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How Important is the Credibility Problem in Politics? Evidence from State-Level Abortion Legislation.

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Abstract: This paper proposes a simple mechanism for evaluating the relevance of credibility problems in politics. If candidates are capable of making credible policy promises, we will not expect them to systematically adopt platforms that entail large probabilities of losing an election. This is because the adoption of very extreme platforms has the effect of shifting expected policies systematically away from their ideal points. For candidates who lack the capacity of making credible commitments, in contrast, policy platforms are simply a reflection of their preferences, which may well be very extreme. I show that this fact implies that when politicians are credible the correlation between candidates' preferences and expected policies will always be positive, whereas when they lack credibility the correlation can be negative. Empirical tests on a panel of US abortion preferences and legislation show that the correlation between the preferences of party constituents and enacted policies is consistently negative, a result that strongly suggests the existence of significant credibility problems.

1 Introduction

The traditional Downsian model of politics assumes that politicians can credibly commit to keep their policy promises once they reach office. The availability of this commitment technology allows them to manipulate policy proposals so

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as to garner the majority of votes. Political competition thus leads to convergence to the median voter's ideal point. Many refinements of this model have been proposed in the literature, particularly with the intention of reversing the problematic hypothesis of complete convergence in policy proposals implied by Downsian competition. It is only relatively recently, however, that the assumption of credibility has come under scrutiny in the formal literature with the contributions of Besley and Coate (1997) and Osborne and Slivinsky (1996). As shown by these authors, when we assume that politicians cannot make credible promises the Downsian equilibrium of platform convergence is not the only one that can arise. Indeed, a multiplicity of equilibria become feasible, some of which entail very extreme policies being proposed in equilibrium

A growing field of empirical research has attempted to analyze the relative importance of the credibility problem in politics. Two recent examples are Chattopadyhay and Duflo (2004) and Lee, Moretti and Butler (2004). The former use the randomized allocation of reservations for Village Council head positions in India to show that the allocation of public services changes when the village head position is reserved for a female. This suggests that the preferences of policymakers (in this case women) and not just those of voters are important for policy determination. The latter use outcomes from close elections to argue that exogenous changes in political strength do not have a discernible effect on policy outcomes, suggesting that voters simply elect among different policies but do not affect them.

This paper proposes an alternative simple way of discriminating between models that assume commitment and those that preclude it. The test is based on the following observation: the key distinction between politicians who can credibly commit to keep their promises and those who cannot is that the former can control the expectations of the policies that they will enact once they reach office. For politicians who have the capacity to commit, their policy promises *are* the expected policy conditional on their reaching office, while for those who lack it, promises lack much relevance and, if anything, are likely to be no more than reflections of their preferences. Suppose then that we can pinpoint a case in which policy positions appear to be set in a manner that is clearly inconsistent with optimizing behavior, in the sense that if candidates were to change them they could increase their expected utility. Then we would have reason to doubt that candidates have much control over these positions.

A simple example of a policy position that is inconsistent with optimizing behavior is a position that is so extreme that it ensures its candidate defeat at the polls. Adopting such a position brings no gains to the candidate, as it leads enacted policies to move away from his ideal point. If we see candidates systematically adopting such positions, this may be symptomatic of the fact that this is not a variable that they control.

Examples of candidates whose positions were too extreme for their own good abound. Take the example of George McGovern's 1972 candidacy in the US presidential elections against Richard Nixon. Most political analysts believed there was a reasonable chance that Nixon would have been defeated in the

1972 election had he run against a more moderate candidate. Nixon won the 1972 election with a remarkable landslide, carrying more than 60% of the vote and losing only Massachusetts and the District of Columbia. The generalized perception was that George McGovern's candidacy, which proposed a 37% reduction in defense spending in the midst of the Cold War (White, 1972, p. 123) was too radical to woo a majority of American voters. By adopting such a radical platform, the democrats ensured Nixon's victory and the continuation of conservative policies.

McGovern's example is not isolated. Barry Goldwater's 1964 promise to make social security voluntary and Walter Mondale's 1984 commitment to raise taxes are often seen as examples of misguided political choices that cost them the presidency. So was Mario Vargas Llosa's promise to carry out a shock-therapy stabilization programme in the 1990 Peruvian presidential election against Alberto Fujimori, or Carlos Menem's 2003 promise to dollarize the Argentine economy in his failed bid to recover the presidency after the collapse of the Argentine convertibility plan. All these examples beg the question of why the candidates did not choose to moderate their policy proposals. A conventional analysis of these examples would characterize them as gross miscalculations, based on mistaken beliefs about what voters' actual preferences really were. Under the alternative interpretation that we espouse, there is nothing irrational about these policy platforms. It wasn't George McGovern's policy platform that cost him the presidency; it was his preferences. The platform is nothing more than the description of the policies that both candidates and voters know will be carried out, and any attempt to lay out a platform that significantly differs from a candidates' preferences is unlikely to bring much gain. If politicians are unable to commit to keeping their policy promises, then we are likely to see them adopting extreme positions in equilibrium - even if they are too extreme for their own good - because those positions are nothing more than reflections of their own, possibly very extreme, preferences.

This paper proposes that we look for evidence of credibility problems by estimating the expected policy function: the function that describes enacted policies as a function of the policy that voters expect that candidate to carry out if elected. For candidates that have the capacity to precommit, the expected policy function should always be positively sloped at the equilibrium. This follows from the fact that whenever a politician decides to adopt a policy position, he is trading off two effects of moderation. On the one hand, adopting a more moderate stance may move expected policies away from his ideal point. This will occur if the change in policies when he is elected outweighs the lower probability of losing the election. On the other hand, moderating his position will certainly imply decreasing the distance between the enacted policy if he wins and the enacted policy if he loses, and thus entails undertaking a safer bet. In equilibrium, politicians will trade off these two effects until they find a policy proposal where the marginal loss from further moderation is exactly offset by the marginal gain in terms of reduced risk. It follows that credible politicians will therefore always situate themselves in a position in which further moderations would lead to movements in the expected policy away from their

ideal points. Politicians whose ideal point is below that of their opponents will be in a range where increases in heir policy proposals lead to increases in the expected policies; those that are above the ideal point of their opponents will be in a situation where decreases in their policy proposals lead to decreases in expected policies. Therefore the expected policy function must be positively sloped.

This does not happen if politicians lack credibility, because in this case they cannot affect their probability of winning by moderating. Voters know that they will carry out their preferred policies and will pay no attention to any policy commitments that candidates attempt to make. Therefore, it is perfectly possible for a candidate that lacks credibility to be situated in the downward-sloping segment of the expected policy function. It would be great for that politician if he were able to moderate his policy proposals: it would lead expected policies to come closer to his own preferred policy and would also reduce the risk coming from the uncertainty over the policies enacted by different candidates. But the candidate with a credibility problem may well be stuck in this range of the expected policy function because he has no capacity to affect the expected policy function: for voters his relevant platform comes not from what he says, but from what they know that he will do.

Figure 1 illustrates the intuition behind our approach. It graphs the expected policy function $E(\pi) = g(\pi_l^e, \overline{\pi}_r^e) = p(\pi_l^e, \overline{\pi}_r^e)\pi_l^e + (1 - p(\pi_l^e, \overline{\pi}_r^e))\pi_r^e$, where $p(\pi_l^e, \overline{\pi}_r^e)$ is the probability that the left-wing candidate wins the election given the policy that voters expect him to carry out if elected (π_l^e) and that which they expect his opponent to carry out if elected (π_r^e). In the case of politicians that can make credible commitments voters will believe their policy promises and will expect them to carry out their policy platform π_i for $\{i = l, r\}$. Generally, the expected policy function will have increasing and decreasing segments. Now suppose that l were a credible politician with an ideal policy π_l^* lower than that of the right-wing candidate (π_r^*). Since a credible politician can convince voters that he will implement any policy that he promises to pursue, then he can move at will to any point on $g(\pi_l^e, \overline{\pi}_r^e)$ simply by changing his platform. Suppose now that in equilibrium he is proposing a policy platform π_l^A which is on a decreasing segment of the expected policy function (i.e., where $\frac{\partial g(\pi_l^e, \overline{\pi}_r^e)}{\partial \pi_l^e} < 0$). It would then be better for l to slightly moderate his strategy, say to π_l^B. At the new strategy, the expected policy will be $g(\pi_l^B, \overline{\pi}_r^e)$, which is lower (and thus closer to his ideal point) than $g(\pi_l^A, \overline{\pi}_r^e)$. He will also face a less risky lottery than he was facing before between the outcome in which he wins the election and π_l^B gets implemented and that in which he loses and $\overline{\pi}_r^e$ gets implemented, since π_l^B is closer to $\overline{\pi}_r^e$ than π_l^A was. It thus follows that π_l^A is not an optimal policy for a risk-averse credible left-wing politician.

Matters are different for a politician that faces a credibility problem. Such a politician cannot affect expected policies by moderating his platform because voters are convinced that he will always carry out his optimal policy π_l^* if elected. Thus, he does not have the capacity to move along the expected policy function. In effect, voters will always expect non-credible politicians to carrry

out their prefered policy ($\pi_i^e = \pi_i^*$), so that they are in effect stuck at whatever segment on the expected policy function their ideal point falls on. It follows that it is perfectly feasible for non-credible politicians to be on the decreasing segment of the expected policy function. This contrast between the empirical implications of the models gives rise to our empirical strategy: to estimate the expected policy function and to idetify whether politicians tend to be located on the increasing or decreasing portions of this function.

