Pass a Law, Any Law, Fast!
State Legislative Responses to the *Kelo* Backlash*

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In Kelo v. City of New London, the U.S. Supreme Court left it to the states to protect property against takings for economic development. Since Kelo, thirty-seven states have enacted legislation to update their eminent domain laws. This paper is the first to theoretically and empirically analyze the factors that influence whether, in what manner, and how quickly states change their laws through new legislation. Fourteen of the thirty-seven new laws offer only weak protections against development takings. The legislative response to Kelo was responsive to measures of the backlash but only in the binary decision whether to pass any new law. The decision to enact a meaningful restriction was more a function of relevant political economy measures. States with more economic freedom, greater value of new housing construction, and less racial and income inequality are more likely to have enacted stronger restrictions, and sooner. Of the thirteen states that have not updated, Arkansas, Oklahoma and Mississippi are highly likely to do so in the future. Hawaii, Massachusetts and New York are unlikely to update at all.

1. INTRODUCTION

> The 5-4 decision in *Kelo v. New London* affirms that eminent domain is an important tool for local governments in the redevelopment and revitalization of economically distressed areas.… Judicious use of eminent domain is *critical* to the economic growth and development of cities and towns throughout the country.
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> *International Economic Development Council* [1]

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> Within the last fifty years, "public use" has been interpreted more broadly to be "public purpose," which is subsequently reinterpreted by public officials as "public benefit" and has now become a primary vehicle for transferring property rights and ownership from one private owner to another…. [I]n some cases eminent domain has become a tool of first resort (and sometimes the only tool).
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> *Reason Public Policy Foundation* [2]

> For more than a century our jurisprudence has wisely eschewed rigid formulas and intrusive scrutiny in favor of affording legislatures broad latitude in determining what public needs justify the use of the takings power.… Clearly there is no basis for exempting economic development from our traditionally broad understanding of public purpose…. We emphasize that nothing in our opinion precludes any State from placing further restrictions on its exercise of the takings power.
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> *STEVENS, J. for majority of the Kelo Court* [3]

Public use and just compensation are the most commonly discussed protections of individual rights against the takings power, but another potentially important check is federalism. Although the *Kelo* ruling in June 2005 created no new law, nearly all the state legislatures have since considered changing their eminent domain laws. As of June 2008 bills to further restrict eminent domain powers had been passed in 38 legislatures and were enacted in 37 of those states.[4] Six of those 37 states also enacted reforms by direct or mixed democracy mechanisms, such as citizen initiatives and legislative referenda. Four additional states have enacted laws only by popular vote, so a total of 41 states have passed some kind of reform in response to the *Kelo* ruling. Voters and commentators decried the perceived injustice of the *Kelo* ruling, which ignited a national backlash of popular support for property rights. It was with great zeal that state legislators took up Justice Stevens' invitation to further limit eminent domain powers.

However, these new eminent domain laws do not necessarily increase the protection of individual rights against economic development takings. The

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[1] "Eminent Domain Resource Kit," prepared by the International Economic Development Council, accessed online at www.iedconline.org, June 20, 2006.

[2] Staley and Blair (2005).

[3] Kelo et al. v. City of New London, Connecticut, 545 U.S. 469 (2005) at 12-16.

[4] Governors in New Mexico, Arizona, and Iowa vetoed bills in 2006. The Iowa legislature over-rode the veto, and New Mexico passed separate legislation in 2007.

Supreme Court left such protection to the states' political systems, which are run by fallible policymakers subject to swells of public opinion and pressure by organized interests. Since economic development takings are a form of wealth transfer that attract political competition, rational policymakers balance competing interests according to their relative abilities to exert political pressure. Whether these state laws further restrict eminent domain, and in what manner, are empirical questions. While the legislative response to *Kelo* has been addressed in much recent research, this is the first paper to investigate empirically the political-economic determinants of how the state legislatures responded to the *Kelo* backlash.

We first evaluate each of the new laws and categorize the states according to whether they changed their eminent domain law by legislation.[5] If yes, we also categorize by whether the new legislation was largely symbolic—enacting only weak restrictions—or instead imposed meaningful constraints on development takings. We then estimate limited-dependent-variable and duration models to determine the political-economic factors that influence the legislative responses. We find that whether a state updates its law is influenced by observed measures of the intensity of the *Kelo* backlash and the collective action costs of local governments. However, deciding on a meaningful versus symbolic change, and the timing of the new laws, are determined by a state's history of economic liberty, the value of new housing construction, and racial and income inequality. The legislative responses are not empirically determined by measures of policymaker behavior, such as corruption or dependence on property taxes, or presidential voting patterns, population density, and political institutions.

The race and income effects are of particular relevance to concerns that economic development takings systematically harm the poor and non-white segments of society. When choosing sites for economic development plans, policymakers may prefer to target poor areas because they are where the public-interest need for development is greatest. However, buyouts in minority and poor areas are prone to escalate to condemnation proceedings because property values are lower, so subjective value is relatively high and just compensation is relatively low. The poor are also less able to mount political resistance to takings. Many critics of the *Kelo* decision, including Justice Clarence Thomas in his dissenting opinion, point out these income and racial effects.[6] An important question that we take up in this paper is to what extent,

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[5] Initially we model only those new laws that originated in the legislature. Later in our robustness checks we also count referenda and initiatives in the dependent variable.

[6] See THOMAS, J., dissenting, *Kelo et al. v. City of New London, Connecticut*, 545 U.S. 469 (2005) at 17-18: "The consequences of today's decision are not difficult to predict, and promise to be harmful. So-called 'urban renewal' programs provide some compensation for the properties they

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if any, legislative responses to *Kelo* serve to alleviate the bias of development takings against minorities and the poor. In the next section, we discuss a rational choice basis for expecting little of such alleviation, and in the sections that follow we report findings in support of that.

2. THEORETICAL MOTIVATION: COLLECTIVE INSTITUTIONAL CHOICE

Like other political institutions, eminent domain laws are intended to constrain, or otherwise alter, policymakers' choice sets. Some states have loose statutory restrictions on eminent domain power. For example, New York's law, which dates to 1978 and was not modified after *Kelo*, enumerates eminent domain powers and specifies bureaucratic procedures (for holding public hearings, determining just compensation, etc.) but does not otherwise state restrictions on condemning authorities' powers.[7] Elsewhere, states impose statutory restrictions on policymakers but allow generous exemptions and loopholes. For example, in states where "blight" is defined expansively, it is easier for condemning authorities to satisfy public use requirements.[8] Where exemptions are initially narrow in scope, local officials tend to mine loopholes to loosen restrictions over time.

Beginning in the late 1800s court decisions have incrementally expanded takings power (Ely, 2005). Through deference to legislative bodies, the judiciary has allowed a gradual expansion of public use to encompass broad economic

take, but no compensation is possible for the subjective value of these lands to the individuals displaced and the indignity inflicted by uprooting them from their homes. Allowing the government to take property solely for public purposes is bad enough, but extending the concept of public purpose to encompass any economically beneficial goal guarantees that these losses will fall disproportionately on poor communities. Those communities are not only systematically less likely to put their lands to the highest and best social use, but are also the least politically powerful. If ever there were justification for intrusive judicial review of constitutional provisions that protect 'discrete and insular minorities,' *United States v. Carolene Products Co*., 304 U.S. 144, 152, n. 4 (1938), surely that principle would apply with great force to the powerless groups and individuals the Public Use Clause protects. The deferential standard this Court has adopted for the Public Use Clause is therefore deeply perverse. It encourages 'those citizens with disproportionate influence and power in the political process, including large corporations and development firms' to victimize the weak. Ante, at 11 (O'CONNOR, J., dissenting)."

[7] New York relies on judicial review to constrain eminent domain powers. Current precedent interprets economic development as public use (see discussion in López and Totah 2007). The statute is Laws of New York, "Eminent Domain Procedure Law," accessed online February 25, 2007, at public.leginfo.state.ny.us. Section 103, Paragraph (G) states: "'Public project' means any program or project for which acquisition of property may be required for a public use, benefit or purpose."

