Minority Rights and Direct Legislation:
Theory, Methods, and Evidence

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Abstract
In recent years, one of the most debated questions about direct legislation has concerned its effect on minority rights. Recent theory suggests that these effects are both direct and indirect. Policy advocates influence policy directly by passing or blocking new laws by initiative or referendum. They influence policy indirectly when legislatures respond to the threat or use of direct legislation. However, most empirical studies focus exclusively on outcomes at the ballot box and so are limited to estimating direct effects. Theory also suggests that direct legislation institutions mediate underlying voter preferences in specific ways. Using multivariate logistic regression analysis, we compare the probability of having various minority protection and anti-discrimination laws in American states that do and do not allow direct legislation. We find that permitting direct legislation has a minimal independent effect on minority rights policies. Rather, its presence and use changes the mapping between voter preferences and outcomes. Thus, depending on the nature of voter preferences, direct legislation institutions may either increase or decrease minority protections.

Introduction

A number of recent studies have addressed the question of whether direct legislation undermines minority rights (Gamble 1997, Witt and McCorkle 1997, Donovan and Bowler 1998, Frey and Goette 1998, Tolbert and Hero 1998, Donovan, Wenzel, and Bowler 1999).[1] Strikingly, they arrive at very different conclusions. Gamble (1997), for example, concludes that in American states and cities, direct legislation significantly curtails minority rights achieved through the legislative process. Donovan and Bowler's (1998) results, based on analyses of state level ballot measures on the civil rights of gays and lesbians, contradict some of Gamble's findings. And Frey and Goette (1998) show that in Switzerland, comparatively few measures restricting minority rights have passed in popular votes.

It is lamentable that on a question as important as the effect of direct legislation on minority rights, such contrasting and contradictory conclusions appear. Direct legislation is now used in 27 American states and hundreds of cities and counties. In recent years, it has been used more and more often to decide social and moral issues with direct implications for racial, ethnic, sexual, and social minorities. Indeed, as observers such as Schrag (1998) contend, the politics of immigration reform, affirmative action, gay rights, and bilingual education, embodied in statewide initiatives in several states, represented some of the most important state-level political battles of the 1990s and show no sign of diminishing in the 21st Century.

One might argue that the different conclusions produced in these recent studies result from their uses of different datasets and approaches; we take a different stand in this paper. We argue that by focusing on passage rates of anti-minority ballot measures, the results reported in

[1] We employ the term "direct legislation" to refer to lawmaking by direct citizen vote. This includes voting on referendums, where citizens or interest groups petition to vote on legislation passed by elected representatives, and voting on popular initiatives, where citizens or interest groups draft legislation, qualify it for the ballot, and vote on it directly.

recent articles fail to account for two important insights produced by recent theoretical work on the policy consequences of direct legislation. First, these theories emphasize the *indirect* effects that the existence, potential, and use of direct legislation may have on legislative policy makers. In the end, we care about the set of policies that affect minorities, not only the subset passed by initiatives and referendums. Therefore, accounting for the possibility that direct legislation may have both direct and indirect effects allows us to more accurately assess whether states that allow direct legislation produce more or less minority-protective policies. Second, recent theoretical work also considers the aggregation properties of direct legislation institutions, particularly how they change the mapping between citizen preferences and outcomes. Failing to account for differences in these preferences might lead to incorrect inferences about the relative impact of preferences and institutions.

Taking into account these insights leads us to utilize more appropriate methods for studying the effects of direct legislation on minority rights. Consequently, we analyze the overall impact of direct legislation (both direct and indirect) on a set of policies aimed at protecting (or not protecting) numerous under-represented groups, including language minorities, racial and ethnic minorities, women, and gays and lesbians. By studying a range of policies, considering both direct and indirect effects, and relying on a coherent theoretical framework to specify our empirical models, we believe our approach provides a more accurate view of the overall impact of direct legislation on minority rights. It also allows us to reconcile the existing empirical results by highlighting what assumptions must hold for their conclusions to be valid.

In the next section, we briefly review the recent theoretical work that forms the basis for our analysis. This review suggests that the effect of direct legislation can be best assessed by comparing policy outcomes between states that do and do not allow direct legislation. We develop our empirical model and present evidence that strongly supports our view that direct legislation has both direct and indirect effects on policy outcomes at the state level. We also find, however, that once we account for differences in voter preferences across states, the independent effects of direct legislation are minimal. In other words, direct legislation does not, by itself, necessarily lead to policies that undermine minority rights, but rather amplifies the preferences of the voting majority. We conclude by emphasizing the need for additional careful and theoretically guided empirical work studying the policy consequences of direct legislation.

Theory

Scholars have debated the consequences of direct legislation since the early days of American democracy.[2] Much of the recent work on direct legislation consists of formal models that use spatial or game-theoretic approaches to study the impact of direct legislation on policy outcomes (e.g., Steunenberg 1992, Gerber 1996 and 1999, Moser 1996, Besley and Coate 2001, Matsusaka and McCarthy 2001 and Hug and Tsebelis 2002). In these studies, one observes the emergence of some consensus that direct legislation institutions can have both direct and indirect influences; this consensus is increasingly shared among direct legislation scholars of various methodological leanings (e.g., Cronin 1989; Magleby 1995; Bowler, Donovan, and Tolbert 1998, 169; and Donovan and Bowler 1998, 7). Matsusaka (2000, 658) describes some of these effects:

> Theory suggests that the initiative can influence policy in three ways: (1) citizens can propose and approve policies directly, (2) the threat of an initiative can cause the legislature to approve policies different from those it would pass in the absence of an initiative, and (3) election returns from initiative contests can convey information to representatives about citizen preferences that they later incorporate into policy.

[2] Donovan and Bowler (1998) discuss this early debate, dating to Publius, about the majoritarian tendencies of direct democracy.