Estimating the expected policy function would seem to be a daunting task. Even if one were able to assemble a meaningful data set of policy promises, these would not be very useful unless one were able to know how they affected voters ' expectations of policies conditional on particular candidates reaching office. I propose to get around this problem in two ways. First, I show that a credible candidate's policy proposal will always be an increasing function of her policy preference. That is, if a candidate becomes more conservative (liberal), in equilibrium her policy proposal will also become more conservative (liberal). This allows us to define an *indirect* expected policy function as one that maps politician preferences into enacted policies. For credible politicians, this will be qualitatively identical to the expected policy function, in the sense that decreasing (increasing) segments of the former will correspond to decreasing (increasing) segments of the latter, whereas for non-credible politiciansm the indirect expected policy function will be identical to the expected policy function since expected policies are equal to politician preferences. But the indirect expected policy function can be estimated by looking at the relationship between politician preferences and enacted policies. This strategy leads to another problem: how do we estimate politician preferences? I propose that we measure candidates' policy preferences (as opposed to their platforms or actions when elected) by looking at the preferences of the constituents that elect them to be candidates for office. For the case of the U.S., I thus propose that we use the median preferences of Republican and Democrat consituents as an indicator of candidates' policy preferences[1].

The rest of the paper is organized as follows. Section 2 lays out the basic theory. In that section I show that optimizing politicians will always manipulate their policies so that in equilibrium the marginal gain from the reduction in uncertainty that comes from moderation is offset by the marginal loss entailed by letting expected policies shift away from one's ideal point. I also show that in equilibrium policy proposals and policy preferences will move in the same direction, so that the correlation between policy preferences and expected policies is positive. In contrast, I show that the opposite can happen when politicians face credibility problems: their inability to control voters' expectations implies

[1] At first sight, this may appear inconsistent: wouldn't one expect there to be a credibility problem within parties also? Upon further reflection, however, one can see that even when there are credibility problems, expected policies within parties react positively to changes in the median party voter in the same way that the national expected policy function depends positively on the median voter's ideal policy: given any two candidates, a shift of constituent preferences in a given direction will lead to an increase in the probability that the candidate placed in that direction will win the nomination.

that they can easily be stuck in a situation in which there is a negative correlation between expected policies and policy preferences. Section 3 lays out the data that I use to implement the proposed empirical test. I use data on abortion preferences and legislation for a panel of 50 states and the District of Columbia, and show that in the great majority of estimates the data displays a negative correlation between the policy preferences of Democrat and Republican constituents and enacted policies, evidence that is inconsistent with the existence of a commitment technology. Section 4 concludes.

2 Theoretical Framework

Let $\pi \in [0,1]$ denote the harshness of legal restrictions on abortion. $\pi = 0$ denotes complete absence of legal restrictions on abortion, whereas $\pi = 1$ would imply a state of the world in which abortion is illegal under all circumstances. Let all individuals have Euclidean preferences that are described by:

$$V_i(a) = -(\pi_i^* - \pi)^2. \tag{1}$$

with $\pi_i \tilde{} f[\pi_i]$. I assume that there are two parties, l and r, with ideal points $\pi_l^* < \pi_r^*$. I also assume that preferences are distributed uniformly on the unit interval:

$$\begin{aligned} f(\pi_i) &= 1 \text{ for } \pi_1 \in [0,1] \\ &\quad\ 0 \text{ otherwise.} \end{aligned}$$

2.1 A model with commitment

If policymakers have a commitment technology that allows them to bind themselves to the promise of implementing their policy announcement and there is no uncertainty regarding the preferences of voters or the outcome of the election once these promises are made, then we are in the arena of Downsian politics and full convergence ensues. In this model, equilibrium policies are insensitive to candidates' preferences. Full convergence can be reversed by combining politicians that have preferences over outcomes as given by (1) and uncertainty about the distribution of voters. I analyze the effects of commitment under such a model.

Following Roemer (2001), I model uncertainty as to the distribution of voters as uncertainty by the politicians regarding the true fraction of individuals who prefer π_l to π_r. We assume such a fraction is distributed uniformly on the interval $\left\{F\left(\frac{\pi_l+\pi_r}{2}\right) - \beta, F\left(\frac{\pi_l+\pi_r}{2}\right) + \beta\right\} = \left\{\frac{\pi_l+\pi_r}{2} - \beta, \frac{\pi_l+\pi_r}{2} + \beta\right\}$. Thus the probability that l defeats r when they respectively promise π_l and π_r is:

$$\begin{aligned} P(\pi_l, \pi_r) &= 0 \text{ if } \frac{\pi_l + \pi_r}{2} - \beta < \frac{1}{2} \\ &= \frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta} \text{ if } \frac{\pi_l + \pi_r}{2} \in \left\{ \frac{1}{2} - \beta, \frac{1}{2} + \beta \right\} \\ &= 1 \text{ if } \frac{\pi_l + \pi_r}{2} + \beta > \frac{1}{2}. \end{aligned} \tag{2}$$

Parties will set policies to solve:

$$\underset{\pi_l}{Min} \left\{ \left(\frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_l^* - \pi_l)^2 + \left(\frac{1}{2} - \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_l^* - \pi_r)^2 \right\} \tag{3}$$

$$\underset{\pi_r}{Min} \left\{ \left(\frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_r^* - \pi_l)^2 + \left(\frac{1}{2} - \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_r^* - \pi_r)^2 \right\} \tag{4}$$

At an interior minimum, the first order conditions will give:

$$\begin{bmatrix} \left(\frac{1}{4\beta} \right) \left[(\pi_l^* - \pi_l)^2 - (\pi_l^* - \pi_r)^2 \right] \\ -2 \left(\frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_l^* - \pi_l) \end{bmatrix} = 0 \tag{5}$$

$$\begin{bmatrix} \left(\frac{1}{4\beta} \right) \left[(\pi_r^* - \pi_l)^2 - (\pi_r^* - \pi_r)^2 \right] \\ -2 \left(\frac{1}{2} - \frac{\pi_l + \pi_r - 1}{4\beta} \right) (\pi_r^* - \pi_r) \end{bmatrix} = 0. \tag{6}$$

(2) imply two additional inequality restrictions that must hold for an interior minimum

$$\frac{\pi_l + \pi_r}{2} > \frac{1}{2} - \beta \rightarrow \pi_l + \pi_r + 2\beta - 1 > 0 \tag{7}$$

$$\frac{\pi_l + \pi_r}{2} < \frac{1}{2} + \beta \rightarrow -\pi_l - \pi_r + 2\beta + 1 > 0 \tag{8}$$

The second order conditions give:

$$3\pi_l + \pi_r - 2\pi_l^* + 2\beta - 1 > 0 \tag{9}$$

$$2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1 > 0. \tag{10}$$

Modifying the first-order conditions gives us:

$$(\pi_l - \pi_r) \left[-2\pi_l^* + (\pi_l + \pi_r) \right] - 2 \left(2\beta + \pi_l + \pi_r - 1 \right) (\pi_l^* - \pi_l) = 0 \tag{11}$$

$$(\pi_l - \pi_r) \left[-2\pi_r^* + (\pi_l + \pi_r) \right] - 2 \left(2\beta - \pi_l - \pi_r + 1 \right) (\pi_r^* - \pi_r) = 0. \tag{12}$$

We now establish:

Lemma 1 *If π_l and π_r jointly solve (3) and (4), then $\pi_l^* \leq \pi_l < \pi_r \leq \pi_r^*$.*

Proof. First we establish that in equilibrium it must be the case that $\pi_l \geq \pi_l^*$. In order to see this, assume it were not the case, that is, assume that $\pi_l < \pi_l^*$. Then by (5):

$$\left(\frac{1}{4\beta}\right)\left[(\pi_l^* - \pi_l)^2 - (\pi_l^* - \pi_r)^2\right] = 2\left(\frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta}\right)(\pi_l^* - \pi_l) > 0$$

That is, it must be that

$$(\pi_l^* - \pi_l)^2 - (\pi_l^* - \pi_r)^2 > 0 \tag{13}$$

$$(\pi_l^* - \pi_l)^2 > (\pi_l^* - \pi_r)^2. \tag{14}$$

The loss that l experiences when it wins is greater than that experienced if it loses. But then from (3) l could improve its utility by setting $\pi_l = \pi_r$. It would then with certainty have loss $(\pi_l^* - \pi_r)^2$, which is less than the weighted average of $(\pi_l^* - \pi_r)^2$ and the, by (14), higher $(\pi_l^* - \pi_l)^2$. An analogous argument helps establish that $\pi_r \leq \pi_r^*$. Thus, politicians will propose more moderate policies than their preferred ones in equilibrium. Now we show that at an equilibrium it must be the case that $\pi_r > \pi_l$. Assume this were not the case. From (11):

$$[-2\pi_l^* + (\pi_l + \pi_r)] = \frac{2\,(2\beta + \pi_l + \pi_r - 1)\,(\pi_l^* - \pi_l)}{(\pi_l - \pi_r)} = 0.$$

We have just established that $\pi_l^* - \pi_l < 0$ and we know from (7) that $(2\beta + \pi_l + \pi_r - 1) > 0$. If $\pi_l - \pi_r > 0$, then

$$[-2\pi_l^* + (\pi_l + \pi_r)] < 0$$

$$(\pi_l - \pi_l^*) + (\pi_r - \pi_l^*) < 0.$$

As we have established above that $\pi_l - \pi_l^* \geq 0$, then it must be that $(\pi_r - \pi_l^*) < 0$, that is, that r has positioned himself to the left of l's ideal point. But then $(\pi_r^* - \pi_l)^2 < (\pi_r^* - \pi_r)^2$ and r would gain from setting π_r equal to π_l and ensuring with certainty a loss of $(\pi_r^* - \pi_l)^2$. Therefore it must be that $\pi_r > \pi_l$, completing our proof. ■

I now establish that the policy proposal π_l and the preferred policy π_l^* will always move in the same direction in equilibrium. To see this, take total derivatives letting $d\pi_r^* = 0$. This gives:

$$[-2\pi_l^* + 3\pi_l + \pi_r + 2\beta - 1]\,d\pi_l + (\pi_l - \pi_r)\,d\pi_r + [-2\pi_l - (2\beta - 1)]\,d\pi_l^* = 0 \tag{15}$$

$$(-2\pi_r^* + \pi_l + \pi_r)\,d\pi_l + [2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1]\,d\pi_r = 0 \tag{16}$$

so that:

$$d\pi_r = \frac{2\pi_r^* - \pi_l - \pi_r}{[2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1]} d\pi_l. \tag{17}$$