[8] Examples of states with broad exemptions and loopholes include Alabama, Maine, Minnesota, Nebraska, Texas, Vermont and Wisconsin. For detailed discussion, see Sandefur (2006), Somin (2007), López and Totah (2007) or López, Kerekes, and Johnson (2007).

benefits such as the elimination of blight by urban renewal,[9] protection of competitive real estate markets,[10] jobs and growth,[11] and the well-intentioned democratic planning of economic development.[12] Beginning in the 1980s local governments have taken an increased role in centrally planning local and regional economic development. The list of projects is readily familiar: office parks, sports arenas, casinos, auto assembly plants, mixed-use communities and so on. Over time, these types of projects have gradually joined more traditional land use policies made by state and local governments, such as rights of way for common carriers, environmental conservation, and recreation space. All these projects compete for relatively large tracts of land, which often must be pieced together of dozens or even hundreds of contiguous properties. Private developers have a long and well-documented history of successfully assembling contiguous properties through market transactions (Benson, 2005). But with the gradual expansion of public use, developers began to realize new options for partnering with local governments in land acquisition. Developers could now weigh the transaction costs of dealing with scores of property owners against the bureaucratic and rent seeking costs of forging long-term relationships with land use officials. Not only do local governments tend to sweeten deals with tax increment financing and other subsidies, but the power of eminent domain serves as an expedient tool in forcing out extant property owners. From 1998 through 2002, over 10,000 properties in 41 states were threatened or condemned for transfer to other private parties (Berliner, 2003).[13]

But it had been half a century since the last major Supreme Court case (*Berman v. Parker* in 1954) involving the displacement of large numbers of property owners for apparent private gain. The general public was relatively oblivious to the increasing emergence of development takings. Then came the *Kelo* decision in June 2005. It is no wonder the ruling came as such a shock and created much controversy. The Supreme Court put states on notice that *Kelo*-style development takings are the law of the land, and a plurality of states were compelled to establish a different threshold.

The states have adjusted in similar ways to previous federal baseline protections of personal property rights. According to Hynes, Malani and Posner (2004), the federal Bankruptcy Reform Act of 1978 established generous (relative to most states) property exemption levels to protect debtors in

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[9] *Berman et al. v. Parker et al.*, 348 U.S. 26 (1954).

[10] *Hawaii Housing Authority v. Midkiff*, 467 U.S. 229 (1984).

[11] *Poletown Neighborhood Council v. Detroit*, 410 Mich. 616, 304 N.W. 2d 455 (1981).

[12] *Kelo et al. v. City of New London*, Connecticut, 545 U.S. 469 (2005).

[13] Somin (2004) reports that between 3.5 and 4 million people have been dislocated under federally funded urban renewal programs since the late 1940's.

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bankruptcy cases. The Act gave the states the option of defaulting to the federal baseline protections or enacting legislation to opt out. In all, 37 states opted out, the most likely being states with less generous exemptions than the federal baseline, relatively high bankruptcy rates, and less redistribution to the poor. In a similar fashion, *Kelo* was an external shock to the states' equilibrium policies. In response, the states that preferred to restrict development takings more than the *Kelo*-established federal baseline would be the most likely to update their laws, impose more meaningful restrictions, and do so sooner.

Although each of the new eminent domain laws is unique, most employ some combination of similar provisions. Some states have relatively strict eminent domain laws. South Dakota and New Mexico passed laws that simply ban takings for economic development, with no exemptions or loopholes.[14] Other states try to narrowly define "public use" or "blight." For example, Florida's new law expressly excludes economic development from its definition of public use, and Georgia prohibits aesthetic considerations in determining blight. Another common provision is to require the condemning authority to pay some percentage premium above fair market value as just compensation.

For purposes of selecting the empirical specification, it is important to recognize these legislative responses as each state's collective choice. Suppose a state has loose restrictions due to past loophole mining or a New York-style court ruling. Suppose further that takings for private transfer have become routine, even though condemning authorities follow necessary procedures of notification, transparency, and compensation. Then *Kelo* sounds the wake-up call. Property owners in the state and property rights advocates may begin to react negatively and vociferate their objection to these takings. The news media may begin to run more frequent and detailed stories. Citizen action groups will form. Buses filled with demonstrators might visit the statehouse. Economic developers and local governments will present their side of the issue. Legislators will face increasing pressure under coalescing public opinion. Bills will be introduced and debated, amended and passed, and perhaps signed into law.

Something generally like that process occurred in the states' legislative responses to the *Kelo* ruling. In equilibrium, legislators enact the policies and institutions that achieve optimal wealth redistribution under prevailing political support and opposition (e.g. Peltzman, 1976). Development takings transfer wealth from property owners to planners and developers. Therefore, in response to *Kelo* the legislatures had to decide whether and how strictly to constrain local policymakers in order to achieve the optimal in development takings. It is

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[14] New Mexico's version of this strict law was vetoed by Governor Bill Richardson in 2006. New Mexico enacted less strict legislation in 2007.

important to note that doing nothing may be the equilibrium-achieving choice, effectively adopting the federal baseline.

In this paper, we approach the issue as one of institutional choice where the state legislature is the unit of decision. A great deal of research has examined states' choices on political institutions.[15] Public choice theory provides an outline of useful factors to consider in modeling collective choice of political institutions. Legislators respond to the political costs and benefits of their actions, which in turn are influenced by interest groups in proportion to their collective action costs.[16] It bears emphasizing that state policymakers are deciding how to constrain the powers of local policymakers. Thus, we treat local governments as a potential interest group seeking to influence the legislature's choice.

Status quo politics is likely to feature prominently in the legislatures' responses. Swells of popular support for attenuating policymaker power tend to be ephemeral, such as the tax revolt of the 1930s, the term limits and regulatory takings movements in the 1990s, and perhaps the ongoing *Kelo* property rights backlash (the endurance of which remains to be seen). This is not to say that popular uprisings fail to have long term effects; for example, some organizations currently advocating for lower taxes had their beginnings in the 1930s. In response to a public outcry, legislators have a strong incentive to do *something*, if only for the appearance of listening to constituents' demands. Uncertain what depth the *Kelo* backlash might have, rational legislators may be inclined to establish political equilibrium via bark-no-bite legislation that comes wrapped in symbolic reassurances to an outraged populace (Edelman, 1964, 1971). Thus, a new law does not necessarily mean that development takings have been restricted. Legislators may also wish to obfuscate the effects of the new laws in order to avoid appearances of corruption (Boylan and Long, 2003).

Policy outcomes exhibit bias in racially and economically fragmented polities. In general, fragmented electorates feature more redistribution of wealth (there being no impetus for redistribution in a homogeneous society). As ethnic and economic minority groups grow in size and number they can earn rents on their representatives' relative ability to win redistributionist coalitions. Their

[15] See for example Shughart and Tollison (1985) on corporate chartering, Besley and Case (1995) on gubernatorial term limits, Hanssen (2002) on judicial selection, de Figueiredo (2003) on line item veto, Stratmann (2005) on campaign finance and ballot access, and López and Jewell (2007) on congressional term limits.

[16] Theoretical contributions to the interest group model include Olson (1965), Stigler (1971), Peltzman (1976), McCormick and Tollison (1981) and Denzau and Munger (1986). For discussion see Tollison (1988) and Mueller (2003). Our focus is on legislation at the state level. For state data and analysis of direct democracy and constitutional amendments, as well as legislation, see Somin (2008).

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incumbents, for example, enjoy greater electoral security independent of performance. In data on U.S. cities, Alesina, Baqir and Easterly (2000) find that greater ethnic fragmentation means that more political conflicts are resolved along racial cleavages, thereby supporting greater wealth redistribution and greater obfuscation (in the form of public employment rather than spending). Similarly, Glaeser and Saks (2006) find that corruption of public officials is greater in U.S. states with greater racial fragmentation. Similar theoretical and empirical results support counterpart effects of income inequality. Takings, especially for economic development, disproportionately redistribute from poor and ethnic property owners (or tenants).

To sum up, we model the states' legislative processes in choosing whether, how and when to reform eminent domain laws. Legislators may be prone to symbolic politics due to the intensity of the popular backlash (perhaps assuming it is ephemeral). Interest groups will attempt to influence the institutional choice. And the institutional choice is expected to reflect the underlying political-economic character of each state. Racial and income inequality should influence the legislatures' responses, as should the profile of prevailing policies that protect individual rights, voter attitudes, and political institutions.

3. CATEGORIZATION OF THE DEPENDENT VARIABLE

We present three alternatives for quantifying states' choices. First a dichotomous coding is used to indicate whether a state legislature enacted a change to its eminent domain laws (in later robustness checks we also include referenda in the dependent variable). For the 37 states that have updated their eminent domain laws as of June 2008, the variable *Law Enact* is coded 1 and 0 otherwise. The state-by-state breakdown and descriptive statistics are reported in Table 1. In Section 5.1 below we report probit estimates of marginal effects on *Law Enact*, and later in Section 5.5 we compare findings against an alternative variable that counts all 41 states that enacted any type of reform.