The first of these influences is direct in the sense that the effect on policy comes about as an immediate consequence of the initiative or referendum. The second and third influences are indirect in the sense that the effect on policy comes about as policy makers in another arena, the legislative process, adapt their behavior in anticipation of, or in response to, initiatives or referendums.[3] Gerber (1996), for instance, proposes a theoretical model that demonstrates the second type of influence, that is, the ability of initiative proponents to pressure state legislators through the threat of adverse initiatives. In Gerber's model, legislators who care about policy outcomes may pass compromise legislation (i.e., laws that are closer to the ideal points of the initiative proponent and the median voter) in order to preempt a more extreme majority-preferred initiative. Matsusaka and McCarty (2001) analyze a similar model that accounts for the possibility of a legislature learning about policy options from votes occurring in other states, thus providing a rationale for the third type of influence.[4] Note that both forms of indirect influence can occur without a popular vote ever taking place on an initiative or referendum in a given jurisdiction. Rather, initiative proponents need only be able to credibly threaten to mobilize the necessary resources - both financial and electoral - to pass an initiative that the legislature opposes.[5] Schmidt (1989, 26) gives a few interesting examples of these indirect effects:

> Frequently, just the filing of an Initiative petition will spur legislators into action. Laws passed under the pressure of an Initiative petition include Massachusetts'

[3] One might add shirking or buck-passing by legislators as another example of indirect influence (e.g., Magleby 1984, 186 and 192; Cronin 1989, 229). Legislators might prefer to have voters consider particularly contentious topics in the direct legislation process, rather than addressing those issues themselves in the legislature. Apart from these policy effects, indirect effects also may appear in other realms, for instance when politicians use an initiative as a campaign tool (see Nicholson 1998). In this paper we focus on the policy effects of direct legislation.

[4] Matsusaka and McCarty also show that when legislators are uncertain about voter preferences, the threat of initiatives may induce them to pass laws that are further from the ideal point of the median voter.

[5] The logic of indirect effects requires that legislators are held responsible (or believe they will be held responsible) for policy outcomes, even if the policies are actually passed by voters. So, for example, a legislator will prefer to preempt an initiative if she believes her financial or electoral constituents will withhold support if the legislature fails to protect their interests from damaging initiatives.

> laws to reduce the air pollution that causes acid rain (1985) and ban the experimental use of cats and dogs from pounds (1983); Arizona's laws abolishing the state sales tax on food (1980), making funding available for Medicaid (1982), and restricting water pollution caused by toxic chemicals (1986); and Wyoming's law to maintain stream flows adequate for fish and wildlife (1986).

In addition to emphasizing direct legislation's indirect effects, most recent theoretical work (e.g., Steunenberg 1992; Matsusaka 1995 and 2000; Gerber 1996 and 1999; Moser 1996; Besley and Coate 2001; and Matsusaka and McCarty 2001) suggests that policies in direct legislation (DL) states will not only differ from policies in non-direct legislation (NDL) states, but that they will differ in predictable, systematic ways. Specifically, they find that policies in DL states should, on average, more closely reflect the median voter's (i.e., the majority's) preferences than policies in NDL states. For example, a number of studies focusing on direct effects show that broad-based majoritarian interests are much more effective than narrow economic interests in passing direct legislation measures (Lowenstein 1984, Owens and Wade 1986, and Gerber 1996, 1999). Others, focusing on indirect effects, show that to exert credible pressure on a legislature, groups must have the backing of an electoral majority (Gerber 1996). Together, these studies imply that having direct legislation changes the mapping between citizen (i.e., majority) preferences and outcomes, in effect augmenting the ability of majoritarian interests to translate their preferences into policy outcomes.[6]

Recent empirical work on the effect of direct legislation on policy (although not necessarily minority rights policy) attempts to control for the preferences of voters. Gerber (1996, 1999) finds in her analysis of laws requiring parental consent for teenage abortions and allowing

[6]To the extent that the third effect described by Matsusaka (2000) (that is, signaling citizen or interest group preferences) prevails, the representational consequences depend on the type of group doing the signaling. To our knowledge, no spatial model has focused on the effects of such different groups when it comes to signaling preferences.

the death penalty that legislative policies in DL states more closely reflect the (estimated) median voter's preference than legislative policies in NDL states. However, Lascher et al. (1996) find that for eight policies, the mean of the voters' positions on a liberal-conservative scale fails to be more strongly related with the final policy outcome in DL states than in NDL states. Camobreco (1998) finds similar null results for differences in taxing and spending policies in DL and NDL states.[7]

Both of these theoretically important elements - indirect effects and voter preferences - are absent in recent empirical studies of minority rights (e.g., Gamble 1997, Donovan and Bowler 1998a, and Frey and Goette 1998). Indeed, since all of these articles draw their conclusions from analyses of the percentage of anti-minority initiatives and referendums that passed on the ballot, their results about the effects of direct legislation hold only if indirect effects are unimportant. Likewise, since they omit measures of voter preferences, their conclusions about what drives passage rates are valid only if preferences are constant across jurisdictions or have neither an independent nor a mediating effect on outcomes.

Methods

We extend the recent analyses by estimating the net policy consequences - both direct and indirect - of direct legislation in three different policy areas affecting minorities, namely English-only laws, affirmative action policies, and protections for gays and lesbians against discrimination. We chose these policy areas for several reasons. First, all have been important foci of recent direct legislation activity in several states. This means that voters are likely to be aware of these issues and to have well-formed preferences; interest groups can credibly threaten

[7] Matsusaka (2001) provides critiques of both Lascher et al.'s (1996) and Camobreco's (1998) empirical model specification.

to propose initiatives in these policy areas; and policy-motivated state legislators cannot ignore their threats. Second, they include several of the areas for which Gamble (1997), Donovan and Bowler (1998), and Frey and Goette (1998) come to very different conclusions in their recent studies. Third, the policy areas are sufficiently narrow to ensure a high degree of comparability across states. Fourth, they include both policies that aim to increase (in the cases of gay/lesbian protections and affirmative action) and policies that aim to decrease (in the case of English-only laws) protections for targeted minorities. Fifth, they include policies targeting a range of social/policy minority groups (i.e., language minorities, racial/ethnic minorities, women, and sexual minorities). Sixth, relevant data for these policies are readily accessible.

