)
proflegi					-0.55**			-0.26
					(0.31)			(0.34)
turnhous					-0.00			0.00
					(0.00)			(0.00)
constant 2			0.39**		0.67**	-0.96**	-0.97**	2.03**
			(0.05)		(0.12)	(0.12)	(0.13)	(1.22)
σ_ϵ		0.49	0.26		0.27	0.25	0.25	0.40
			(0.02)		(0.02)	(0.00)	(0.00)	(0.00)
$2\sigma_{\epsilon,\theta}$			0.00		0.07			
			(0.07)		(0.06)			
llik			-2.70		1.79	-1.41	-1.35	-23.11
n	47	47	47		47	47	47	47

7 Conclusion

In the current discussion of the merits and drawbacks of direct legislation both in the states and around the world, misguided generalizations often lead to erroneous statements about the effect of direct legislation. Systematic studies of the effect of direct legislation on policy outcomes (e.g., Matsusaka 1995 and 2000, Gerber (1996 and 1999), Kirchgässner, Feld and Savioz 1999, Feld and Matsusaka 2000, Gerber and Hug 2001) come to more nuanced conclusions that journalistic assessments (e.g., Schrag 1998 and Broder 2000) or studies relying on case studies (e.g. Smith 1998).

But even among the more systematic studies it has often proved elusive to find demonstrable effects of direct legislation on policy outcomes of the type we would expect from theory. In this paper I argued that the reason for this elusive quest is largely attributable to faulty empirical models. Based on the basic implication of most theoretical models, I derived two empirical models which improve on the models currently used by researchers trying to show that direct legislation has policy consequences. More precisely, these models directly acknowledge the fact that the effect of direct legislation is contingent on the preferences of at least the voters. Taking this into consideration in the empirical models I employed in this paper, I was able to show that results obtained by Lascher, Hagen and Rochlin (1996) are problematic, and do not allow for the rejection of the theoretically derived hypothesis. More precisely, using their dataset and replicating their analysis both with their empirical model and the ones proposed here, I demonstrated that the improved empirical models yield results largely in synch with theory. In addition, both empirical models proposed here yield largely similar results, testifying to the robustness of the results discussed here.

This empirical investigation, however, is hardly sufficient to demonstrate the merits of the empirical models proposed here. Future research has to show to what degree the simplifying assumptions employed to derive the two empirical models affect the properties of the estimates obtained with their help. Only Monte-Carlo simulations, especially given the importance of small-sample properties, can yield answers to this important question. Future will tell.

8 Appendix

Table 12 reports the descriptive statistics of the variables employed in this paper.

Table 12: Descriptive statistics

variable		minimum	mean	maximum	std dev	N
INIT	direct legislation dummy	0	0.45	1	0.5	47
INITP	lowest signature requirement for initiative	0	3.28	15	4.12	47
libop	state ideology measure	-28	-14.6	-0.81	7.3	47
income	mean income 1980	6.68	9.01	11.54	1.17	47
HS grad	percent of high school graduates	53	66.4	80	7.32	47
urban	percent of urban policy	0	0.53	0.93	0.25	47
ADC80		87	241.36	400	83.45	47
CONSUME2		4	13.57	21	3.85	47
CRIMJUS2		-100	153.19	400	131.63	47
EXPPUPIL		168	237.85	376	47.99	47
ERABOLES		0	3.74	6	2.63	47
GAMBLING		0	257.45	600	171.62	47
MEDICAR2		100	125.94	159	17.7	47
LOWRY2		-13	-3.45	7	4.67	47
ZPOLICY		-1.55	0	2.13	1	47
TURNHOUS	turnouver rate in state house	2	21.7	55	12.88	47
PROFLEGI	professionalization of legislature	0.04	0.22	0.66	0.15	47

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