It is important to note for future reference that the coefficient of $d\pi_l$ in (17) is positive, as its numerator is positive from Lemma 1 and its denominator is positive from the second order condition (10), so that

$$\frac{d\pi_r}{d\pi_l}|_{d\pi_r^*=0} > 0. \tag{18}$$

Substituting in (15) gives us:

$$\frac{d\pi_l}{d\pi_l^*} = \frac{2\pi_l + (2\beta - 1)}{\begin{array}{c}[-2\pi_l^* + 3\pi_l + \pi_r + 2\beta - 1] \\ + (\pi_l - \pi_r) \frac{2\pi_r^* - \pi_l - \pi_r}{[2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1]}\end{array}}. \tag{19}$$

We now establish that this expression is positive. First we show that the denominator is positive. To see this, note that if we use (11) we can establish that:

$$2\pi_l + (2\beta - 1) = (\pi_l - \pi_r) \frac{1}{2} \left(1 - \frac{(\pi_r - \pi_l^*)}{(\pi_l - \pi_l^*)}\right)$$

By Lemma 1, $\pi_l - \pi_r < 0$ and $\frac{(\pi_r - \pi_l^*)}{(\pi_l - \pi_l^*)} > 1$ so that:

$$2\pi_l + (2\beta - 1) > 0. \tag{20}$$

Now we show that the denominator in (19) is positive. To see this, note that:

$$\begin{aligned} & [-2\pi_l^* + 3\pi_l + \pi_r + 2\beta - 1] + (\pi_l - \pi_r) \frac{2\pi_r^* - \pi_l - \pi_r}{[2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1]} \\ = \quad & -(\pi_l - \pi_r) \left(\frac{[-2\pi_l^* + 3\pi_l + \pi_r + 2\beta - 1]}{(\pi_r - \pi_l)} - \frac{2\pi_r^* - \pi_l - \pi_r}{[2\pi_r^* - 3\pi_r - \pi_l + 2\beta + 1]} \right). \end{aligned}$$

Decompose the expression in parenthesis as the difference between two parts, both of which are positive (by the second order conditions and Lemma 1). The first part is larger than one, as

$$2(\pi_l - \pi_l^*) + (2\pi_l + 2\beta - 1) > 0$$

by Lemma 1 and (20). The second part is less than one, as

$$0 < -2\pi_r + 2\beta + 1 = (\pi_r - \pi_l) \left(\frac{1}{2}\right) \left(-1 + \frac{\pi_r^* - \pi_l}{(\pi_r^* - \pi_r)}\right) \tag{21}$$

Again by Lemma 1, $\pi_l < \pi_r$ implies $\frac{\pi_r^* - \pi_l}{(\pi_r^* - \pi_r)} > 1$, so that $\pi_l - \pi_r + (2\beta + 1 - \pi_l - \pi_r) > 0$. Therefore both the numerator and the denominator in (19) are greater than zero, and:

$$\frac{d\pi_l}{d\pi_l^*} > 0. \tag{22}$$

Using (17), it then follows that:

$$\frac{d\pi_r}{d\pi_l^*} > 0. \tag{23}$$

.

A similar argument establishes that

$$\frac{d\pi_r}{d\pi_r^*} > 0. \tag{24}$$

I now calculate the expected policy function. Note that the direct expected policy function will be given by:

$$E(\pi) = \left(\frac{1}{2} + \frac{\pi_l + \pi_r - 1}{4\beta}\right)\pi_l + \left(\left(\frac{1}{2} - \frac{\pi_l + \pi_r - 1}{4\beta}\right)\right)\pi_r, \tag{25}$$

as the assumption of credibility ipmlies that $\pi_i^e = \pi_i$ for $i = l, r$. We can now use (23) and (24) to derive our comparative statics result of interest linking the indirect expected policy function and politician preferences. Taking the total derivative of $E(\pi)$ with respect to π_l^* gives:

$$\frac{dE(\pi)}{d\pi_l^*} = \left(\frac{1}{4\beta}\right)[2\beta - 1 + 2\pi_l]\frac{d\pi_l}{d\pi_l^*} + \left(\frac{1}{4\beta}\right)[2\beta + 1 - 2\pi_r]\frac{d\pi_r}{d\pi_l^*} \tag{26}$$

Using (22), (23), (20), and (21), we establish that

$$\frac{dE(\pi)}{d\pi_l^*} > 0.$$

2.2 A model without commitment

What changes when politicians cannot precommit to carrying out their preferred policy promises? The algebra for such a case is simple. The key distinction between the commitment and the no commitment setting is that under commitment policymakers will always end up implementing their preferred policy, so that $\pi_l = \pi_l^*$, $\pi_r = \pi_r^*$. The direct and indirect expected policy functions will now be identical:

$$
\begin{aligned}
E^c(\pi) &= \pi_r^* \text{ if } \pi_l^* > 1 - 2\beta - \pi_r^* && (27)\\
&= \left(\begin{array}{c} \left(\frac{1}{2} + \frac{\pi_l^* + \pi_r^* - 1}{4\beta}\right)\pi_l^* \\ + \left(\left(\frac{1}{2} - \frac{\pi_l^* + \pi_r^* - 1}{4\beta}\right)\right) \end{array} \right) \pi_r^* \text{if } \pi_l^* \in (1 - 2\beta - \pi_r^*, 2\beta + 1 - \pi_r^*) \\
&= \pi_l^* \text{ if } \pi_l^* > 2\beta + 1 - \pi_r^*
\end{aligned}
$$

and its reaction to changes in π_l^* will be:

$$
\begin{aligned}
\frac{dE^c(\pi)}{d\pi_l^*} &= 0 \text{ if } \pi_l^* > 1 - 2\beta - \pi_r^* && (28)\\
&= \left(\frac{1}{4\beta}\right)[2\beta - 1 + 2\pi_l^*] \text{ if } \pi_l^* \in (1 - 2\beta - \pi_r^*, 2\beta + 1 - \pi_r^*) && (29)\\
&= 1 \text{ if } \pi_l^* > 2\beta + 1 - \pi_r^* && (30)
\end{aligned}
$$

Note that when π_l^* falls in the middle range of this expression (Equation 29), in contrast to the commitment case, it cannot be ensured that $\frac{dE^c(\pi)}{d\pi_l^*}$ will be positive, as (20) will not necessarily apply. The reason is that (20) was derived in the context of an optimizing model by appealing to the problem's first order conditions and Lemma 1, neither of which will hold in this case. For example, if $\pi_l^* = 1 - 2\beta - \pi_r^*$, equation (29) will give

$$
\frac{dE^c(\pi)}{d\pi_l^*} = \left(\frac{1}{4\beta}\right)[-2\beta + 1 - 2\pi_r^*] \tag{31}
$$

,which will be negative for any configuration of preferences in which the right-wing candidate is to the right of the median voter ($\pi_r^* > 0.5$). $\frac{dE^c(\pi)}{d\pi_l^*}$ turns positive as π_l^*increases, and will be equal to one for any π_l^* at which (30) is valid.

Figures 2 and 3 plot the indirect expected policy function as a function of changes in π_l^* and π_r^*, holding each other fixed. The upshot of this figure is that, unlike the model with commitment, in which optimizing behavior ensured that $\frac{dE(\pi)}{d\pi_l^*} > 0$, in this case $\frac{dE^c(\pi)}{d\pi_l^*} < 0$ is a real possibility.

The interpretation for this result is straightforward. No optimizing left-party politician will ever locate herself in a range of policies in which $\frac{dE(\pi)}{d\pi_l^*} < 0$. The reason is that if she were to increase her policy proposal slightly, she would get two gains (i) The expected policy would decrease, bringing it closer to her preferred policy (ii) she would now have a less risky bet because the two possible outcomes (π_l and π_r) would now be closer together. However, doing this presumes that the politician can alter her proposed policy. When there is a commitment problem, so that the politician is not able to choose her policy proposal (in the sense that voters will always understand that he will carry out π_l^*), then she may well be caught in such a region. She would obviously like to be able to moderate the policy that voters believe she will carry out, but the credibility problem precludes her from doing so.

3 Empirical Evidence

The model above has shown that, in contrast to models with pre-commitment, models in which there is a credibility problem tend to support the prediction that enacted policies and candidates ' preferred outcomes can move opposite directions. We now evaluate these predictions using data on preferences over abortion and abortion legislation decisions in a panel of U.S. states. (27) and (25) define expected policies as a function of π_l^*,π_r^* and β. Furthermore, both of these equations define $E(\pi)$, whereas what we observe is the realization of π:

$$\pi = E(\pi) + \eta_i = f(\pi_l^*, \pi_r^*, \beta) + \eta_i.$$

η_i refers to the effect on the expected value of π of the realization of the uncertainty regarding $p(\pi_l, \pi_r)$. Note that the uncertainty captured in (2) is in essence an uncertainty regarding the true location of the median voter. As the preferences of the median voter may not be orthogonal to the ideal points of politicians, it will be important to control for this term. Thus we decompose η_i into the part of it that depends on the preferences of the median voter and those that reflect the uncertainty arising from the impreciseness of the model:

$$\eta_{it} = \gamma \pi_{mdit} + \varepsilon_{it}$$

Adopting a linear form gives us our estimating equation:

$$\pi_{it} = \alpha_0 + \alpha_1 \pi_{rit}^* + \alpha_2 \pi_{lit}^* + \alpha_3 \beta + \gamma \pi_{mdit} + \varepsilon_{it}. \tag{32}$$

Equation (32) thus allows us to nest the predictions from the commitment and the credibility models. In the model with commitment, $\alpha_1 > 0$ and $\alpha_2 > 0$. In the credibility model, no definitive prediction is made about the sign of α_1 and α_2. Nevertheless, we have shown that $\alpha_1 < 0$ and $\alpha_2 < 0$ are distinct possibilities in such a model. It follows that if we estimate α_1 and α_2 to be positive, this will not allow us to distinguish between models, but if we estimate them to be negative, it will be strong evidence against the commitment model. γ is premised to be positive in both models: Given the proposed policies, an increase in the preferences of the median voter leads to an increase in the probability that the highest proposed policy wins[2].