Our empirical approach builds upon the insights from the recent theoretical models described above. These models suggest that the difference between an adopted policy and the ideal point of the median voter ($|P-X_m|$) should be smaller in DL states than in NDL states. In addition, both theoretical and empirical work suggests that this effect should be smaller as the costs of employing direct legislation increase (Gerber 1996, Matsusaka and McCarty 2001). Consequently, the theoretical models imply the following specification of the empirical model:

$$|P-X_m| = f(DL, DL*Costs, Z) \qquad (1)$$

where Z is a vector of control variables.

There are two major problems with implementing this empirical framework. First, X_m is typically estimated using surveys or population characteristics. However, as numerous papers have shown, estimates of aggregate preferences such as the median voter's ideal point based on national random sample surveys will be biased due to non-representative state-level sub-samples.[8] Estimates based on population characteristics will be only rough proxies of the median voter's preferences towards a particular policy, resulting in attenuated estimates. Second, these problems are especially severe if both P and X_m are measured continuously. Several studies ignore the problem of scaling by simply dropping the absolute value operator on the left-hand side of the equation, moving X_m to the right-hand side, and adding an interaction between X_m and DL (see e.g., Lascher et al. 1996, Camobreco 1998). As Matsusaka (2001) shows, this approach may lead to severely biased inferences.

Our analysis addresses these specification issues by analyzing policies that can be more naturally measured as dichotomies (i.e., did the state have a given policy or not).[9] This allows us to employ the familiar logit specification. The question then becomes how swiftly policy reacts to shifts in voter preferences in DL and NDL states. Hence, the following specification translates the theoretical models into an empirical specification for dichotomous policies:

$$\text{prob}(P) = f(X_m, DL, X_m{*}DL, X_m{*}DL{*}Costs, Z) \qquad (2)$$

We estimate this model for each of our three minority protection policies. For each policy, the dependent variable is a dichotomous variable scored one if the state has such a policy at a point in time and scored zero if it does not.

Estimating X_m poses a difficult challenge. Ideally, one would rely on surveys eliciting support or opposition for each particular policy from large, random samples of citizens or voters in each state. From these responses, we could then compute the median preference of the state's

[8] Erikson (1978) and Jackson (1989) develop methods to reduce some of this bias, while Jones and Norrander (1996) discuss a method to assess the reliability of such measures.

[9] Indeed, one great advantage of using direct legislation as a way of studying the relationship between preferences and policy is that voter choices naturally take on this dichotomous quality (see Johnston and Lupia 2001). This approach requires us to assume that policies are comparable across states. We select our policy areas with this assumption in mind.

electorate and compare the proximity of policies and preferences in DL and NDL states. Short of this ideal, there are two alternative approaches. The first alternative is to proxy preferences with a general measure of ideology.[10] The second alternative is to employ population or community characteristics that we expect to be correlated with attitudes towards minority rights.[11] We use both approaches in this study. As our primary measure of preferences, we estimate X_m as the mean value of self-reported ideology for respondents in each state in the Pooled Senate Studies (Miller, Kinder, Rosenstone and NES 1993).[12] The Pooled Senate Studies sample voters in all fifty states. While somewhat small (n=127 to 209), these state-level sub-samples approximate random samples within each state. In addition, we also include variables measuring several population characteristics that specialists on minority rights have used as controls.[13]

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[10] The advantage of this approach is that measures of ideology, at both the individual level (from numerous public opinion surveys) and at the state level (from composite measures such as those developed by Erikson, Wright and McIver 1993) are readily available. However, the disadvantage of this approach is that it is not clear that preferences towards the specific policies we analyze necessarily correspond with a unidimensional left-right measure of ideology. For example, both traditional liberals and civil libertarians may support policies that prohibit discrimination against racial or language minorities, for very different reasons, with the former scoring high on a left-right ideology scale but the latter scoring low.

[11] The advantage of this approach is that such measures are also easily obtained for each state. One disadvantage is that they are only rough indicators of preferences. A second disadvantage is that these indicators, such as membership in gay rights or Latino organizations, may arise from a hostile environment, rather than indicate supportive citizen preferences. In other words, they may be either positively or negatively related to underlying preferences.

[12]Theoretical models employ the state's median voter's preference. In empirical applications where voter survey responses are measured with a very small number of categories, the median preference tends not to vary much across states. In addition, provided that underlying preferences are distributed symmetrically, the mean and median preference is the same. Hence, in this article we consistently employ estimated mean preferences. Where appropriate, we also estimated the same models using median preferences. Virtually all of our results show the same general patterns but are weaker when we employ median preferences.

Our estimates of state median voter's preferences range from -50.4 in Arkansas to -2.4 in Massachusetts, with -100 indicating a completely conservative state and 100 a completely liberal state. We note that the correlation between our state ideology measure and Erikson, McIver and Wright's (1993) measure used by Lascher, Hagen and Rochlin (1996) and Camobreco (1998) is only 0.66 (for the 48 states covered by Erikson, Wright and McIver, 1993) and is only slightly higher if we exclude Nevada, which these authors consider as an outlier. For the current purposes of estimating the state's median voter's ideology, we believe our measure, which is constructed strictly from reported voter ideology, is preferable to Erikson, Wright, and McIver's (1993) composite measure of state ideology. It also allows us to cover all 50 states, instead of eliminating Alaska and Hawaii (missing data) and Nevada (outlier). Nevertheless, we also estimated our models with Erikson, Wright and McIver's (1993) measure and found largely similar results.

[13] Strictly speaking these controls also measure preferences, and thus should appear with interactive terms with the DL-dummy in the empirical specification. Such a specification, however, would diminish rapidly the degrees of

DL is a dummy variable scored one if the state allows initiatives or referendums, and scored zero otherwise. Including this dummy variable in our model allows us to test whether direct legislation produces a systematic policy bias, that is, whether policies are more or less protective of minority rights, independent of the effects of preferences and the other controls. However, since all of the theoretical models discussed above suggest that the effect of direct legislation is dependent on voter preferences, we expect this dummy variable to have no sizable independent effect on the likelihood of the various policies.

X_m*DL captures the interaction between preferences and direct legislation institutions. According to the theoretical literature, we expect direct legislation to move policy (both directly and indirectly) in the direction of the median voter's preference. In other words, we expect policies in DL states to more closely reflect the majority's preference. Like-signed coefficients on X_m and X_m*DL are consistent with this hypothesis.