Table 1: Dependent Variables—By State and Summary Statistics

	State Name	*Law Enact*	*Law Type*	*Law Days*
1	Alabama	1	2	215
2	Alaska	1	2	551
3	Arizona	0	0	926
4	Arkansas	0	0	926
5	California	1	1	648
6	Colorado	1	2	522
7	Connecticut	1	1	805
8	Delaware	1	2	202
9	Florida	1	2	496

10	Georgia	1	2	520
11	Hawaii	0	0	926
12	Idaho	1	2	445
13	Illinois	1	1	575
14	Indiana	1	2	448
15	Iowa	1	2	561
16	Kansas	1	1	503
17	Kentucky	1	2	452
18	Louisiana	0	0	926
19	Maine	1	2	529
20	Maryland	1	1	858
21	Massachusetts	0	0	926
22	Michigan	1	2	628
23	Minnesota	1	2	504
24	Mississippi	0	0	926
25	Missouri	1	2	560
26	Montana	1	2	867
27	Nebraska	1	1	529
28	Nevada	1	2	874
29	New Hampshire	1	2	536
30	New Jersey	0	0	926
31	New Mexico	1	2	853
32	New York	0	0	926
33	North Carolina	1	2	587
34	North Dakota	1	2	855
35	Ohio	1	1	320
36	Oklahoma	0	0	926
37	Oregon	0	0	926
38	Pennsylvania	1	1	489
39	Rhode Island	0	0	926
40	South Carolina	0	0	926
41	South Dakota	1	2	413
42	Tennessee	1	1	515
43	Texas	1	1	244
44	Utah	1	1	445
45	Vermont	1	1	530
46	Virginia	1	2	854
47	Washington	0	0	926
48	West Virginia	1	1	521
49	Wisconsin	1	1	454
50	Wyoming	1	2	789
	Count if = 0	13	13	n/a
	Count if = 1	37	14	n/a
	Count if = 2	n/a	23	n/a
	Mean	.74	1.2	559.4
	Standard Deviation	.443	.832	29.8
	Skew	-1.13	-0.397	0.241
	Kurtosis	-0.76	-1.44	-0.195

Notes: *Law Enact* is coded 1 if the state updated its eminent domain law through the state legislature (referenda are excluded), 0 otherwise. *Law Type* is coded 1 for a weak or symbolic update, 2 for meaningful restriction, 0 otherwise. *Law Days* is the number of days after January 1, 2005 until a state updated, with right truncation at 926 days (2.5 years); descriptive statistics on *Law Days* include only the 37 states that updated.

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Second, a multi-category coding is used to capture variation in the type of laws enacted. We qualitatively analyze each of the new state laws. In general, laws that do little to restrict takings power feature vague and encompassing definitions for "blight" and "public use." Weak or symbolic laws also give broad deference to local legislative majorities. Strong laws that meaningfully restrict takings for private use contain some sort of prohibition on development takings, without much in the way of exemptions or loopholes, while otherwise allowing takings for traditional uses (e.g. common carriage rights of way).

Within this general framework, we surveyed each law on a battery of 18 criteria each designed to elicit a "yes" or "no" response (see Appendix). For example, Criterion 1 was "does the law prohibit the use of eminent domain for promoting tax base, jobs, or economic development?" We found that the answer is "yes" for 23 of the 37 laws surveyed. Criteria 2 through 10 are similar in that a "yes" response indicates that this aspect of the law restricts eminent domain. In contrast, Criterion 11 captures a loophole effect by asking "does the law restrict takings '*solely*' or '*primarily*' for economic development?" We found that the answer is "yes" for six of the 37 laws. Criteria 12 through 18 capture similar factors by which the law becomes weaker in restricting takings power. With responses to all 18 criteria in hand, and assuming equal weights, we take the sum of Criteria 1-10 and subtract the sum of Criteria 11-18. A sensible and systematic pattern emerges. Of the 37 state laws, 14 have a net sum less than or equal to zero, which we interpret to mean the law does little or nothing to restrict takings power. The remaining 23 states have a net sum greater than zero, which indicates that the law does enact some restriction on takings power. The variable *Law Type* is coded {0, 1, 2} for states that enacted {none, weak, meaningful} restrictions on development takings.[17] Table 1 reports the data and descriptive statistics. The estimator used on *Law Type* is ordered probit. Later in the paper, we compare estimates on *Law Type* against two alternative measures of the stringency of the new laws.

Our third coding of the dependent variable captures the timing of the enactment. Some states were quick to respond to the *Kelo* backlash. In fact, several state legislatures began work on the issue even before the June 2005 ruling, and further changes are still being considered in some states at the time of this writing. The variable *Law Days* measures the number of days between January 1, 2005, and the time of enactment (specifically, the date of gubernatorial

[17] Please see the Appendix for further discussion of the qualitative analysis and coding for all criteria by state.

signature), with right truncation at 926 days.[18] The number of days at right truncation is equivalent to two and a half years from January 1, 2005, or approximately two years from the date of the *Kelo* ruling. As of June 2008, the last state to update was Nevada on May 23, 2007, or 874 days. We use a Weibull distribution hazard model to estimate the effect of covariates on *Law Days*, with details discussed in Section 5.3 below. In Section 5.5, we compare estimates on *Law Days* with an alternative measure based on legislative session days.

4. INDEPENDENT VARIABLES

We discuss four sets of independent variables that are expected to influence a state's choice on eminent domain restrictions. Table 2 presents all variable descriptions and summary statistics.

4.1. BACKLASH VARIABLES

The first set of variables measures intensity of the *Kelo* backlash by state. Some states do not have a history of development takings at all, while in others the eminent domain power has become a routine tool of economic development, which makes the issue more likely to be on property-owning voters' minds. The variable *ED9802* measures the number of economic development takings cases per 100,000 people by state, cumulative for the years 1998 through 2002 (Berliner, 2003). After the *Kelo* decision in June 2005, there were many reports of emboldened local policy makers taking greater numbers of properties for private use.[19] To capture the extent of this emboldening effect, the variable *ED0506* counts takings cases per 100,000 people for one year beginning June 30, 2005. In addition, there was a public outcry that was fueled by much media attention. In the year following the decision, about four times as many news stories covered eminent domain than the previous year (López, Kerekes, and Johnson, 2007). The variable *Newspapers* contains each state's count of major newspaper stories in the Lexis/Nexis database for the period January 1, 2004, through September 15, 2006, searching keywords *eminent domain*, *legislation*, and *legislature*.

4.2. POLICY AND POLICYMAKER VARIABLES

The observed takings power in a state may be expected to vary with other measures of policymaker incentives and constraints. For example, where local policymakers already regulate private property more heavily local governments

[18] A few states began debating the issue even in anticipation of the *Kelo* decision. We begin at January 1, 2005, to allow for the probability that states might update the law prior to the issuance of the ruling.

[19] See Berliner (2006) and López and Totah (2007) for examples.

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may lobby harder against further restrictions on their takings power. Proponents of development takings routinely list enhancing the tax base as one of their top justifications. And in the menu of policy options, eminent domain is a substitute for regulation, which affects property values and tax revenues.[20] Unfortunately, we have not been able to identify a data source that measures such regulation by state. Instead, we use *Property Tax_Local*, which measures thousands of local property tax dollars per capita in 2003-04. Another possible substitute is *Commute Times*, average minutes commuting to work, which tend to increase (in some cases by policy design) with more interventionist land regulation.