Ideally, an empirical test of the theories sketched would study the relationship between the preferences of politicians and policy outcomes. If we were to be able to precisely measure the preferences of politicians, then a clean test of whether there are credibility problems would come from the simple comparison between enacted policies and their policy preferences. The proposal of

[2] In the simple model that we have sketched, there is no ex-ante variation in the preferences of the median voter. However, if we add this level of complexity to both models it is easy to show that optimizing politicians will adjust their policies in the direction of the median policy (Roemer, 2001). In the credibility model, this will not happen (since politicians cannot move their policies), but the probability that the candidate with higher π wins is increased when π_{md} increases.

this paper is to instead use data on the preferences of constituents. Since we will concentrate on the comparative statics results, the key assumption that is required for this approach to be valid is that politicians ' preferences move in the same direction as those of their constituents. These preferences will be correlated with those of candidates regardless of the importance of the credibility problem within the party: more right-wing constituents, for example, are likely to elect more right-wing candidates in their primaries.

Our key tests will be focused on estimating an equation like (32). However, such an equation may not provide a very strong test against alternative theories of politics according to which other characteristics of the distribution of preferences affect policy outcomes. To take an example, theories of interest groups (Grossman and Helpman, 2002) tend to emphasize that some individuals (those who belong to better-organized groups) have a greater capacity to affect policies than others. In order to provide an empirical test that is strong against such type of competing hypotheses, I augment the regression in (32) as

$$\pi_{it} = \alpha_0 + \alpha_1 \pi^*_{rit} + \alpha_2 \pi^*_{lit} + \alpha_3 \beta + \alpha_4 \theta_i + ... + \alpha_{3+n} \theta_n + \gamma \pi_{mdit} + \varepsilon_{it}. \quad (33)$$

where $\theta_1 ... \theta_n$ are alternative characteristics of the distribution of voter preferences. Equation (33) embodies the idea that any theory of politics is in effect a theory of the relationship between the distribution of policy preferences and policy outcomes. It also suggests a simple straightforward misspecification test for evaluating models such as those of sections 2.1 and 2.2. If those models capture adequately the policy-generating process, then we should be able to verify that $\alpha_4 = ... = \alpha_{3+n} = 0$.

3.1 The Data

Our paper will test the above derived hypotheses on a panel covering data on abortion legislation and preferences for the 50 United States. Although the model can in principle be applied to the study of the determination of any policy, I concentrate on abortion for several reasons. In the first place, the extensive interest on the abortion issue in the United States means that - in contrast to other issues - there is broad availability of data on abortion preferences that is sufficiently detailed so as to allow us to implement our methodology. Furthermore, the public opinion literature has found that abortion preferences are somewhat less susceptible to framing effects than many other public policies. Even though, as is generally the case in public opinion studies, the answers given to survey questions tend to be sensitive to changes in question wording, studies have found that respondents can accurately place themselves on abortion using a variety of question formats, and that the operationalization of attitudes towards abortion generally seems robust across different measurement strategies (Cook, et. al. 1993, Jelen and Wilcox, 2003). A last point has to do with the availability of significant variation across states and time in the adoption of legislative restrictions to abortion.

The data on abortion preferences comes from the General Social Survey (GSS) and the National Election Studies (NES). Both of these surveys have been measuring attitudes towards abortion in questions that allow us to build a consistent time series since the end of the seventies. The National Election Studies have been posing the following question in every survey since 1980:

I am going to read you a short list of opinions. Please tell me which one of the opinions best agrees with your view. (1) By law, abortion should never be permitted, (2) The law should permit abortion only in case of rape, incest, or when the woman's life is in danger, (3) The law should permit abortion for reasons other than rape, incest, or danger to the woman's life, but only after the need for the abortion has been clearly established. (4) By law, a woman should always be able to obtain an abortion.

The General Social Survey's question is the following:

Please tell me whether or not you think it should be possible for a pregnant woman to obtain an abortion... (1) if there is a strong chance of serious defect in the baby? (2) if she is married and does not want any more children? (3) if the woman's own health is seriously endangered by the pregnancy? (4)if the family has a very low income and cannot afford any more children? (5) if she became pregnant as a result of rape? (6) if she is not married and does not want to marry the man? (7) if the woman wants it for any reason?

There is an obvious difference between the NES question and the GSS question. While in the NES case we have respondents' answers to one question, which is logically defined to progress from less inclusive to more inclusive definitions of permissible abortion, the GSS survey gives us the yes-no responses to seven different questions, without making it evident how to combine them into a single scale. One possibility, which we attempt below, is to follow conventional practice in the public opinion literature and build a 0-7 scale which captures the number of times that respondents answered each of these questions in the affirmative. However, this may be putting together policy preferences related to different dimensions of the abortion issue. We explore other alternative definitions below.

Our interest is in how these preference indicators correlate with enacted abortion legislation across time in the 50 United States. In order to implement this, I construct indicators of the distribution of preferences over abortion policies for each state-year. Given each respondents' answer to the NES and GSS questions, I calculate the median preference over abortion restrictions (π^*_{mdit}), the median preference of voters who self-identify as republicans (π^*_{rit}) and of those who self-identify as democrats (π^*_{dit}). I also construct several indicators of the distribution of preferences over abortion legislation $\theta_1 ... \theta_n$, including the mean, standard deviation, skewness and kurtosis of this distribution as well as the average preference conditional upon race, college education and having a high level of income. I calculate 378 state-year observations based on 18409 individual-level survey responses for the NES data and 575 state-year observations based on 18976 individual-level survey responses for the GSS data.

The use of these measures to proxy for the distribution of preferences within

states at any given year may raise questions about reliability. The NES and GSS samples are designed to be representative at a national level and not at the state level. Among other things, this means that survey weights are calculated so as to replicate national, not state-level population characteristics. For example, it is not atypical for very small states like Rhode Island or Delaware to have as few as two observations in given years whereas large states like California and New York commonly have around 200 observations per year. The signal-to-noise ratio for small states is likely to be low, and may generate substantial attenuation bias.

Commonly, the optimal (and often unavailable) solution to ascertain the reliability of a given survey-based measure is to correlate it with a measure obtained from another survey. While this solution is commonly unavailable at the individual level (as it would require independent surveying of the same individuals), it is available to us by comparing the correlation between survey responses to the GSS and NES questions. This method also allows us to gauge the effect of different question wordings and sampling decisions on our summary state-level indicators. If these survey characteristics had a significant effect on our indicators, we would expect the correlation between the GSS and NES data to be low, while if they are not very relevant we would expect to derive a high correlation.

Table 1 shows the correlations between different characteristics of the state-level preference distribution for the GSS and NES data. The correlation between the mean abortion preference between both indicators is .413. This low correlation indicates that there appears to be considerable measurement error for our complete sample. This problem is exacerbated as one goes to the higher moments of the distribution, which typically require more observations to attain a given level of confidence. However, Table 1 also shows that a considerable part of this low correlation can be attributed to the effect of states with a small number of observations by showing how this correlation evolves when we impose the requirement that a given state must have less than a minimum threshold of observations. In that case, we find that the correlations improve significantly: if the lower threshold is 50 observations, our correlation between means from the GSS and NES indicators increases to .649, while if it is 90 observations, the correlation increases to .8045. These calculations suggest that a substantial fraction of the measurement error in this variable comes from the effect of very small states. The lower panel of Table 1 also puts this problem in perspective by calculating the average widths of 95% confidence intervals that could be built around each state-level estimator of a distributional characteristic. In order to interpret these numbers, note that both the GSS and NES indices are scaled between 0 and 1. The average half-width of a confidence interval for the mean abortion preference in the whole sample is +/- 0.097, whereas that for the sample restricted to $n \geq 50$ would be +/-.058. While the width of these intervals is far from negligible, it does suggest that the data has the ability to distinguish between conservative, moderate and liberal states.

As an indicator of enacted policy, I will use variations in abortion legislation across states. In its hallmark 1972 *Roe v. Wade* decision, the Supreme Court

left open substantial leeway for states to regulate abortion during the second two trimesters of pregnancy. While overturning existing abortion bans, the Supreme Court argued that after the first trimester, states could regulate abortion in ways which are reasonably related to maternal health with the purpose of protecting the health of the mother, while in the third trimester it could regulate and even prohibit abortion, except when it was necessary to protect the health of the mother. In 1992, *Casey v. Planned Parenthood of Southeastern Pennsylvania* opened the door for legislation which did not impose an "undue burden " on pregnant women seeking abortions, such as informed consent laws. A number of states also passed partial birth abortion bans until these were struck down by the 2000 *Stenberg v. Carhart* decision.[3] These decisions generated broad variations in abortion legislation across states. Our dependent variable will consist of four indicators of the restrictiveness of abortion legislation at the state level, taken from New (2004): (i) whether the state had a parental involvement law requiring notification or consent of parents for performing the abortion procedure (ii) whether the state had an informed consent law requiring abortion providers to inform pregnant women about potential health risks, the development of their unborn children, and resources available to them in case they decided not to perform the procedure (iii) whether the state adopted a partial birth abortion ban, restricting the use of the Intact Dilation and Extraction abortion procedure that is commonly only used during late-second trimester or early third trimester pregnancies, and (iv) whether the state restricts the use of Medicaid funds to fund therapeutic abortions.