X_m*DL*Costs captures the effect of the cost of ballot access on the relationship between direct legislation institutions, voter preferences, and policy. We operationalize costs as the state's signature requirement, which is usually the most substantial cost associated with accessing the ballot. Costs are defined the lowest of the state's signature requirements for statutory and constitutional initiatives and referendums, measured as the percent of the electorate's signatures required to qualify a measure for the ballot. As Gerber (1996) and Matsusaka (2000) argue, we expect the impact of direct legislation institutions to be mediated by the costs of ballot access. In states where ballot access is costly (i.e., where the signature requirement is high), we expect the effects of institutions to be less than in states where ballot access is relatively easy. We therefore expect the sign on X_m*DL*Costs to be the opposite of the sign on X_m*DL.

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freedom, and for this reason we refrained from pursuing this avenue. Doing so is likely to weaken our results.

English-only laws

Our first set of analyses compare English-only laws in DL and NDL states. These policies range from largely symbolic measures that simply declare English as a state's official language, to more substantive measures that proscribe certain actions (e.g., printing ballots, election materials, and other government publications in English only). Santoro (1999) shows that English-only laws have been adopted through both the initiative process and the legislative process. Current statutes date from 1811 in Louisiana, to the most recent adoptions in 1998 (Alaska and Missouri).[14] Many of these laws are perceived to have anti-minority - and in recent years, anti-Latino - undertones.[15]

~~We first report a difference of proportions test that compares DL and NDL states. The χ^2 statistic shows that NDL states are significantly less likely to have such policies than DL states.[16] However, this bivariate analysis looks only at overall differences in policies between DL and NDL states and does not control for preferences or costs, which also differ across states.~~

We ~~next~~ estimate a series of multivariate logistic regression models based on equation (2). We begin with a baseline model that includes our estimate of preferences plus population characteristics that have been found in previous research to relate to the probability that a state has an English-only law (see Santorro 1999). These include a dummy variable for southern states (members of the former Confederacy), the state's average vote for the democratic presidential candidate in 1980, 1984 and 1988, an interaction between south and mean presidential vote, ,

[14] Source: http://www.english-only.org (accessed October 5, 2000).

[15] A case challenging Alabama's English-only law has recently been heard by the US Supreme Court. In that case, a Mexican immigrant argued that the law is discriminatory in that it requires driver's license tests to be taken in English (*Financial Times*, January 17, 2001, 4).

[16] RERUN The χ^2 equals 4.46, which with one degree of freedom is significant at the .03 level.

percent growth in the Latino population over the period 1980 and 1997, percent of adults with a bachelor's degree, percent urban, government revenue, and a measure of elite values with respect to gays and lesbians.[17] ~~Based on Gerber's (1996) model, we also include three control variables tapping into the political environment of the state. The first measures the professionalization of the state legislature using Squire's (1992) data. The second measures the competitiveness of the state's political system using Holbrook and Van Dunk's (1993) competitiveness measure, while the third reflects the turnover rate in the state's lower house. We expect these three factors to influence the legislature's responsiveness to interest group demands, with highly professionalized legislatures and legislatures with higher turnover being less responsive and legislatures with greater competitiveness being more responsive.~~ Table 1 reports the results of our analyses of English-only laws.

Model 1.1 reports the results of our baseline model. Among the various control variables we find that the institutional strength of the hispanic population (i.e., voter registration and legislators) significantly affects the presence of an english-only law, this, however, negatively. The other control variables only weakly affect the presence of these laws. Our preference measure also fails to influence significantly the presence of english-only laws, and in addition the coefficient flips signs compared to a bivariate logistic regression.[18] Adding our DL dummy (model 1.2) only weakly affects our results. Most coefficients only change marginally and a log-likelihood ratio test suggests that the addition of this variable does not significantly improve the model (p=0.334). The positive of the estimated coefficient suggests that DL states are, on average, more likely to have english-only laws, a finding also reported in Santoro (1999).

[17] We selected for each of our policies the most significant explanatory variables and used these in all analyses, if possible. Please refer to the appendix for descriptions and sources of each variable.

[18] In a bivariate model the preference measure significantly influences the dependent variable : -0.056 (s.e. 0.027), constant : -1.798 (s.e. 0.818).

In model 1.3, we add the interaction between preferences and direct legislation (X_m*DL), and an interaction between preferences, direct legislation, and the state's signature requirement (X_m*DL*Costs). On the basis of this specification we obtain results that are largely in line with the theoretical models. While the same positive and insignificant overall effect of voter preferences remains, we see that liberal DL states are less likely to have English-only laws, and conservative DL states are more likely to have these laws. This interaction between preferences and institutions is significantly reduced by higher signature requirements, as indicated by the significant positive sigh on X_m*DL*Costs. The coefficient on the DL dummy is negative but insignificant, and the signs and magnitudes on the control variables are largely unchanged. Finally, in terms of overall fit, the institutional and preference variables are jointly significant with the log-likelihood ratio test between models 1.2 and 1.3 significant at $p<.05$. Model 1.3 correctly classifies 43 of the 50 observations, compared to 41 correctly predicted in model 1.2. And the pseudo-R^2 increases from 0.49 in model 1.2 to 0.63 in model 1.3.[19]

[20]

Table 1
Determinants of English-only Laws
Logistic Regression Estimates

Because logit coefficients are difficult to interpret, we provide figure 1, which illustrates the joint effect of direct legislation institutions and voter preferences on the likelihood of states having English-only laws. We plot the predicted probabilities for such laws as a function the estimated mean state ideology. We depict three curves, the first showing the probability of

[19] We report the Nagelkerke R^2.