Table 2: Variable Names, Explanations, Sources and Descriptive Statistics

Category	Variable	Description (Source)	Obs	Mean	Std.Dev.	Min	Max
Backlash Variables	*Newspapers*	Lexis/Nexis™ stories per 100,000 people with keywords *eminent domain*, *legislation* and *legislature*, from 01/01/04 to 09/15/06	50	.912	.968	0	5.32
	ED0506	Properties threatened or condemned per 100,000 people, 06/30/05 to 06/30/06, from news stories (Berliner, 2006)	50	2.76	7.72	0	43.45
	ED9802	Properties threatened or condemned per 100,000 people between 1998 and 2002, from news stories (Berliner, 2003)	50	1.808	3.61	0	20.01
Policy and Policymaker Variables	*Property Tax_Local*	Local governments' property tax revenue, $1000s per capita, 2003-04 fiscal year (U.S. Census, 2000)	50	9.64	4.04	2.11	20.98
	Property Tax_State	State government's property tax revenue per capita, $1000s per capita, 2003-04 fiscal year (U.S. Census Bureau, 2000b)	50	61.49	128.3	0	721.7
	Commute Times	Average minutes commuting to work (U.S. Census Bureau, 2004)	50	23.66	3.45	15.8	31.7
	Corruption Rate	Federal convictions of state and local policymakers per 1,000 people, annual avg. for 1976-2002 (Glaeser and Saks, 2006)	50	27.8	13.34	7.4	64.3
	N_Local Governments	Number of local governments per capita (U.S. Census Bureau, 2002)	50	.640	.891	.016	5.38
	Economic Freedom	Fraser Institute *Economic Freedom of North America* index, overall score, "subnational" grouping, annual average 1981-2003, rescaled from original to range 1 to 100 increasing in freedom (Karabegovic and McMahon, 2006)	50	69.1	6.98	54.0	82.17
State Political and Economic	*Housing Price Index*	Average of quarterly values, 1975Q1-2006Q2 (OFHEO, 2007)	50	169.1	35.9	118.4	292.1

[20] See Fischel (2004) for a detailed description, arguing that policymakers are sensitive to the implicit price of takings and to the cross-price of substitution with zoning regulation.

Characteristics	*Housing Value*	Dollar value ($millions) new home construction authorizations, 2004 (U.S. Census Bureau, 2006)	50	5.84	7.62	.362	36.96
	FDIC Home Loans Value	Dollar value ($millions) of FDIC institution loans (FDIC, 2006)	50	3.24	7.65	.280	43.22
	Income Inequality	= [(percent of families earning less than 25K)* (percent of families earning more than 100K)], for year 2004.	50	29.93	6.44	17.55	44.35
	Racial Diversity	[(10000 - $\sum s_i^2$) / 1,000], where s_i is i's race/ethnicity share as percent of state population. Increasing in diversity (U.S. Census Bureau, 2000a)	50	3.80	1.65	.727	7.64
	Race Percent Non-white	= [1 - percent of population white] (U.S. Census Bureau, 2000a)	50	25.47	15.16	3.9	76.7
	Third Party Voting	Percent of statewide vote for third party presidential candidates, average for five elections 1988 through 2004 (Leip, 2007)	50	7.64	2.12	3.94	13.49
	Land Percent Rural	Percent of state land rural (U.S. Dept. of Agriculture, 2003)	50	74.2	18.06	14.2	95.3
	Population Density	Persons per square mile (U.S. Census, 2000c)	50	159	204.8	.991	995.9
Political Institution and Organization Variables	*Legislature Size*	Number of legislators per 1,000 capita. (Council of State Governments)	50	6.02	6.91	.335	32.63
	Legislature Ratio	Ratio of size of House to size of Senate. (Council of State Governments)	49	2.91	2.17	1	16.67
	Unified Government	=1 if executive of same party as legislative majority, = 0 otherwise (Council of State Governments)	49	.346	.480	0	1

Notes: Nebraska has no data for *Legislature Ratio* and *Unified Government* due to unicameral legislature and non-partisan officials.

Rent seeking and corruption are likely to play a role if more corrupt governments are more resistant to enacting procedural restraints. On the other hand, with more corrupt local officials state policymakers may be under greater pressure to clamp down on eminent domain abuse. To capture the state's history of government corruption, we use the variable *Corruption Rate*, which measures federal convictions of state and local officials per 1,000 capita averaged over the years 1976-2002 (Glaeser and Saks, 2006). Local governments will face greater collective action costs in states with high numbers of localities. And the evidence suggests that those governments practicing development takings (thus presumably opposing restrictions on them) are minority in number. To capture this effect, the variable *N_Local Governments* measures the number of local governments per capita in each state.

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Finally, our dependent variables are likely to be related to the state's protection of individual liberty in other policy areas. In some places state and local governments are accustomed to high taxation, heavy regulation of labor and product markets, and other policies attenuating rights. Their legislatures may be less inclined to restrict takings power. The 2006 edition of *Economic Freedom of North America* provides the data (Karabegovic and McMahon, 2006). The index rates each state on a 10-point scale with a higher score indicating greater economic freedom. We use the "subnational" categorization, which measures the influence of state and local, but not federal, governments. The variable *Economic Freedom* is each state's average overall score for the period 1981-2003, which we then rescale to 100-point range for clarity of interpreting marginal effects. We expect states with a history of greater economic freedom to enact stronger restrictions on development takings after *Kelo*.

4.3. STATES' POLITICAL ECONOMY VARIABLES

Next, any legislation is influenced by the underlying political, demographic, and economic characteristics of the polity. Property owners have a long history of influencing public policy (Fischel, 2001), and the home building industry has been a vociferous opponent of development takings at the federal level.[21] Where housing values are high, home owners and builders have more to lose under strong takings power. We estimate measures of the housing stock (*Housing Price Index*) and also flow (*Housing Value* and *FDIC Home Loans Value*). Based on Fischel's "home voter" hypothesis, we expect these housing value measures to influence stronger restrictions on the takings power.

For racial inequality we use the percent of non-white citizens (*Race Percent Non-White*) and a racial concentration index among ethnicities identified by census data (*Race Diversity Index*).[22] Our variable *Income Inequality* is the product of the percent of families below $25,000 and above $100,000 annual income, capturing mass in the income distribution tails. As discussed in Section 2, we expect greater racial and income inequality to negatively influence whether, how strongly, and when to restrict takings power.

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[21] The National Association of Home Builders and National Association of Realtors filed amici curiae with the Kelo Court. See Pickel (2004), expressing concern for the relative position of new home construction under expanded takings power: "[The] NAHB recognizes that housing will almost never afford a community with the economic development benefits that a commercial application will. If economic development as a sole justification for public use is decided using a rational basis test with deference to local legislative bodies, then the door is left open for local governments to abuse their eminent domain powers and take developable land from NAHB members as they could from any other property owner," (Pickel, 2004:1)

[22] The groups are the Census Bureau categories Asian, Black, Hispanic, Native American, and White.

Voter attitudes also influence legislation. The *Kelo* backlash emerged as a swell of popular support for individual rights. In terms of the popular swell, the backlash is similar to populist movements at the turn of the twentieth century that resulted in initiatives, recall, direct election of U.S. senators, and other provisions to constrain representative democracy. As advocacy for individual rights, the backlash shares similarities with the 1990s term limits movement, which also resulted in widespread institutional change.[23] The *Kelo* backlash was motivated largely by a respect for home and business ownership. We expect that these preferences will naturally be stronger in some states than in others. Presidential vote scores are a common measure of these types of voter attitudes. To focus on the popular swell, we track the state's recent history of supporting third party presidential candidates. The variable *Third Party Voting* is the percent of statewide vote won by all non-Republican and non-Democratic candidates, averaged for the five presidential elections beginning 1988. We expect voters who support third parties to have stronger attitudes toward protecting private property against takings.

Finally, to control for density issues, we include the percent of the land that is rural (*Land Percent Rural*) and the population per square mile (*Population Density*). The main use of rural land is agriculture, which typically does not employ large numbers of people nor generate large amounts of tax revenue. And redevelopment agencies typically are not planned for rural areas, so the support for *Kelo*-level takings power is lower to begin with.

4.4. Political Organization and Institutions

As with any legislation, eminent domain laws are likely to be influenced by political organization and institutions within the state. We use a familiar set of control variables that includes: a dummy variable for whether the governor and legislature are controlled by the same party (*Unified Government*); the number of legislators per capita (*Legislature Size*); and the size of the senate relative to the house (*Legislature Ratio*); all of which are well established in the public choice literature (e.g., Gilligan and Matsusaka, 2006).

[23] About half the states limited terms of state legislators. Many imposed or modified gubernatorial term limits. Also about half voted to limit terms of their own members of Congress, but these laws were overturned in the 1995 Supreme Court case, *U.S. Term Limits, Inc. v. Thorton*. See López (2002; 2003) and López and Jewell (2007) for discussion.

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5. RESULTS

The above Sections 2 and 4 are important theoretical guides for specifying the empirical model. However, there are too many independent variables and combinations thereof to report findings on all possible or even interesting factors that can be investigated. Space limitations are one constraint, but there are modeling constraints as well. To narrow our choice of results to report, we begin by discussing two specification issues presented by the data.