I experiment with two different ways in which to combine these four different indicators of abortion legislation into an outcome variable. The simplest approach is to generate an indicator of the number of abortion restrictions (out of these four) that the state imposed on abortion in a given year. Since this index can be interpreted either as a cardinal or as an ordinal indicator of abortion preferences, I use both panel Poisson and Ordered Probit regressions to study its relationship with abortion preferences. Such an indicator, however, would incorrectly assume that each of the four possible restrictions have the same effect on induced abortions. Since this assumption may no be very appealing, I construct an additional indicator which measures the estimated reduction in abortions that can be attributed to each state's combination of abortion policies. Michael New (2004) has estimated the effect of these four pieces of abortion legislation on the abortion rate, and found that all four have the expected negative coefficients, with informed consent laws and Medicaid funding restrictions having a consistently significant negative effect, while partial birth and parental involvement laws have a negative yet not always significant effect. I use the expected reduction in the CDC abortion rate generated by each state's combination of abortion legislation measures as an indicator of the restrictiveness of abortion legislation. Since this variable is censored at zero, I use panel Tobit regressions to study its relation with abortion preferences.

[3] In November, 2003, President Bush signed a National Partial Birth Abortion Ban. This statute was declared unconstitutional by federal judges in, California, New York, and Nebraska, and is currently before the US Supreme Court.

3.2 Results

Table 2 presents the results of baseline regressions with the NES indicator. The first two sets of columns use the number of legislative restrictions on abortion as the dependent variable, which takes a value between 0 and 4 according to whether the state had adopted any of the four restrictions discussed above (informed consent, parental consent, partial birth abortion bans and restrictions on Medicaid funds). Since this indicator could be interpreted either as a cardinal or as an ordinal measure of restrictiveness of abortion legislation, I report both population-averaged panel poisson regressions and random-effects ordered probit regressions. The last set of columns uses the predicted effect of enacted legislation on the CDC abortion rate estimated by New (2004). This is a continuous variable that is censored at zero (the lowest reduction that a state could achieve if it imposed no restrictions) so that I use a random effects Tobit specification. For each methodology, four different specifications are reported: a baseline specification that controls for the moments of the preference distribution, a second one that adds a control for the possibility that more educated voters have a greater impact on policies, a third one that adds controls for a set of environmental variables that may affect the disposition of governments to adopt abortion restrictions, and a last specification dropping the preference distribution controls. As one can see from the results reported in this table, the evidence suggests that there is indeed a negative correlation between constituents ' preferences and enacted legislation, as suggested by the credibility model and in contrast to the predictions of the commitment model. All twelve regression specifications deliver negative point estimates of the effects of republicans ' and democrats ' preferences on enacted legislation. The effect is statistically strongest in the case of republicans ' preferences, where 8 of the 12 coefficients are significant at the 1% level, another two at the 5% level and the remaining two at the 10% level. In contrast, only two of the coefficient estimates for democrats ' preferences are significantly negative at the 5% level, while 5 are significant at 10% and the remaining 5 are not significant. The median population preference has a positive sign, as indicated by theory, albeit it is only significant in 9 of the 12 specifications.

As I have discussed above, there may be considerable measurement error arising from the fact that some of our state-level indicators are built with a small number of observations. Table 3 addresses this issue by repeating the same set of regressions as in Table 2, but now limited to state-years with at least 50 observations. This restricts the sample considerably, to 137 observations in 27 states. The absolute values of the coefficient estimates now increase across the board, as would be expected if measurement error were generating attenuation bias. The statistical significance of the democrats ' preference variable is now considerably higher, with all twelve coefficients significant at 5% (11 at 1%). That of republicans' preferences is somewhat weaker, but still considerably strong: two of the 12 specifications return significant coefficients at 1%, an additional 7 at 5% and 1 at 10%. The median voters' preference still

enters with a positive significant sign in ten of the twelve specifications.

Table 4 studies the results of running the same regressions using the General Social Survey based preference variables as our right-hand side variable. Table 4 uses the sum of responses to the seven GSS questions as an indicator of aversion to abortion - a principal components index (not reported) delivered similar results. In the case of the GSS, we can again see that the party constituents ' preferences display a negative sign in the regression estimates. There is a significant difference in patterns observed between the results for democrats, which are uniformly statistically significant (7 cases at 1%, 5 at 5%) and that of the median preference of republicans, whose coefficient is not significant in nay of the specifications (although it is always negative). In this case again, as predicted by theory, the coefficient on the median individual 's preference is always positive, and significant half of the times.

One of the problems in using the GSS indicators that it may indeed be combining two very different dimensions of attitudes towards abortion that may not have the same relevance. On the one hand, some of the questions in the GSS survey pertain to abortion choice in extreme cases - those of rape, chance of serious birth defects and risk health. Another set of questions, in contrast, appear to be more related to choice under less extreme circumstances, associated with the desire to not have additional children or the lack of economic means to sustain them. These two sets of questions may be associated with two different types of attitudes towards abortion. Indeed, a principal components analysis of individual-level responses to these five questions produces two factors with eigenvalues greater than 1, with the second factor putting only positive weight on the "extreme case " responses (Table 5).

Using the results from this analysis, I calculate two indicators using GSS responses to measure these different dimensions of preferences over abortion. On the one hand, I use responses to questions 1 (birth defects) 3 (health danger) and 5 (rape) to build an indicator of preferences over abortion in "imposed needs " cases which reflect situations which are clearly out of the control of the mother. On the other hand, I use responses to questions 2 (no more children) 4 (low income) 6 (doesn't want to marry) and 7 (any situation) to construct an indicator of preferences over abortion in "preference based " cases which reflect choices that are not made under extreme circumstances. Tables 6 and 7 display the results of our regressions for these two indices. For the case of imposed-needs preferences the results are very similar to those in Table 4: all of the coefficients on democrats ' preferences are negative and significant, while none of those on republicans' preferences are significant (The case of republicans ' preferences in Table 6 is the only case that we have found in which some of the point estimates actually turn positive). However, in the case of the preference-based index reported in Table 7, the results differ: the coefficients on republicans 'preferences now become significant in 8 out of the 12 cases, while those on democrats' preferences remain significant in all specifications. An interpretation of these results is that, given that courts have upheld the right of mothers to abortions in the case of most extreme circumstances, state-level

governments only have open to them the possibility of legislating over abortion regulations that will have an effect on abortion decisions that are not made under extreme circumstances. Preferences over abortion rights in extreme cases may not have much of an effect on the political determination of abortion legislation that does not affect these cases.

I now turn to the issue of possible misspecification bias arising from the possible endogeneity of our right-hand side variables. As our right-hand side variables pertain to indicators of the distribution of preferences across state residents, any argument of endogeneity would have to start out from the assumption that policies can either affect preferences or be affected by a third variable that also determines preferences. One reason why this may be the case may have to do with the role of "leadership" in galvanizing support for or against policies. The adoption of policies may make it easier to mobilize support against them, particularly amongst politically mobilized individuals that may become inflamed when certain policies are adopted. This phenomenon could account for a negative relationship between policies and the preferences of party members.

My proposed instruments are taken from the literature on the determination of abortion preferences (Jelen and Wilcox, 2003) and refer to variables that are unlikely to be affected by enacted policies and that nevertheless have been found to have a significant effect on abortion preferences. In particular, I choose indicators of church attendance, religious orientation and age composition of republicans and democrats as our instruments. The key identification assumption should be understood as the assumption that if the average church attendance (religious orientation, age composition) of republicans increases, holding fixed the preferences over abortion of the population as a whole, then the only effect that this will have on equilibrium policies will operate through the changes that it will effect on the preferences of republicans over abortion. Table 8 shows the results of applying the Rivers-Vuong (1988) endogeneity tests with these three instruments on our specification. I report two specifications, one with controls over population characteristics and environmental variables in the second stage regression, and another one without them. The tests fail to find evidence of endogeneity for the case of democrats ' preferences, but do find some weak evidence of endogeneity for the case of republicans' preferences. Table 9 presents our second stage estimates. All of the coefficients on republicans' and democrats' preferences retain the negative sign. The coefficient remains significant in four of the 9 cases reported in this table; in all of them the IV estimate increases the absolute value of the point estimate.

4 Concluding Comments

I have argued that the cross-state abortion legislation data displays a pattern that is consistent with the existence of credibility problems that preclude politicians from making binding policy promises. Politicians with the ability to pre-

commit will never be on the downward sloping portion of the expected policy function, in which a moderation of their policy promise would move expected policies in their direction. Placing yourself in such a situation would be inconsistent with optimizing behavior, as utility could be easily increased by moderating, thus moving expected policies in your direction, while at the same time generating a safer lottery among policy alternatives. Politicians that lack the ability to credibly commit to keeping their promises, in contrast, may well find themselves on this downward sloping segment. Estimating the shape of the expected policy function that links enacted policies with the policy preferences of candidates gives us a way to empirically evaluate the relevance of the credibility problem in politics.

In this paper, I have provided such a test using data on abortion legislation and preferences for a panel of US states. I find that the expected policy function appears on average to be negatively sloped. Out of 120 coefficient estimates for this slope presented in this paper, 114 were negative and 85 were significantly negative. This evidence thus appears to be strongly inconsistent with the hypothesis of the commitment model that proposes a positively sloped expected policy function.

Why this function is pervasively negatively sloped is an interesting fact in itself. The credibility model does not predict that this function must be negatively sloped: it only shows that it **can** have this slope. Recall from section 2 that the negative slope of the expected policy function corresponds to the cases in which politicians' preferences are too extreme for voters. If candidates happen to be moderate, they will fall in the same range of the expected policy function in which credible politicians are able to position themselves. The fact that our coefficient estimates tend to systematically be negative is indicative that in equilibrium, extreme politicians not only exist but appear to be pervasive. To understand why this is the case, one would wish to be able to study the interaction between the shape of this policy function and the process of candidate selection. Regrettably, existing models that study the entry decision together with the existence of credibility problems (Besley and Coate, 1997, Osborne and Sivlinsky, 1996) have only dealt with the case of certainty and therefore produce degenerate policy functions which are not easily amenable to empirical analysis. Further elaboration of these interactions is a promising area of future research.