[20] Analysis of the results in table 2 produce a figure similar to figure 1 for gay rights policies. However, since the effect of voter preferences in the baseline (NDL) case is positive but insignificant in model 2.3, the figure reflects an upward sloping curve for NDL states. We discount this result due to the insignificant coefficient on the effect of preferences.

having an English-only law for NDL states, the other two showing these same probabilities for DL states with a 2 percent signature requirement (the lowest used in any state) and a 15 percent signature requirement (the highest used in any state).[21]

Affirmative action in public contracts and public work

Affirmative action has been arguably the centerpiece of civil rights legislation in many states since the early 1970s. In recent years, a number of states have changed their affirmative action policies. Impetus for much of the recent legislation began in California, where University of California Regent Ward Connerly successfully spearheaded an effort to roll back affirmative action in UC admissions. Shortly thereafter, three states followed suit and adopted racially blind admission procedures (*Economist* March 13, 1999; *Los Angeles Times* February 18, 2000, A22; *New York Times*, Feburary 27, 2000). Connerly subsequently took his effort statewide and orchestrated the passage of Proposition 209 (1996), which prohibited affirmative action in public contracting, employment, and education. In the wake of Prop 209s passage, 15 states adopted similar laws repealing affirmative action in the public sector. Most of these recent changes have intended to weaken or eliminate existing affirmative action statutes.

In our analysis, we focus on a wide range of affirmative action policies. Our dependent variable is a dichotomous variable scored one if a state's current laws include language that establishes affirmative action policies in the broadly defined areas of public contracts or public work.[22] Overall 15 states' codes contain such references. We utilize this summary measure for many of the same reasons underlying our composite measure of gay rights policies, specifically

[21] We held all remaining variables, e.g., our control variables, constant at their sample means.

[22] Santorro and McGuire (1997) analyze a wide range of policies mandating affirmative action in public contracts but summarize them for the purposes of their analysis in a single additive index.

our interest in the overall policy climate and the partial substitutability of many individual policies. As in the previous analysis, we begin with a baseline model that includes population and political controls found in previous research to be related to citizen preferences for affirmative action (see Santorro and McGuire 1997). These include a dummy variable for southern states, the mean democratic presidential vote in the 1980s, an interaction between south and presidential vote, income per capita, and a measure of the strength of civil rights organizations (NAACP affiliates).[23]

Table 2 reports the results of our logistic regression analyses. In model 2.1, we find that the level of government revenues significantly affects the presence of affirmative action programs in public contracts. Our preference measure is positively related to the presence of these provisions, but the estiamted coefficient fails to reach statistical significance.[24] In model 2.2, we add our DL dummy. This effect is positive but insignificant, implying that states with more liberal electorates are more likely to have affirmative action policies (but there is a great deal of variability across states). In model 2.3, the effects of the interactions between preferences and direct legislation, and between preferences, institutions, and costs are in the hypothesized directions, but none of these effects are significant.~~[25]~~ Despite these weak results, the inclusion of the preference and institutional variables improves the overall explanatory power of the model.

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[23] Several of Santorro and McGuire's independent variables, namely revenues, percentage of blacks in the population, the number of black legislators, NOW membership, number of female elected officials and a spatial control, are not significant in our model and so are dropped.

[24] In a bivariate model the preference measure significantly influences the dependent variable : 0.104 (s.e. 0.038), constant : 1.737 (s.e. 0.932).

[25] ~~As with our analysis of English-only laws, further analysis of these results produces a figure similar to figure 1, with the same caveat above – since the estimated effect of voter preferences in the baseline case is insignificant, its counter-intuitive effect on the relationship between preferences and the probability of having an affirmative action policy should be strongly discounted. However, such analysis still powerfully illustrates the interactive effect of preferences and direct legislation institutions, with the probability of a state having an affirmative action policy increasing rapidly in direct legislation states with low signature requirements, and the probability increasing more slowly in direct legislation states with high signature requirements.~~

The pseudo-R^2 improves from 0.54 in model 2.2 to 0.56 in model 3.3. Not surprisingly, this small difference proves insignificant in a log-likelihood test (p=0.540).

Table 2
Determinants of Affirmative Action Policies
Logistic Regression Estimates

Protecting gays and lesbians against discrimination

Finally, we compare gay and lesbian rights policies in DL and NDL states. Haider-Markl and Meier (1996) study several similar policies in their analysis of policies protecting gays and lesbians from discrimination. They find that the degree of protection against discrimination in the states depends on interest group politics, the salience of gay politics, and several environmental factors.[26]

We analyze a composite measure of the policies included in Haider-Markl and Meier's analysis. Specifically, we create a dichotomous variable scored one if a state had protections in five or more of the seven categories considered by Haider-Markl and Meier, and scored zero if they had four or fewer such policies.[27] We utilize this composite measure, rather than indicators of the individual protection policies, for several reasons. First, although a given individual may be concerned with whether his or her state prohibits discrimination in one particular area (such as a prospective homebuyer being particularly interested in protections against discrimination in housing), we as analysts are interested in the overall policy consequences of direct legislation for

[26] Haider-Markl and Meier's study focuses on the proportion of a state's population covered by any anti-discrimination measures, thus they consider both state and local policies. We recognize that there may be important effects at the local level as well. We leave the question of the effects of direct legislation institutions on local level minority rights policy to future research.

[27] To facilitate comparison with Haider-Markel and Meier's study of similar policies, we consider those laws adopted as of August 1998. Donovan, Wenzel, and Bowler (1999) discuss the outcomes of more recent votes on gay rights measures. Further, policies in several states have been invalidated by court decisions but are still on the books. In each case, we adopt the codings of the Policy Institute of the National Gay and Lesbian Task Force (1998).

minority rights. In other words, we are interested in the set of policies that aim to protect or not protect minorities, and hence this composite measure more closely reflects our theoretical interests than any single policy. Second, even if we are interested in protections in specific areas, we expect the impact of a given policy to be highly dependent upon the existence (or non-existence) of other policies. A state that has a single policy that protects gays and lesbians from discrimination in a single area may interpret and implement that policy differently than a state that has numerous protection policies in place. Third, a number of these protection policies may be partially substitutable, meaning that the same effective protections may be felt by members of the minority group in states that have formal policies relating to different areas. At the same time, we recognize that our composite measure has some shortcomings. Perhaps most importantly, using an additive measure is somewhat arbitrary (even though the policies follow very closely a pattern of a Guttman scale), as is our cut-off of five or more policies.[28]