First, we expect ex ante that our dependent variables are endogenous with *Newspapers* and *ED0506*. The argument is intuitive: local officials became emboldened after *Kelo* to take more properties for development purposes. In states where local officials did not expect a new restrictive law, the emboldening effect may have resulted in greater takings. If this is the case, then *ED0506* and *Law Enact* would be endogenously determined. The same is likely to be the case with the variable *Newspapers* since more news attention is observed when the legislature is working on an issue and, in turn, legislators will be more likely to focus on an issue the more news attention it receives. In contrast, there is no expectation that the dependent variables will feed back into the value of *ED9802* because that variable measures a state's history of takings. We expect the most reliable specification will include *ED9802* as a historical instrument for the severity of the *Kelo* backlash.

A second specification problem arises with degrees of freedom and our small sample size. With a cross section of 50 observations, model fit and stability are both sensitive to selection of the independent variables. Certain specifications that we tried either failed to converge or provided unstable estimates (all unreported results are available on request). This type of problem is common in cross-sectional estimates on the U.S. states (e.g., Glaeser and Saks, 2006; López and Jewell, 2007).[24] To achieve a reliable set of results, we make every attempt at parsimony in specifying the models, including as few covariates as needed to

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[24] Glaeser and Saks (2006:1062) state, in regard to their findings on corruption rates across the states: "An important caveat to these results…is that they are based on a cross-section of states with at most fifty observations. Due to this small number of degrees of freedom and the high degree of correlation among various other variables that could be potentially used as controls, our results are somewhat sensitive to which other variables are included in the regression. We have chosen this set of controls because they provide a relatively parsimonious way of capturing other economic factors that might be correlated with…our independent variables of interest. While the statistical significance of these results depends on the specification chosen, the signs and magnitudes of the effects are relatively similar across a broad set of alternative control variables. Therefore, we focus our analysis on the direction and magnitude of our estimates, and do not place too much weight on the statistical significance of any given result."

satisfy theoretical significance while focusing on the variables that are robust predictors under many different specifications.

Given the foregoing space and modeling constraints, we report only the few most important models for each of the three dependent variables. Results on *Law Enact*, *Law Type* and *Law Days* appear respectively in Tables 3 through 5. To broadly preview what follows, we find that the decision simply whether to update (*Law Enact*) follows a somewhat different empirical process than the decisions on symbolic vs. meaningful reform (*Law Type*) or the timing of the update (*Law Days*). To simply pass any law, the observed variation is driven primarily by the backlash and collective action variables. The historical number of takings, number of local governments, and value of new housing account for most of the explanatory power of *Law Enact*. But *Law Type* is empirically influenced by the economic freedom index, the flow dollar measures of new housing, and the income inequality and racial diversity measures. The timing of the update, *Law Days*, is also driven by the housing and inequality measures and, to a lesser extent, by economic freedom.

5.1. Probit Results on Law Enact

Table 3 reports results from a probit estimation including robust standard errors for five specifications. For ease of interpretation, we report the estimated marginal effects (rather than the estimated coefficients) for all results in the paper.[25] With the exception of Model 2, model fit appears to be good as shown by the pseudo-R^2 values and significant Chi^2 values. In general none of the political organization variables is significant, and neither are many of the policy variables or state characteristics. Rather, the key explanatory variables are the backlash and local governments' collective action variables. The likelihood of a state updating its law increases with a greater history of more development takings (*ED9802*), a greater value of new home construction, and more local governments per capita. To a lesser extent economic freedom also matters. However, measures of racial and income inequality, property taxes, commute time, corruption, third party voting, and density are statistically insignificant in most specifications. We now discuss key explanatory variables in turn.

[25] The marginal effects and standard errors in Table 3 are generated using the STATA *dprobit* command (StataCorp, 2005).

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Table 3: Probit Results Reporting Marginal Effects
Dependent Variable is *Law Enact*

Category	Variable	1	2	3	4	5
Backlash Variables	*Newspapers*	.108 * (.065)				
	ED9802	.022 * (.012)	.026 * (.015)	.028 * (.014)	.021 (.014)	.024 * (.014)
Policy and Policymaker Variables	*Property Tax_Local*	.018 (.015)	-.003 (.017)	.014 (.013)	.002 (.007)	.013 (.013)
	Commute Times		-.017 (.029)	-.012 (.022)		
	Corruption Rate	.001 (.005)			-.001 (.003)	-.001 (.003)
	N_Local Governments			.240*** (.092)	.222 * (.122)	.192 ** (.094)
	Economic Freedom	.007 (.008)	.017 (.011)	.013 * (.007)	.011 (.008)	.012* (.006)
State Political & Economic Characteristics	*Housing Value*	.033*** (.013)		.023** (.012)	.020 (.013)	.023 * (.012)
	FDIC Home Loans Value		.006 (.009)			
	Income Inequality	-.035** (.016)		-.014 (.019)		-.017 (.014)
	Racial Diversity		-.071 (.049)		-.032 (.042)	
	Third Party Voting	-.003 (.037)	.039 (.048)	.027 (.030)	.029 (.025)	.026 (.024)
	Land Percent Rural		.003 (.004)	.005* (.002)	.004 (.003)	.004 (.002)
Political Institutions and Organization	*Unified Government*	-.053 (.119)		-.019 (.085)		
	Legislature Size	.016 (.013)	-.001 (.031)		.007 (.010)	.007 (.011)
	Legislature Ratio	.055 (.049)				
Model Statistics	Chi^2 test statistic	36.48	13.85	23.43	15.42	20.45
	$Prob>Chi^2$	.000	.128	.015	.085	.025
	$Pseudo-R^2$	.438	.205	.400	.389	.405
	N	49	50	49	50	50

Notes: Marginal effects are calculated at sample medians. Heteroskedasticity corrected standard errors appear in parentheses. Significance levels are indicated by *** for ≥99% for ** for ≥95% and * for ≥90% confidence.

The backlash variables *Newspapers* and *ED9802* are positive and significant. Given our strong expectation that *Newspapers* is endogenous with *Law Enact*, we will avoid interpreting much from this result (Model 1 is included mainly for illustrative purposes). Although these results cannot be used to infer causation, the significance of a given variable does imply correlation. For instance, *ED9802*, our historical instrument for the incidence of development takings, is positive and significant with a sizeable marginal effect in Models 2-5. This implies that an increase of one takings case per thousand population is associated with an increase in the probability of updating by between 2.4 and 2.8 percent. This result invites a fairly straightforward interpretation: in states where development takings have been more common, property owners perceive a greater threat to their own properties and have a greater demand for legislators to *do something*, a demand which that state's legislators respond to by updating the law. A similar effect is found with the result on *Housing Permits*, which is also positive and significant. Another million dollars in authorized new home construction increases the probability of an update by an estimated 2.3 to 3.3 percent. In other (unreported) specifications using *Housing Price Index*, the *stock* value of housing is not significant. Legislatures appear responsive to interests that align with homebuilders but not necessarily with owners of existing homes. Homebuilders and realtors were vociferous opponents to *Kelo* and there is evidence that their interests were represented in the state legislative responses. The estimates on *Economic Freedom* are positive and statistically significant with comparatively small marginal effects. A one-unit increase in the economic freedom index increases the predicted probability of a new law by between an estimated 2.5 and 4.1 percent. Thus, we can conclude that states with a history of economic liberty—low taxes and spending, lenient regulation and protection of individual rights—are also more likely to update their laws after *Kelo*. Finally, *N_Local Governments* is positive and significant. This result is not surprising, since any local government that might want to maintain broad takings powers faces greater collective action costs in organizing to influence legislation when there are more local governments to organize.

The political control variables have no explanatory power. This is somewhat surprising since in much previous research these political institutions variables do influence legislation (e.g., Gilligan and Matsusaka, 2006). This "non-result" may derive from collective choice considerations discussed earlier, that efforts to limit eminent domain have been heavily grassroots. Independent of the existing political rules and reality, it may be that an electorate either agitated to limit eminent domain or it did not, seemingly regardless of the political institutions that typically affect whether legislation passes. These results do not suggest otherwise.

Other non-results may be surprising from the standpoint of theoretical significance. *Property Tax*, for example, is statistically insignificant whether using state or local taxes on either per capita or percent of revenue bases, regardless of specification and estimator (as will be apparent in Tables 4 and 5). On the one hand this non-result should be surprising. Proponents of development takings stress that eminent domain powers are critical to property values and tax base. Thus, regulators who rely more on property taxes would ex ante seem to have a strong interest in broad takings power. But this relationship fails to

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appear in the current data set. In addition, our motivation for using this variable was to proxy the degree of land-use regulation. Property taxes evidently do a poor job as such a proxy, or act as a second order effect due to aggregation or measurement errors. *Corruption Rate* and *Commute Time* also play no discernible empirical roles. Because the corruption variable measures federal arrest rates, it may reflect the state's political standing among federal policymakers, particularly in the executive branch, more than it does the state's latent political-economic realities. The actual or near sacking of dozens of US Attorneys in the spring of 2007 (Eggan and Goldstein, 2007), followed by its dramatic political fallout, strongly suggest that the fervor and priorities with which a U.S. Attorney approaches her job are closely watched at the highest levels of the federal political order. It is possible that corrupt officials are more likely to engage in economic development takings, but this does not extend to a greater likelihood of being prosecuted by U.S. attorneys operating under politicized incentives. The commuting variable might be capturing scattered information of second-order import. Commute times are the end product of many influences including natural conditions (geography and topography), citizen preferences (e.g. amenities of density versus space), variance in school quality, and policy variables like smart growth and taxes.