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Table 1: Measures of Reliability

	Minimum number of observations per state-year						
Correlates	0	10	30	50	70	90	100
Correlations between GSS and NES state-year indicators							
Mean	0.413	0.470	0.554	0.649	0.687	0.8045	0.8147
Standard Deviation	0.050	0.029	0.066	0.192	0.148	0.6037	0.6436
Skewness	0.400	0.457	0.536	0.671	0.667	0.7664	0.7693
Kurtosis	0.078	0.016	0.115	0.568	0.599	0.7186	0.7325
Widths of confidence interval (NES)							
Mean	+/-0.097	+/-0.082	+/-0.069	+/-0.058	+/-0.051	+/-0.046	+/-0.044
Standard Deviation	+/-0.062	+/-0.058	+/-0.049	+/-0.041	+/-0.036	+/-0.033	+/-0.031
Skewness	+/-0.948	+/-0.756	+/-0.640	+/-0.533	+/-0.468	+/-0.424	+/-0.402
Kurtosis	+/-1.897	+/-1.512	+/-1.281	+/-1.067	+/-0.937	+/-0.848	+/-0.805

Table 2: Complete Sample Regressions, National Election Studies

	Population Averaged Poisson				Random Effects Ordered Probit				Random Effects Tobit			
Specification	1	2	3	4	1	2	3	4	1	2	3	4
Dependent Variable	Number of legislative restrictions (0-4)				Number of legislative restrictions (0-4)				Abortion-reducing effect of legal restrictions			
Median abort	0.276	0.274	0.301	0.351**	2.351**	2.331**	2.128**	1.939***	0.694*	0.708*	1.310***	1.077***
	(0.201)	(0.669)	(0.195)	(0.138)	(0.967)	(4.249)	(0.957)	(0.690)	(0.411)	(1.499)	(0.380)	(0.286)
Median abort republicans	-0.270***	-0.265***	-0.287***	-0.253***	-1.020*	-0.961*	-1.236**	-0.990**	-0.633***	-0.621***	-0.593***	-0.653***
	(0.089)	0.088	(0.086)	(0.075)	(0.531)	(0.086)	(0.527)	(0.489)	(0.217)	0.198	(0.212)	(0.191)
Median abort democrats	-0.179*	-0.191*	-0.187*	-0.158	-0.907	-0.941	-0.414	-0.896	-0.541*	-0.584*	-0.500**	-0.565**
	(0.108)	(0.026)	(0.105)	(0.098)	(0.598)	(0.239)	(0.582)	(0.561)	(0.320)	(0.017)	(0.243)	(0.228)
Mean abort	0.952	1.093	0.933		1.527	2.125	4.428		0.782	1.114	1.008	
	(0.691)	(2.500)	(0.686)		(2.973)	(8.414)	(2.789)		(1.184)	(3.622)	(1.203)	
Standard deviation abort	-0.213	-0.140	-0.184		-7.329*	-6.896	-4.295		-1.101	-0.850	-1.616	
	(0.908)	(1.698)	(0.916)		(4.436)	(2.028)	(4.225)		(1.770)	(2.683)	(1.768)	
Skewness abort	0.128**	0.137**	0.126**		0.550	0.544	0.451		0.189	0.183	0.280*	
	(0.064)	(0.263)	(0.063)		(0.406)	(1.347)	(0.390)		(0.161)	(0.502)	(0.155)	
Kurtosis abort	0.037	0.040	0.020		0.472*	0.469*	0.195		0.068	0.063	0.104	
	(0.039)	(0.118)	(0.040)		(0.252)	(0.975)	(0.253)		(0.099)	(0.261)	(0.097)	
Interquartile	-0.085	-0.066	-0.128		1.201	1.199	0.472		-0.075	-0.086	-0.011	-0.354
	(0.168)	(0.278)	(0.170)		(1.009)	(3.243)	(1.002)		(0.412)	(0.741)	(0.391)	(0.277)
Median College Educated		-0.047				-0.416				-0.250		
		(0.145)				(0.582)				(0.172)		
Proportion of females 15-44			-2.821	-3.342			11.582**	4.287			7.791***	3.692
			(2.253)	(2.386)			(5.066)	(5.154)			(2.028)	(2.528)
Log of Per Capita Real Gross State Product			0.408*	0.378			1.470**	0.479			2.438***	2.497***
			(0.236)	(0.242)			(0.699)	(0.744)			(0.280)	(0.278)
Log of Intergovernmental Aid			0.154*	0.165*			0.707**	0.846**			0.302**	0.522***
			(0.091)	(0.094)			(0.330)	(0.331)			(0.133)	(0.130)
N. Observations	323	321	323	324	323	321	323	324	323	321	323	324
N. Groups	42	42	42	43	42	42	42	43	42	42	42	43
Wald / LR test	101.26***	95.17***	123.05***	80.87***	234.59***	232.37***	236.74***	232.64***	190.73***	192.46***	418.56***	361.99***

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively.

Table 3: Restricted Sample Regressions, National Election Studies												
	Population Averaged Poisson				Random Effects Ordered Probit				Random Effects Tobit			
Specification	1	2	3	4	1	2	3	4	1	2	3	4
Dependent Variable	Number of legislative restrictions (0-4)				Number of legislative restrictions (0-4)				Abortion-reducing effect of legal restrictions			
	0.641***	0.639***	0.655***	0.880***	4.790**	4.832**	3.041	6.251***	1.097*	1.084	1.527**	2.093***
Median abort	(0.219)	(0.220)	(0.222)	(0.167)	(2.097)	(2.059)	(1.968)	(1.609)	(0.662)	(0.661)	(0.616)	(0.450)
Median abort	-0.344**	-0.351**	-0.358**	-0.277**	-3.457***	-3.548***	-1.323	-2.537**	-0.898**	-0.914**	-0.642*	-0.388
republicans	(0.155)	(0.155)	(0.155)	(0.130)	(1.329)	(1.306)	(1.213)	(1.157)	(0.386)	(0.384)	(0.348)	(0.313)
Median abort	-0.490***	-0.506***	-0.498***	-0.407***	-3.812***	-4.001***	-2.586**	-3.623***	-1.161***	-1.206***	-1.194***	-1.065***
democrats	(0.128)	(0.132)	(0.131)	(0.105)	(1.411)	(1.422)	(1.257)	(1.179)	(0.437)	(0.444)	(0.392)	(0.359)
	0.003	0.175	-0.079		11.069	12.177	13.277		4.345	4.899	4.014	
Mean abort	(2.258)	(2.414)	(2.221)		(18.942)	(18.913)	(18.318)		(6.467)	(6.645)	(5.718)	
Standard	-1.658	-1.525	-1.233		-0.575	0.252	16.316		-1.053	-0.598	0.453	
deviation abort	(2.306)	(2.400)	(2.400)		(25.797)	(25.600)	(23.832)		(8.401)	(8.570)	(7.358)	
	-0.190	-0.179	-0.209		0.789	0.759	1.105		0.095	0.132	0.342	
Skewness abort	(0.405)	(0.410)	(0.380)		(3.241)	(3.236)	(3.128)		(1.156)	(1.183)	(1.002)	
	0.050	0.056	0.025		1.140	1.185	2.479*		0.424	0.451	0.446	
Kurtosis abort	(0.164)	(0.170)	(0.170)		(1.465)	(1.454)	(1.410)		(0.516)	(0.536)	(0.423)	
Interquartile	0.096	0.110	0.063		1.647	1.891	1.222		0.554	0.595	0.575	0.292
range	(0.196)	(0.201)	(0.180)		(2.282)	(2.309)	(2.283)		(0.650)	(0.659)	(0.606)	(0.551)
Median College		-0.052				-0.608				-0.157		
Educated		(0.118)				(1.042)				(0.357)		
Proportion of			2.711	2.805			10.012	-8.296			27.300***	27.953***
females 15-44			(1.868)	(1.979)			(10.302)	(10.534)			(3.332)	(3.335)
Log of Per Capita			-0.150	-0.078			0.563	3.771*			0.336	0.497
Real Gross State			(0.315)	(0.348)			(2.237)	(2.272)			(0.600)	(0.608)
Log of			0.240	0.274			1.593**	2.244***			0.796***	0.875***
Intergovernmental			(0.189)	(0.181)			(0.770)	(0.810)			(0.202)	(0.204)
N. Observations	137.000	137.000	137.000	137.000	137.000	137.000	137.000	137.000	137.000	137.000	137.000	137.000
N. Groups	27.000	27.000	27.000	27.000					27.000	27.000	27.000	27.000
Wald / LR test	961.97***	2218.81***	602.53***	241.16***	106.68***	107.02***	106.48***	107.31***	102.81***	103.57***	339.44***	320***

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively.