As previously, we estimate a series of multivariate logistic regression models. Model 3.1 in table 3 reports the results of our baseline model, which contrary to the previous two analyses omits the set of variables related to the democratic presidential vote and the dummy for the states of the confederacy. The reason for this omission is that the southern dummy perfectly predicts the absence of protective measures in all southern states. Given this caveat, the results in table 3 are consistent with expectations, based on previous analyses, but the effects are generally weak and insignificant. Only the elite values with respect to gays and lesbians significantly and positively affects the degree of protections against discrimination. In other words, in states where

[28] Constructing a Guttman-type scale results in only two errors with the following order of policies (from most frequent to least frequent: protection in public employment, protection in private employment, protection in public accommodation, protection in housing, protection in education, protection in credit, and protection for unions. With respect to the cut-off point, our analysis does show that, empirically, it seems to differentiate states with substantial protections from those with minimal protections. Similar but statistically weaker results obtain from different cut-offs.

the gay community has support among the elite, state policies are more likely to contain substantial protections against discrimination. Our preference measure is also positively related to the presence of these protections, but the coeffficient is not statistically significant.[29]

Model 3.2 adds our DL dummy to the baseline model. The coefficient for this dummy is not significant, but the log-likelihood ratio test suggests, that model 3.2 improves significantly upon model 3.1. We find that preferences are positively (although not significantly) related to having protections against discrimination. In other words, states with more liberal (mean) preferences are more likely to have policies that protect gays and lesbians, and states with more conservative (mean) preferences are less likely to have these policies, although there is a great deal of variation across states. These effects are independent of other features of preferences captured by the population characteristics in the baseline model.

Model 3.3 reports the results of our full model, which introduces the interactions of DL and preferences and of DL, preferences, and costs. The results reported in model 3.3 strongly support the theoretical models discussed above. The probability of having protective measures increases much more quickly in DL states than in NDL states, as indicated by the positive coefficient on X_m*DL. In other words, direct legislation is associated with a much greater responsiveness of policy to citizen preferences. In addition, the signature requirement depresses this stronger link between preferences and policies, as shown by the negative sign on X_m*DL*Costs. More precisely, the results suggest that a signature requirement of 10 percent, which is the maximum employed by any state, would nearly wipe out the mediating effect of direct legislation. We also find that the effect of the DL dummy is negative but insignificant. We take this as consistent with the argument that direct legislation does not, in itself, produce

[29] Again, in a bivariate model the preference measure significantly influences the dependent variable : 0.113 (s.e. 0.050), constant : 0.949 (s.e. 1.077).

policies that undermine minority rights, but rather mediate the effects of preferences. Finally, in this full model, the population control variables are of the same sign and roughly the same magnitude, but fail to achieve statistical significance.

Table 3
Determinants of Gay and Lesbian Protection Policies
Logistic Regression Estimates

Comparing models 3.1 and 3.3, it is clear that the inclusion of the institutional variables significantly improves the overall fit of the model. Model 3.3 classifies 47 of the 50 observations correctly, while 3.1 correctly classifies only 44 of the 50. The pseudo-R^2 improves from .554 in model 3.1 to .702 in model 3.3, and a log-likelihood ratio test between models 3.1 and 3.3 is significant at $p<.05$. Comparing models 3.2 and 3.3 suggests that the interactive terms do not reach significance, but combined with the DL dummy, they do.

~~**Figure 1**~~
~~**Predicted Probabilities Gay and Lesbian Protection Policies**~~

~~The first curve, representing the probability of having protection laws in NDL states, is the flattest one. It shows that policy reacts only weakly to voter preferences in NDL states. The curves for DL states are much steeper, suggesting a much closer mapping between policy and voter preferences. States with direct legislation and a 2 percent signature requirement show the closest mapping (steepest curve), while a signature requirement of 15 percent slightly decreases this effect. Substantively, Figure 1 shows that our model predicts that English-only laws are largely absent in the most conservative states ($X_m < -30$), whether or not they have direct legislation. In slightly more moderate states ($-30 < X_m < -10$), such laws are most likely to exist in DL states with low signature requirements and are least likely in DL states with high signature~~

~~requirements. In more liberal states ($-10 < X_m < 10$) we find that direct legislation leads to a higher likelihood of having English-only laws. Finally, in the most liberal ("out of sample") states ($10 < X_m$) we would expect all states to have such laws, irrespective of whether or not they allow for direct legislation.~~

Summary

So far we have interpreted the results of our various models only in terms of the size and signs of the theoretically interesting coefficients, namely those for the preference measure and the interaction terms with this variable. Given that our contention is that the effect of provisions for DL is mediated by the preferences of the citizens and that the estimated models are non-linear and comprise various interaction terms, we provide in table 4 an analysis of the effects of direct legislation in terms of predicted probabilities of any state having one of the three policies covered.[30] For this we hold all control variables at their means (including the confederacy dummy, which is a bit stupid) and vary the value for preference measure from the lowest to the highest empirically relevant values (Table 4, column 1) . Next we calculated for each of the three policies the predicted probabilities for NDL states of having the particular set of policies (Table 4, columns 2,5, and 8). Next we calculated for each policy and each level of the preference measure the predicted effect of DL, once for a DL cost of 5 % (Table 4, columns 3,6,9) and once for a DL cost of 2 % (Table 4, columns 4,7,10).

For the english-only laws we find that especially in conservative states direct legislation increases the likelihood of the presence of such laws, while this effect decreases, or becomes even slightly negative (in the case of costs of 2 %) for the most liberal states. Given that in our model the

[30] For this analyses we employed Clarify 2.0 (King, Tomz, and Wittenberg 1998 ; Tomz, Wittenberg, and King 1999).

coefficient for our preference measure has flipped, we also find that conservative NDL states have a low probability of having english-only laws, while liberal NDL states have a high probability.

The results are much stronger for affirmative action. Here the predicted probabilities of having such policies in NDL states varies as expected with our preference measure. Conservative states are less likely than liberal states to have statues for public contracts prescribing some affirmative action. Looking at the predicted effects of DL we find that in conservative states DL decreases the probability of having affirmative action measures, while it leads to increases in liberal states. A similar pattern, though less clear appears for the protective measures of gays and lesbians. Again for the NDL states we find that conservative states are less likely to have such measures, while the predicted probability increases among more liberal states. And again, though the pattern is a bit blurred, DL amplifies this effect, with conservative states with DL even less likely to have such measures, and very liberal states being even more likely to have such measures.