The finding that *Income Inequality* and *Racial Diversity* are not statistically significant (except in model 1) may also be surprising. Given that development takings usually occur in minority and low-income areas, and these populations are relatively weak in influencing the legislative process in general, we would expect states with greater inequality to be less likely to update. As we will discuss in the next section, however, inequality and diversity play more important roles once we distinguish between weak versus meaningful laws.

5.2. ORDERED PROBIT RESULTS ON LAW TYPE

As discussed above, the variable *Law Type* is a trichotomous limited-dependent variable (LDV) taking on the values {0, 1, 2} to indicate {none, weak, meaningful} updates to eminent domain restrictions. Thirty-seven states take a value greater than zero, of which 14 enacted weak or symbolic laws and 23 meaningful (see Appendix and discussion in Section 3 above). We consider two potential strategies to estimate the effect of the independent variables on *Law Type*: ordered and unordered LDV models.

Under certain assumptions, *Law Type* can be considered an ordered LDV. Specifically, we can assume that whether and what type of law passes indicate a polity's latent preference to limit takings power. The stronger the underlying desire, the higher the probability of passing a meaningful law. In more formal terms, we can assume that state i has a demand for restrictions on development takings, D_i, and that this demand takes on a linear form:

$$(1) \qquad D_i = \Omega_i\beta + \varepsilon_i,$$

where D_i is the actual amount of restrictions that the i^{th} state desires. The vector Ω_i contains the independent variables for the ith state, the vector β contains coefficients to be estimated, and ε_i is the error term. However, D_i is an unobserved (latent) variable, since the researcher only observes discrete outcomes. In the case of *Law Type*, the outcome is observed with the following decision rule:

$$(2) \qquad \begin{array}{ll} \chi_i = 1 \ (\textit{no update}) & \text{if } D_i \leq \Delta_1, \\ \chi_i = 2 \ (\textit{weak update}) & \text{if } \Delta_1 < D_i \leq \Delta_2, \text{ and} \\ \chi_i = 3 \ (\textit{meaningful update}) & \text{if } D_i > \Delta_2, \end{array}$$

where Δ_1 and Δ_2 are cutoff points to be estimated. If ε is normally distributed, equations (1) and (2) imply that the demand for eminent domain restrictions can be estimated using an ordered probit model.

On the other hand, one could assume that *Law Type* is an unordered LDV. This assumption has some appeal, especially if states pass weak laws as substitutes for strong laws. However, an unordered LDV estimation model has some drawbacks, the most important of which in the present case is that unordered LDV models are degrees-of-freedom intensive. Specifically, if J equals the number of unordered categories, an unordered LDV model estimates J–1 different β vectors, while an ordered model estimates only one β vector. Given the limitations of our data in terms of sample size, we choose to utilize the ordered model.[26]

[26] In general terms, ordered probit can be thought of as unordered (i.e. multinomial) probit assuming that the J–1 β vectors for each potential outcome are the same (sometimes called "the parallel regression assumption"). This assumption can be tested using a likelihood-ratio test (Wolfe and Gould, 1998). The results from such a test on the alternative specifications show that the parallel regression assumption holds for our sample. Although this test does not directly test for the appropriateness of an ordered versus unordered model, it does suggest that the results would be approximately the same. Indeed, unreported estimates from multinomial probit models show this to be true (available on request).

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Table 4: Ordered Probit Reporting Marginal Effects
Dependent Variable is *Law Type*
(Reporting Specification 5 from Table 3)

1	2	3	4	5
***Law Type* = 0 (no update)**	**Marginal Effect ∂pr(law type)/∂x**	**Standard Error**	**Marginal Effect + 1 std. deviation**	**95% Confidence Interval**
ED9802	-.012	.010	-.042	[-0.103, 0.020]
Property Tax_Local	-.005	.015	-.021	[-0.132, 0.090]
Corruption Rate	-.003	.005	-.036	[-0.142, 0.071]
N_Local Governments	-.166	.114	-.112 #	[-0.217, -0.068]
Economic Freedom	-.015 **	.007	-.084 #	[-0.149, -0.019]
Housing Value	-.018 **	.008	-.106 #	[-0.186, -0.026]
Income Inequality	.025 *	.013	.201	[-0.030, 0.432]
Third Party Voting	-.027	.036	-.053	[-0.175, 0.070]
Land Percent Rural	.00006	.003	.001	[-0.109, 0.111]
Legislature Size	-.005	.008	-.031	[-0.132, 0.070]
***Law Type* = 1 (symbolic)**				
ED9802	-.006	.005	-.026 #	[-0.026, -0.025]
Property Tax_Local	-.003	.007	-.012 #	[-0.012, -0.011]
Corruption Rate	-.001	.003	-.021 #	[-0.021, -0.020]
N_Local Governments	-.081	.075	-.102 #	[-0.121, -0.084]
Economic Freedom	-.007	.005	-.066 #	[-0.073, -0.058]
Housing Value	-.009 **	.004	-.094 #	[-0.101, -0.078]
Income Inequality	.012 *	.007	.022	[-0.014, 0.057]
Third Party Voting	-.013	.019	-.034 #	[-0.036, -0.033]
Land Percent Rural	.00003	.002	.0006	[-0.001, 0.001]
Legislature Size	-.002	.004	-.018 #	[-0.018, -0.017]
***Law Type* = 2 (meaningful)**				
ED9802	.019	.014	.067	[-0.034, 0.168]
Property Tax_Local	.008	.022	.033	[-0.143, 0.209]
Corruption Rate	.004	.007	.057	[-0.130, 0.243]
N_Local Governments	.248	.179	.214	[-0.061, 0.489]
Economic Freedom	.022 **	.010	.150 #	[0.014, 0.286]
Housing Value	.027 ***	.010	.200 #	[0.054, 0.346]
Income Inequality	-.037 **	.018	-.222 #	[-0.401, -0.044]
Third Party Voting	.041	.054	.087	[-0.136, 0.309]
Land Percent Rural	-.00009	.005	-.002	[-0.165, 0.162]
Legislature Size	.007	.012	.049	[-0.117, 0.216]
Chi^2 test statistic	24.89			
Prob>Chi^2	0.006			
Pseudo-R^2	0.188			
N	50			

Notes: Estimated Pr (*Law Type* = {0, 1, 2}) = {.268, .277, .455}. In Column 2 (the case of marginal effects evaluated as slopes of the normal cumulative distribution function), significance levels are indicated by *** for ≥ 99%, ** for ≥ 95%, and * for ≥ 90% confidence. In Column 4 (marginal effects evaluated as probability changes due to an increase of one standard deviation in each independent variable), # indicates that the 95% confidence interval does not include zero.

Table 4 reports ordered probit estimates on the same specification as Model 5 from Table 3. In the ordered probit we report two types of marginal effects. Columns 2 and 3 contain the estimated "instantaneous" marginal effects (the estimated slope of the probability function) and the standard errors of the predictions produced by the STATA *mfx* command (StataCorp, 2005). The values are interpreted as changes in the probability of each *Law Type* outcome given a marginal change in the independent variable evaluated at sample means. An alternative way to analyze marginal effects is to simulate changes in each independent variable. Column 4 reports changes in the probability of each outcome given a one standard deviation increase in each independent variable, and Column 5 presents bootstrapped confidence intervals for the estimates in Column 4.[27] In addition to interpreting magnitude, the other important advantage of Table 4 is the ability to isolate the influences on weak change as distinct from meaningful change. Consider, for example, the effect of economic freedom as reported by the marginal effect estimates in Column 2. *Economic Freedom* is negative and significant in the category *Law Type* = 0, not significant in category 1, and positive and significant in category 2. Thus, states with a greater history of economic liberty were more likely to enact new legislation—specifically meaningful as opposed to symbolic laws. An increase of one unit on the {1, 100} scale of *Economic Freedom* correlates with a 2.2% greater likelihood of a meaningful law. If *Economic Freedom* increases by a standard deviation, the probability of a meaningful law increases by 15%. Similar magnitude effects are shown for *Housing Value*: an additional million dollars in new homes being built correlates with a 2.7% greater probability of enacting strong restrictions on development takings and the standard deviation marginal effect is 20%. Notice that *ED9802* and *N_Local Governments* are generally not significant in Table 4, but *Income Inequality* now plays a significant explanatory role. Specifically, states with greater income inequality are more likely to enact no new law or a weak one, but they are less likely to enact meaningful restrictions.[28] The value of new housing is the most robust of our independent variables, influencing both the decision to enact and the type of law.