Table 4: Restricted Sample Regressions, General Social Survey												
	Population Averaged Poisson				Random Effects Ordered Probit				Random Effects Tobit			
Specification	1	2	3	4	1	2	3	4	1	2	3	4
Dependent Variable	Number of legislative restrictions (0-4)				Number of legislative restrictions (0-4)				Abortion-reducing effect of legal restrictions			
	1.007	0.931	1.376**	2.052***	6.997*	4.322	8.129**	10.26***	1.967	1.681	3.083	4.927***
Median abort	(0.656)	(0.665)	(0.647)	(0.657)	(3.848)	(4.142)	(3.850)	(2.700)	(1.962)	(1.940)	(2.025)	(1.535)
Median abort	-0.438	-0.427	-0.572	-0.426	-2.513	-0.593	-1.985	-1.560	-0.875	-0.862	-1.422	-1.013
republicans	(0.317)	(0.329)	(0.360)	(0.348)	(1.843)	(1.988)	(1.855)	(1.730)	(0.974)	(0.957)	(1.026)	(0.985)
Median abort	-1.262**	-1.252**	-1.446***	-1.270**	-6.702***	-5.960**	-7.922***	-5.676***	-3.232***	-3.048***	-3.517***	-2.665**
democrats	(0.555)	(0.543)	(0.562)	(0.498)	(2.455)	(2.464)	(2.485)	(2.211)	(1.124)	(1.106)	(1.181)	(1.169)
	3.750***	4.078***	3.695**		32.081**	23.095	21.155	-0.16	12.129	13.327*	10.079	5.773
Mean abort	(1.306)	(1.466)	(1.647)		(15.621)	(15.417)	(14.958)	(12.560)	(7.403)	(7.290)	(7.307)	(9.367)
Standard	2.558	2.594	2.066		49.193*	18.241	25.865	2.906	10.232	10.647	4.718	0.601
deviation abort	(3.117)	(3.244)	(3.342)		(26.991)	(24.275)	(24.149)	(2.349)	(11.291)	(11.049)	(11.498)	(1.595)
	0.156	0.123	0.103		2.249	-0.118	0.682	3.250***	0.478	0.357	-0.020	2.343***
Skewness abort	(0.186)	(0.202)	(0.227)		(2.028)	(1.959)	(1.891)	(0.912)	(0.962)	(0.948)	(0.968)	(0.517)
	0.012	0.003	0.000		0.091	-0.278	-0.137		-0.020	-0.047	-0.070	
Kurtosis abort	(0.039)	(0.041)	(0.041)		(0.411)	(0.406)	(0.400)		(0.197)	(0.193)	(0.197)	
Interquartile	-0.031	0.036	0.287		-2.003	0.001	0.853		-0.291	0.026	2.067	
range	(0.935)	(0.954)	(0.877)		(4.950)	(5.081)	(4.725)		(2.411)	(2.350)	(2.547)	
Median College		-0.268				-2.566*				-1.315**		
Educated		(0.175)				(1.360)				(0.665)		
Proportion of			2.876	-0.16			5.981				6.261	
females 15-44			(2.210)	(12.560)			(13.554)				(6.651)	
Log of Per			-0.310	2.906			2.435				0.566	
Capita Real			(0.269)	(2.349)			(2.526)				(1.222)	
Log of			0.528***	3.250***			3.860***				2.337***	
Intergovernment			(0.153)	(0.912)			(1.041)				(0.416)	
N. Observations	111	111	111	110	110	110	110	110	111	111	111	111
N. Groups	19	19	19	19	19	19	19	19	19	19	19	19
Wald / LR test	5698.5***	896.16***	532.25***	82.61***	70.93***	71.98***	90.37***	82.61***	74.87***	82.21***	125.68***	85.66***

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively.

Table 5: Factor loadings, principal components analysis of GSS responses

Variable	Factor1	Factor2	Uniqueness
Birth defect	0.674	0.528	0.268
No more children	0.863	-0.314	0.157
Health Risk	0.557	0.663	0.251
No Income	0.855	-0.272	0.195
Rape	0.668	0.504	0.300
Single	0.868	-0.311	0.150
Any reason	0.835	-0.344	0.184
Associated Eigenvalue	4.136	1.360	

Table 6: Restricted Sample Regressions, Imposed Needs Indicator, General Social Survey												
	Population Averaged Poisson				Random Effects Ordered Probit				Random Effects Tobit			
Specification	1	2	3	4	1	2	3	4	1	2	3	4
Dependent Variable	Number of legislative restrictions (0-4)				Number of legislative restrictions (0-4)				Abortion-reducing effect of legal restrictions			
	0.769**	0.769**	0.882**	1.137***	5.931**	6.010**	7.670***	9.288***	2.058**	2.036**	2.587**	3.057***
Median abort	(0.345)	(0.346)	(0.379)	(0.416)	(2.386)	(2.386)	(2.285)	(2.499)	(1.020)	(1.025)	(1.038)	(0.853)
Median abort	-0.168	-0.168	-0.237	-0.098	0.264	0.341	0.002	0.026	0.110	0.107	-0.289	-0.046
republicans	(0.153)	(0.152)	(0.181)	(0.207)	(1.360)	(1.363)	(1.230)	(1.246)	(0.672)	(0.670)	(0.680)	(0.603)
Median abort	-1.104***	-1.103***	-1.143***	-0.809***	-6.664***	-6.617***	-8.063***	-4.634**	-2.983***	-2.937***	-2.840***	-2.020***
democrats	(0.310)	(0.311)	(0.343)	(0.265)	(2.071)	(2.077)	(1.936)	(1.926)	(0.812)	(0.856)	(0.831)	(0.715)
	1.729	1.717	2.414*		17.853*	17.207*	14.435*	13.543	6.697	7.199	7.226	8.500*
Mean abort	(1.808)	(1.775)	(1.340)		(10.384)	(10.231)	(8.732)	(16.797)	(4.481)	(4.548)	(4.474)	(4.729)
Standard	0.426	0.410	0.184		16.793	16.809	2.149	1.067	6.191	7.001	3.823	0.018
deviation abort	(3.106)	(3.022)	(2.520)		(17.611)	(17.256)	(14.688)	(3.152)	(7.782)	(8.080)	(7.783)	(1.016)
	-0.406	-0.408	-0.391		-0.678	-0.766	-2.400	5.595***	-0.782	-0.688	-1.049	2.499***
Skewness abort	(0.612)	(0.604)	(0.467)		(3.421)	(3.334)	(2.913)	(1.646)	(1.533)	(1.540)	(1.540)	(0.313)
	-0.114	-0.114	-0.137		-0.215	-0.215	-0.783		-0.182	-0.159	-0.342	
Kurtosis abort	(0.151)	(0.149)	(0.126)		(0.956)	(0.935)	(0.803)		(0.420)	(0.426)	(0.420)	
Interquartile	0.411	0.413	0.479		1.997	1.846	4.348**		1.027	0.909	1.421	
range	(0.402)	(0.408)	(0.323)		(2.100)	(2.114)	(1.813)		(1.001)	(1.108)	(1.044)	
Median College		0.005				-0.288				-0.299		
Educated		(0.143)				(1.025)				(0.536)		
Proportion of			1.744	13.543			-5.665				3.761	
females 15-44			(1.730)	(16.797)			(9.525)				(6.789)	
Log of Per			-0.015	1.067			4.533**				1.029	
Capita Real			(0.186)	(3.152)			(2.119)				(1.197)	
Log of			0.471***	5.595***			3.367***				2.261***	
Intergovernment			(0.135)	(1.646)			(0.729)				(0.425)	
N. Observations	111	111	111	110	110	110	110	110	111	111	111	111
N. Groups	19	19	19	19	19	19	19	19	19	19	19	19
Wald / LR test	99164.16***	5050.02***	372.26***	85.62***	79.12***	78.9***	127.77***	85.62***	87.06***	86.62***	135.18***	231.63***

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively.

Table 7: Restricted Sample Regressions, Preference Based Indicator, General Social Survey												
	Population Averaged Poisson				Random Effects Ordered Probit				Random Effects Tobit			
Specification	1	2	3	4	1	2	3	4	1	2	3	4
Dependent Variable	Number of legislative restrictions (0-4)				Number of legislative restrictions (0-4)				Abortion-reducing effect of legal restrictions			
	0.327*	0.358**	0.404**	0.701***	0.681	1.090	1.533	3.227***	0.452	0.509	0.594	1.768***
Median abort	(0.188)	(0.172)	(0.186)	(0.202)	(1.535)	(1.588)	(1.601)	(0.932)	(0.740)	(0.745)	(0.672)	(0.496)
Median abort	-0.166*	-0.165*	-0.213**	-0.165*	-0.895	-1.072*	-0.862	-0.592	-0.516*	-0.517*	-0.565**	-0.451
republicans	(0.085)	(0.085)	(0.096)	(0.092)	(0.630)	(0.645)	(0.654)	(0.577)	(0.302)	(0.298)	(0.276)	(0.307)
Median abort	-0.402**	-0.414**	-0.433**	-0.418**	-1.304*	-1.646**	-1.727**	-1.531**	-0.909**	-0.912**	-0.953***	-0.897**
democrats	(0.203)	(0.194)	(0.181)	(0.183)	(0.768)	(0.817)	(0.867)	(0.740)	(0.384)	(0.376)	(0.344)	(0.391)
	4.401	4.189*	4.769*		34.324	33.658	41.903*	4.957	13.619	13.460	14.812	7.197
Mean abort	(2.754)	(2.503)	(2.771)		(24.143)	(24.171)	(25.346)	(13.860)	(10.884)	(10.884)	(9.927)	(9.925)
Standard	2.951	2.963	3.619*		59.355***	56.435***	53.852***	2.066	21.429***	21.149***	22.685***	0.392
deviation abort	(2.813)	(2.927)	(2.141)		(19.761)	(20.177)	(20.990)	(2.446)	(8.153)	(8.183)	(7.295)	(1.558)
	0.769	0.700	0.837		5.225	4.828	7.837	3.332***	2.104	2.003	2.328	2.334***
Skewness abort	(0.673)	(0.627)	(0.660)		(5.460)	(5.456)	(5.734)	(0.967)	(2.491)	(2.490)	(2.279)	(0.582)
	0.062	0.057	0.090		3.371*	3.106	2.351		1.299*	1.265*	1.379**	
Kurtosis abort	(0.252)	(0.252)	(0.210)		(1.816)	(1.904)	(1.730)		(0.730)	(0.731)	(0.674)	
Interquartile	-0.190	-0.160	-0.109		0.909	0.393	0.890		0.085	0.155	0.550	
range	(0.258)	(0.277)	(0.287)		(2.206)	(2.549)	(2.211)		(1.053)	(1.041)	(0.948)	
Median College		-0.101**				-0.718*				-0.288		
Educated		(0.050)				(0.417)				(0.205)		
Proportion of			3.916*	4.957			23.893				13.290**	
females 15-44			(2.357)	(13.860)			(16.177)				(5.489)	
Log of Per			-0.577*	2.066			-0.581				-0.713	
Capita Real			(0.327)	(2.446)			(3.013)				(1.114)	
Log of			0.583***	3.332***			5.375***				2.653***	
Intergovernment			(0.171)	(0.967)			(1.629)				(0.383)	
N. Observations	111	111	111	110	110	110	110	110	111	111	111	111
N. Groups	19	19	19	19	19	19	19	19	19	19	19	19
Wald / LR test	46104.1***	6065.26***	633.66***	80.92***	77.74***	81.07***	96.28***	80.92***	76.64***	80.83***	633.66***	84.7***

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively.