Table 4: Predicted effect of direct legislation on three policies

pref	english predicted probability in ndl	effect of dl (costs= 5 %)	effect of dl (costs= 2 %)	affirm predicted probability in ndl	effect of dl (costs= 5 %)	Effect of dl (costs= 2 %)	g and l predicted probability in ndl	effect of dl (costs= 5 %)	effect of dl (costs= 2 %)
	b	b	b	b	b	b	b	b	B
	s.e	s.e	s.e	s.e	s.e	s.e	s.e	s.e	s.e
-50	0.14	0.20	0.67	0.23	-0.14	-0.11	0.06	0.01	0.00
	0.24	0.38	0.40	0.29	0.32	0.34	0.18	0.36	0.36
-40	0.17	0.29	0.69	0.19	-0.09	-0.05	0.05	-0.01	-0.01
	0.21	0.36	0.34	0.20	0.25	0.28	0.15	0.31	0.31
-30	0.32	0.35	0.59	0.17	-0.01	0.03	0.05	-0.04	-0.03
	0.14	0.28	0.24	0.11	0.19	0.24	0.13	0.24	0.26
-20	0.66	0.18	0.27	0.22	0.20	0.21	0.07	-0.10	-0.10
	0.20	0.18	0.20	0.16	0.22	0.25	0.15	0.20	0.21
-10	0.80	0.09	0.12	0.32	0.42	0.39	0.12	-0.12	-0.16
	0.26	0.22	0.21	0.29	0.32	0.31	0.22	0.26	0.26
0	0.83	0.10	0.09	0.39	0.39	0.39	0.20	-0.16	-0.16
	0.28	0.24	0.23	0.36	0.34	0.34	0.30	0.44	0.44
10	0.85	0.05	0.01	0.44	0.45	0.43	0.29	0.00	0.05
	0.29	0.29	0.28	0.40	0.43	0.42	0.37	0.51	0.53
20	0.86	0.04	-0.02	0.47	0.44	0.42	0.37	0.02	0.10
	0.30	0.31	0.32	0.42	0.45	0.44	0.41	0.56	0.58

Conclusion

Understanding how direct legislation affects the rights of minorities is of considerable importance. Theory suggests that the effects of direct legislation can be both of a direct and indirect nature, and that these effects are conditional upon voter preferences. Indeed, institutions act not in a vacuum; they mediate the preferences of a state's voting majority. Taking these insights seriously, we compare aggregate policy outcomes in direct legislation and non-direct legislation states. Thus, while our approach does not allow us to separate the direct and indirect effects on a specific policy area, we are able to say whether, at the end of the day, several types of social/policy minorities (gays and lesbians, language minorities, racial/ethnic minorities, and women) are made better or worse off by the existence and use of direct legislation. We find that, in fact, there is mixed evidence of an independent effect of direct legislation on the probability of a state adopting various policies aimed at minorities. Rather, the important consideration is the degree of voter support for minority rights.

We believe the framework we employ here provides a more general view of the effects of direct legislation on minority rights than the one presented in Gamble (1997), Donovan and Bowler (1998) and Frey and Goette (1998). While direct legislation works against minority protections in some states, as Gamble (1997) argues, we show that this is largely due to the voters' conservative ideology. We also find that in liberal states, direct legislation works in favor of minority protections. Only in very conservative and very liberal states do we find no effect, as implied by Donovan and Bowler's (1998) and Frey and Goette's (1998) conclusions. Thus, as the theory suggests, our analysis shows that the effect of direct legislation is highly dependent on the voters' preferences. In states where the voting majority has preferences that are hostile to the rights of minorities, direct legislation allows the majority to enact policies that are less protective of minority rights. In states where the majority prefers stronger protections for minorities, direct legislation provides a mechanism for achieving those protections.

Among the policies analyzed in this paper, we find that in conservative states direct legislation works against the protection of social/policy minorities. At the same time, we also find that direct legislation favors the adoption of protective policies in more liberal direct legislation states. Reconciling these two findings clearly suggests that direct legislation fails to have an independent effect on policy. Quite to the contrary, direct legislation simply acts as an amplifier in aggregating majority preferences.

Finally, we briefly consider the generalizability of the findings we report in this paper. On the one hand, examining policies that aim to protect (or not protect) several different types of social/policy minorities, and obtaining consistent results across these policy areas, leads us to conclude that our results can be confidently extended to many other policy minorities as well. On the other hand, it is still possible that the policies analyzed in this paper are unique in terms of the degree of polarization between the targeted minority groups and the voting majority. If this is true, then minorities in NDL states may find some implicit protection as legislators feel reluctance to take sides with the majority on these issues. In DL states, this policy majority has an additional mechanism for pursuing their policy interests, and minority rights may be sacrificed. Hopefully, more careful research along these lines will provide evidence of the effects (or lack thereof) of direct legislation on the rights of other social, economic, and political minorities.

Appendix

This appendix provides information on the data used in our study. The dataset and results of additional analyses are available at the following URL: http://. . . . .