Overall, the ordered probit results for category 2 focus attention on three main independent variables: economic freedom, new housing dollars, and income (or

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[27] Both the estimated change and the confidence interval are bootstrapped using 1000 replications. The estimated change is the mean of the bootstrapped distribution of probability changes given a one standard deviation increase in each independent variable, and the confidence interval is based on the 2.5 and 97.5 percentiles of the bootstrapped distribution.

[28] We note that when racial diversity is included in place of income inequality (as in Table 3, Model 4), the results are largely similar.

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racial) inequality. A marginal increase in *Economic Freedom* or *Housing Value* increases the probability of a meaningful law by over two percent. By comparison, a marginal increase in *Income Inequality* decreases the chance of passing a meaningful law by 3.7 percent. None of the other independent variables are significant predictors of a meaningful law. Taken together the results in Tables 3 and 4 imply that the decision to update—to pass *any* law—responds to the intensity of the *Kelo* backlash and the ability of local governments to influence the legislature, but economic freedom and inequality of income or race matter more to whether states enact a meaningful versus symbolic reform.

Finally, although we must be careful interpreting the standard deviation marginal effects, they do show an interesting pattern. Consider the two variables that are significant in Column 4 for all three categories: *Economic Freedom* and *Housing Value*. An increase of one standard deviation in either of these variables is associated with a decrease in the probability of no update (category 0) by 8.4% and 10.6%, respectively. In category 1, the same standard deviation increases are estimated to decrease the probability of a weak law by 6.6% and 9.4%, respectively. And as mentioned the effects on category 2 are 15% and 20%. Thus, the standard deviation marginal effects suggest that categories 0 and 1 are actually more alike than categories 1 and 2, i.e., the factors that have a strong negative influence on a state not updating also negatively affect a state updating with a weak law.

5.3. DURATION ANALYSIS ON LAW DAYS

Why did some states update sooner than others? Which states can be expected to update their laws in the near future? And which states are the least likely ever to update? In our third set of estimations, we analyze the timing of states' responses to *Kelo*. Estimating the hazard function is the technique normally used to analyze the effect of time (and other covariates) on spells or durations (Kiefer, 1988). The hazard function, $\lambda(t, x)$, represents the probability of an event occurring at time t with covariates x given that the event has not yet occurred. In the present application, the event is whether a state updates its eminent domain law (either weak or meaningful) in the 2.5 years surrounding the *Kelo* decision.[29] The

[29] One obvious generalization is to model the choice of symbolic vis-à-vis meaningful as competing risks, especially given the results in Table 4 that suggest the two categories of restrictions are different. Competing risks models were estimated (available from the authors) which show that the effect of time and covariates on restriction passage is independent of *Law Type*. Thus, although there appear to be critical differences in the legislative response to *Kelo* in terms of the type of update passed, there is not a significant difference in the timing of passage for symbolic and meaningful updates.

assumption that the covariates have a multiplicative effect on the hazard rate results in the proportional hazards model first proposed by Cox (1972),

$$(3) \qquad \lambda(t, x) = h(x)\lambda_o(t),$$

where $\lambda_o(t)$ is the baseline hazard rate and $h(x)$ is a function of the covariates that takes on only positive values. The standard is to set $h(x) = e^{x\beta}$, where β is the vector of parameters to be estimated. One can then specify $\lambda_o(t)$ parametrically and estimate the effect of time on updating the law. We use the Weibull model (Greene, 2003:798), which leads to the following form for the baseline hazard:

$$(4) \qquad \lambda_o(t) = pt^{p-1},$$

where p is a shape parameter that allows for the hazard to be constant, increasing, or decreasing with time.[30] All estimates are run on the dependent variable *Law Days*, which is the number of days since January 1, 2005, until the governor's signature enacting the law, with right truncation at 926 days (or 2.5 calendar years).

Table 5 presents the estimated instantaneous and one standard deviation marginal effects for the above Weibull hazard model on Models 4 and 5 from Table 3. Recall that Model 5 substitutes *Income Inequality* for *Racial Diversity*, *ceteris paribus*. Overall both specifications exhibit good fit as shown by pseudo-R^2 and the Chi^2 test statistics. The positive estimates on p imply positive duration dependence, which means the baseline probability of an update in any period increases with passage of time—after the external shock from *Kelo*, the states' become increasingly likely with time to update equilibrium policy, ceteris paribus. A negative (positive) estimated marginal effect indicates a shorter (longer) predicted number of calendar days until updating.

[30] Alternatively, $\lambda_o(t)$ can be estimated semi-parametrically using Cox's partial likelihood estimator (Greene, 2003:799). We produced estimates using Cox's model (available from the authors), but the Weibull model showed a better fit in terms of pseudo-R^2 and the Akaike Information Criterion.

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Table 5: Weibull Hazard Model: Estimated Marginal Effects
Dependent Variable is *Law Days*
(Reporting Specifications 4 and 5 from Table 3)

	Model 4 Marginal Effects		Model 5 Marginal Effects	
	∂[Law Days]/∂x (Std. Error)	+ 1 std. deviation [95% Conf. Interval]	∂[Law Days]/∂x (Std. Error)	+ 1 std. deviation [95% Conf. Interval]
ED9802	-7.290 (9.519)	-35.38 [-191.5, 104.2]	-5.707 (8.568)	-29.86 [-173.8, 128.7]
Property Tax_Local	5.810 (10.62)	34.25 [-82.34, 257.7]	-13.93 (11.65)	-57.02 [-217.9, 96.04]
Corruption Rate	-0.576 (3.402)	-24.97 [-196.7, 121.9]	2.546 (2.900)	22.22 [-115.2, 190.5]
N_Local Governments	18.80 (30.01)	-1.40 [-163.3, 107.8]	28.01 (29.25)	2.944 [-168.7, 148.1]
Economic Freedom	-14.65 * (8.00)	-111.69 # [-341.9, -7.83]	-11.54 (8.749)	-75.18 [-248.1, 46.91]
Housing Value	-15.26 *** (5.54)	-150.3 # [-443.6, -24.4]	-13.29 *** (5.251)	-132.0 # [-345.5, -14.57]
Income Inequality	--	--	24.05 *** (8.864)	217.5 # [11.15, 600.8]
Racial Diversity	130.19 ** (55.1)	297.0 # [31.5, 858.4]	--	--
Third Party Voting	-34.92 (36.15)	-85.81 [-354.0, 61.56]	-27.35 (33.21)	-54.19 [-263.1, 136.4]
Land Percent Rural	-4.682 (3.542)	-84.82 [-263.4, 44.9]	-4.933 * (2.566)	-86.72 [-228.5, 28.63]
Legislature Size	6.984 (6.295)	78.4 [-47.4, 318.8]	0.027 (5.576)	18.92 [-111.5, 228.7]
P	3.39*** (.503)		3.33*** (.489)	
Chi^2 test statistic	39.39		35.49	
Prob>Chi^2	.0000		.0001	
Pseudo-R^2	.3026		.3281	
N	50		50	

Notes: Mean of predicted *Law Days* = 712.34. In the case of marginal effects evaluated as slopes of the hazard function in terms of *Law Days*, significance levels are indicated by *** for ≥ 99%, ** for ≥ 95%, and * for ≥ 90% confidence. In the case of marginal effects evaluated as changes in *Law Days* due to an increase of one standard deviation in each independent variable, # indicates that the 95% confidence interval does not include zero.