Table 8: First Stage Regression Tests

			With controls	Without controls
First Stage Instruments Significance	Democrats		0.0008	0.0008
	Republicans		0.007	0.020
Endogeneity test	Democrats	Population Averaged Poisson	0.335	0.154
		Random Effects Tobit	0.2747	0.3747
		Random Effects Ordered Probit	0.7082	0.6291
	Republicans	Population Averaged Poisson	0.0292	0.0033
		Random Effects Tobit	0.0564	0.081
		Random Effects Ordered Probit	0.1522	0.0729

Table 9: Comparison of Baseline with IV estimates

	Population Averaged Poisson		Random Effects Tobit		Random Effects Ordered Probit	
	Baseline	IV	Baseline	IV	Baseline	IV
Median abort	0.641***	0.576**	1.097*	0.828	4.790**	4.398
	(0.219)	(0.275)	(0.662)	(0.988)	(2.097)	(9.761)
Median abort republicans	-0.344**	-0.729***	-0.898**	-1.668*	-3.457***	-5.706
	(0.155)	(0.250)	(0.386)	(0.973)	(1.329)	(8.839)
Median abort democrats	-0.490***	-1.049**	-1.161***	-2.536	-3.812***	-9.13
	(0.128)	(0.476)	(0.437)	(1.823)	(1.411)	(16.943)

Note: Standard errors are in parentheses.*,** and *** indicate significance tests at the 10%, 5%, and 1% levels, respectively. Significance tests are based on bootstrapped standard errors using 2000 replications.

Figure 1: Expected Policy Function

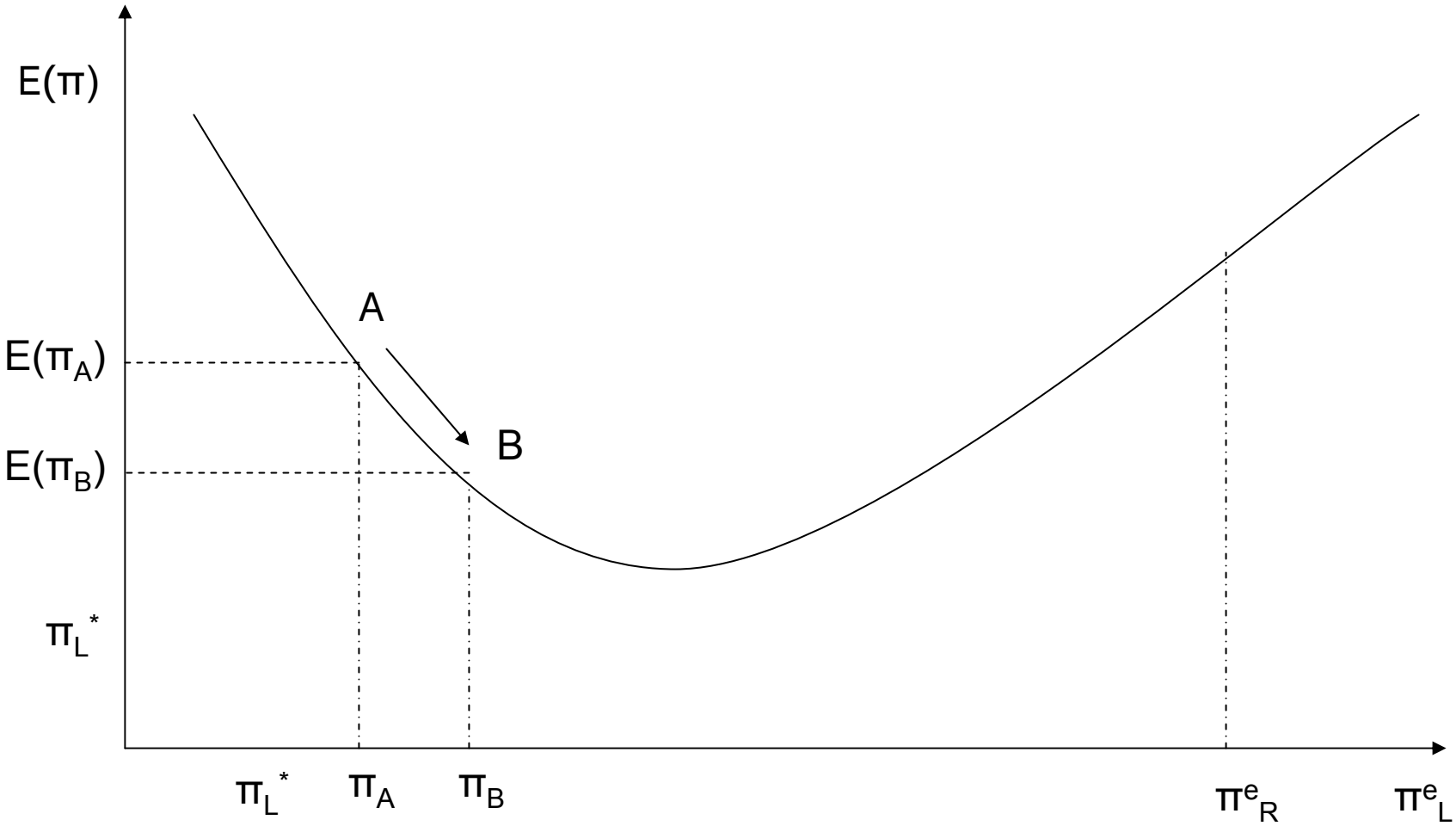

Figure 2: Expected Policies as a function of left party's preferred policy

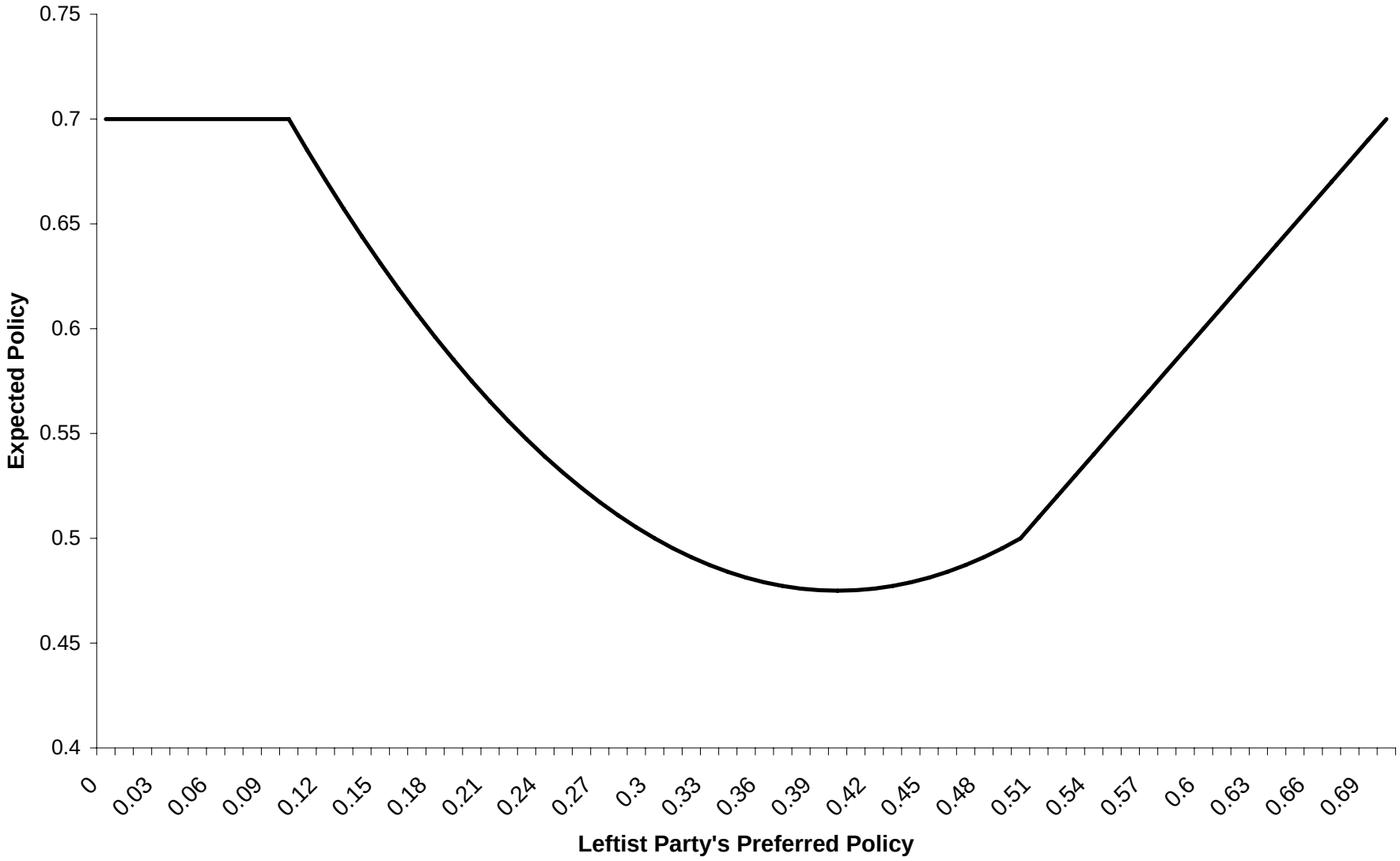

Figure 3: Expected Policies as a function of right party's preferred policy