Variable	min.	mean	max.	n
Policies protecting gays and lesbians from discrimination (Source: Policy Institute of the National Gay and Lesbian Task Force, 1998)	0	0.18	1	50
Affirmation Action (Bureau of National Affairs 2000)	0		1	50
English-only laws (Santoro 1999 and www.english-only.org, accessed May 30, 2000)	0	0.32	1	47
Ideology (Miller, Kinder, Rosenstone, and the National Election Studies 1993)	-50.36	-27.35	-2.42	50
Direct legislation (Magleby 1994)	0		1	50
Signature requirement (Malgeby 1994)	0		15	50
Professionalization of legislature (Squire 1992)	0.04	0.22	0.66	50
Competitiveness of state politics (logged) (Haider-Markel and Meier 1994)	2.23	3.59	4.04	50
Turnover of state lower house (1994) (Book of the States, 1997, 70)	2.0	55	21.90	50
Log(gay businesses) (Haider-Markel and Meier 1994)	0	1.17	2.64	50
Log(task force membership) (Haider-Markel and Meier 1994)	-0.56	1.39	3.3	50
Black (% of population 1997) (Source: U.S. Bureau of the Census, Estimates of the Population of States by Race and Hispanic Origin: July 1, 1997; published 4 September 1998; http://www.census.gov/population/estimates/state/srh/srhus97.txt	0.34	10.11	36.36	50
Hispanic origins (% of population) (idem)	0.55	6.53	40.06	50
South (Santoro 1999)	.00	.22	1.00	50
Mean Vote for Democratic Presidential Candidate (1980-1988) (Statistical Abstract)	28.83	43.77	56.70	50
Percent Hispanic Legislators (Book of the States 1989)	0.00	5.64	33.06	50
Percent Among Hispanic Registered Voters in 1994 (Statistical Abstract)	.00	35.49	80.00	50
Latino Protest Events (Santoro 1999)	0.00	.26	3.00	50
Hispanic Population Growth (1980-1997) (Statistical Abstract)	-3.23	2.24	11.61	50
Income per capita (1989) (Book of the States, 1989)	9.08	12.70	20.01	50

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Table 1: Determinants of English-Only Laws, Logistic Regression Estimates

Independent Variables	Model 2.1	Model 2.2	Model 2.4
	β (s.e.)	β (s.e.)	β (s.e.)
Elite values (log)	-0.43 1.80	-0.72 1.84	-2.85 2.58
Percent Hispanic Legislators + Voters 1989	-1.09 0.58	-0.98 0.56	-1.25 0.61
Hispanic Population Growth (1980-1997)	0.00 0.00	0.00 0.00	0.00 0.00
Government revenue (in bn 1990)	-0.01 0.07	-0.02 0.08	0.01 0.07
South	-3.67 34.28	-8.80 36.11	-1.06 40.47
Mean Democratic Presidential Vote (1980s)	-0.04 0.11	0.01 0.12	-0.06 0.15
South * Mean Democratic Presidential Vote	0.21 0.81	0.33 0.86	0.22 0.97
Percent college education (1990)	-0.01 0.16	0.04 0.17	-0.03 0.20
Percent urban population (1990)	-0.01 0.02	-0.01 0.02	-0.02 0.03
X_m (Mean Preference/ Ideology)	0.05 0.07	0.03 0.07	0.17 0.14
DL		0.88 0.92	-0.20 2.89
X_m*DL			-0.20 0.16
X_m*DL*Costs			0.03 0.01
Constant	4.17 6.70	1.02 7.42	18.24 11.87
Mode N correctly predicted	28 42	28 41	28 43
-2llik	46.65	45.71	36.95
Pseudo-R^2	0.48	0.49	0.63
N	50	50	50
Model 1		0.334	0.021
Model 2			0.012

Table 2: Determinants of Affirmative Action in Public Contracts, Logistic Regression Estimates

Independent Variables	Model 3.1	Model 3.2	Model 3.4
	β (s.e.)	β (s.e.)	β (s.e.)
Elite values (log)	2.27 2.50	2.03 2.54	3.80 3.31
Percent Hispanic Legislators + Voters 1989	-1.30 1.26	-1.23 1.15	-1.02 0.99
Hispanic Population Growth (1980-1997)	0.00 0.00	0.00 0.00	0.00 0.00
Government revenue (in bn 1990)	0.16 0.09	0.16 0.09	0.14 0.08
South	34.58 34.61	29.51 32.84	43.43 38.00
Mean Democratic Presidential Vote (1980s)	-0.09 0.15	-0.04 0.17	-0.11 0.19
South * Mean Democratic Presidential Vote	-0.79 0.81	-0.68 0.77	-1.02 0.90
Percent college education (1990)	0.00 0.18	0.06 0.20	0.00 0.20
Percent urban population (1990)	-0.01 0.03	0.00 0.03	0.00 0.03
X_m (Mean Preference/ Ideology)	0.12 0.09	0.10 0.10	0.03 0.11
DL		0.84 1.10	4.15 3.33
X_m*DL			0.15 0.15
X_m*DL*Costs			0.00 0.01
Constant	-4.95 7.60	-8.61 9.02	-12.27 10.26
Mode			
N correctly predicted	42	42	41
-2llik	37.77	37.18	35.95
Pseudo-R^2	0.53	0.54	0.56
N	50	50	50
Model 1		0.439	0.609
Model 2			0.54

Table 3: Determinants of Gay and Lesbian Protection Policies, Logistic Regression Estimates

Independent Variables	Model 1.1	Model 1.2	Model 1.3
	β (s.e.)	β (s.e.)	β (s.e.)
Elite values (log)	**6.75**	**6.88**	**7.44**
	3.44	**3.88**	**3.98**
Percent Hispanic Legislators + Voters 1989	**-0.27**	**-1.14**	**-2.26**
	0.74	**2.35**	**3.87**
Hispanic Population Growth (1980-1997)	**0.00**	**0.00**	**0.00**
	0.00	**0.01**	**0.01**
Government revenue (in bn 1990)	**-0.01**	**-0.02**	**0.00**
	0.08	**0.09**	**0.10**
South			
Mean Democratic Presidential Vote (1980s)			
South * Mean Democratic Presidential Vote			
Percent college education (1990)	**0.17**	**0.21**	**0.21**
	0.24	**0.30**	**0.34**
Percent urban population (1990)	**-0.02**	**-0.05**	**-0.01**
	0.04	**0.05**	**0.06**
X_m (Mean Preference/ Ideology)	**0.01**	**0.07**	**0.07**
	0.09	**0.10**	**0.12**
DL		**-2.65**	**-2.06**
		1.69	**5.50**
X_m*DL			**0.33**
			0.44
X_m*DL*Costs			**-0.04**
			0.03
Constant	**-31.39**	**-28.81**	**-34.65**
	15.83	**17.43**	**19.74**
Mode			
N correctly predicted	**44**	**46**	**47**
-2llik	**24.37**	**20.72**	**17.52**
Pseudo-R^2	**0.554**	**0.636**	**0.702**
N	**50**	**50**	**50**
Model 1		**0.056**	**0.003**
Model 2			**0.202**

Figure 1: Predicted Probabilities for Protections in Housing