Consider the Model 4 estimates on *Economic Freedom*, where a one-unit increase in the {1, 100} index decreases the time of update by almost 15 days, and an increase of one-standard-deviation decreases the time by more than 111 days. In Model 5, *Economic Freedom* is not statistically significant at conventional levels, but as in the results on *Law Type*, the effect of *Housing Value* works in the same direction and with similar magnitude as *Economic Freedom* does. The instantaneous and standard deviation marginal effect estimates on *Housing Value* are approximately -15 and -150 days in Model 4, and slightly smaller in

Model 5.[31] Next, the *Income Inequality* and *Racial Diversity* variables are both negative and significant. In Model 5, the respective marginal effects are estimated to add 24 and 218 days to the date on which a state updates. A much larger magnitude is estimated for *Racial Diversity*, where a marginal increase adds 130 days and an increase of one standard deviation adds nearly 300 days.

5.4. PREDICTED VERSUS ACTUAL TIMING OF UPDATE

A further advantage of the Weibull hazard model is the ability to predict passage dates in order to examine each state's predicted timing of update. In Table 6 we list each state's actual and predicted values for *Law Enact* and *Law Days*, where the predicted values are calculated from Model 5 in Tables 3 and 5. States are sorted first on *Law Enact* and second on (Predicted – Actual) of *Law Days*.

Table 6: Actual and Predicted *Law Enact* and *Law Days*

	State Name	*Law Enact* Actual	*Law Enact* Predicted	*Law Days* Actual	*Law Days* Predicted	*Law Days* (Predicted – Actual)
1	Arizona +	0	.855	926	669	-257
2	Arkansas	0	.725	926	687	-239
3	Oklahoma	0	.735	926	687	-239
4	South Carolina +	0	.592	926	750	-176
5	Washington	0	.498	926	760	-166
6	Oregon +	0	.254	926	877	-49
7	Mississippi	0	.807	926	896	-30
8	Hawaii	0	.084	926	927	1
9	Massachusetts	0	.191	926	943	17
10	Louisiana +	0	.132	926	988	62
11	Rhode Island	0	.305	926	1001	75
12	New Jersey	0	.346	926	1005	79
13	New York	0	.048	926	1163	237
14	Montana	1	.988	867	649	-218
15	North Dakota	1	1	855	654	-201
16	Wyoming	1	.997	789	621	-168
17	Florida	1	1	496	340	-156
18	Nebraska	1	.996	529	395	-134
19	Iowa	1	.996	561	470	-91
20	Connecticut	1	.633	805	732	-73
21	Maine	1	.995	529	456	-73
22	Virginia	1	.551	854	791	-63
23	Missouri	1	.992	560	509	-51
24	Kansas	1	.999	503	519	16
25	New Hampshire	1	.999	536	553	17
26	Indiana	1	.999	448	467	19

[31] An unreported specification included *Housing Price Index*; however, the stock of housing again fails tests of statistical significance.

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27	Wisconsin	1	.914	454	481	27
28	Maryland	1	.956	858	889	31
29	North Carolina	1	.819	587	625	38
30	Minnesota	1	.975	504	563	59
31	Nevada	1	.785	874	936	62
32	Vermont	1	.997	530	595	65
33	South Dakota	1	.999	413	479	66
34	Michigan	1	.598	628	747	119
35	Alaska	1	.812	551	675	124
36	Tennessee	1	.796	515	658	143
37	Illinois	1	.713	575	727	152
38	Colorado	1	.895	522	693	171
39	Utah	1	.938	445	624	179
40	Pennsylvania	1	.817	489	681	192
41	California	1	.985	648	845	197
42	Georgia	1	.794	520	737	217
43	Texas	1	.999	244	476	232
44	New Mexico	1	.136	853	1121	268
45	Kentucky	1	.725	452	723	271
46	Idaho	1	.963	445	736	291
47	Ohio	1	.882	320	658	338
48	West Virginia	1	.874	521	874	353
49	Delaware	1	.579	202	759	557
50	Alabama	1	.395	215	806	591

Notes: States are sorted first on *Law Enact* then on (Predicted – Actual) *Law Days*. Predicted values for *Law Enact* and *Law Days* are calculated from Column 5 of Table 3 and Table 5, respectively. + indicates new law through citizen initiative or legislative referendum.

The first seven states listed in Table 6 have not updated (*Law Enact* = 0) but have a predicted date of passage sooner than the date of right truncation (926 days). In other words, these seven states should have already, but have not (yet), updated their eminent domain laws according to the hazard model specified in Table 5. Three of these top seven states enacted new laws through citizen initiatives or mixed democracy. The four other states—Arkansas, Oklahoma, Washington, and Mississippi—are "behind schedule" in the sense that our model predicts they would have updated their law already but have not. For Washington, our probit model predicts a less than 50% chance of an update. But the other three states all exceed 72% predicted probability of a new law. In short, our results predict that the most likely states to enact in the future are Arkansas, Oklahoma, and Mississippi.

Also in Table 6, six states have not (yet) updated but are predicted to do so at some date in the future. However, none of these six states can be considered to have a strong likelihood of updating with legislation. Only Rhode Island and New Jersey exceed 30% predicted probability of updating. We would predict

Hawaii, Massachusetts, and New York to have the least probability of enacting new legislation, in the near future or ever.

5.5. ROBUSTNESS CHECKS

The exercise in this paper of quantifying legislative responses has required certain judgment calls. For example, we have focused only on laws coming out of the legislative process, to the exclusion of direct and mixed democracy mechanisms. Yet as we discussed in the Introduction, citizen initiatives and legislative referenda have been used in many states. As for our categorization of weak versus meaningful reforms, the Appendix demonstrates that additional and stronger judgment calls were made. Therefore, as a robustness check on our categorizations, we estimated all of the above empirical models on alternative measures of the states' responses (defined by other researchers).

First, as an alternative to our *Law Enact*, we counted states that enacted any kind of law, including by referendum, following Somin (2008). In total, 41 states have enacted some form of new law regarding development takings. On this count, we obtained probit estimates for the specification in Model 5 of Table 3. In these results, the number of local governments is not significant. But like Table 3, states with a greater history of development takings, greater economic freedom and higher dollar values of new housing construction are more likely to enact some type of reform. It is noteworthy that the marginal effects are all insignificant. But with 41 observations in the 1 category, the marginal effects are calculated where the normal cumulative distribution function becomes very flat. Thus, our basic findings hold up to this alternative dependent variable. In fact, model fit is higher on the alternative variable (pseudo-R^2 of 0.421 compared to 0.405).

Next, we compare *Law Type* against two alternative measures of the stringency of the new laws: first, a five-category variable that quantifies to the letter grades assigned by the *50-State Report Card* published by the Institute for Justice through its Castle Coalition project (Castle Coalition, 2007), which is also used in Morriss (2008); and second, a three-category variable defined {0, 1, 2} corresponding to {No Reform, Ineffective, Effective} as determined by Somin (2008). These two alternatives incorporate the same information in much the same ways, and since the estimates we obtained on these two are very similar, we will discuss them together. Compared to the results on *Law Type* in Table 4, model fit on both alternative variables is similar. And, as in Table 4, the stringency of the new laws as measured by these alternatives is also empirically determined by economic freedom and value of new housing, which are both positive and statistically significant. But against these alternative measures, income inequality is insignificant and the number of local governments is positive and significant. The local governments variable is fairly close to being significant in Table 4,

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and racial heterogeneity is still negative and significant. Thus, with relatively minor differences, our results also remain similar when compared to these two alternative measures of stringency.

Finally, we compare results on *Law Days* to an alternative variable measuring legislative session days rather than calendar days. We first calculated the number of elapsed legislative session days from the date of the *Kelo* ruling (June 23, 2005) and either the date of enactment for the 37 states that updated or the total session days that had elapsed up to June 12, 2008 for the 13 states that did not update. Thus, the alternative dependent variable measures duration in session rather than calendar days. Our empirical model has a slightly better fit on this alternative compared to our results on *Law Days* (pseudo R^2 = .35 compared to .30 and .33). And unlike our results in Table 5, both *ED9802* and *N_Local Governments* are statistically significant, with marginal effects of -10 and -54 days respectively. In direction and significance, these findings are in agreement with the probit results in Table 3. Otherwise the model performs as our Table 5 results, with economic freedom, value of new housing, and income inequality all significant and in the same direction but somewhat smaller magnitude.

6. DISCUSSION

As expected, federalism has provided non-uniform protections against infringements takings for economic development. This paper is the first empirical study on the political-economy of the American states protecting property against development takings. Our analysis boils down to three major findings: First, many of the new eminent domain laws are more symbolic than meaningful. Second, the decision to enact *any* new law follows a different empirical process than the type of law that is passed. And third, the same model explains the timing of new legislation, from which we can distinguish states based on the predicted probability of updating in the future.

Our first major finding relates to the meaningfulness of any update. Many states chose to enact stronger restrictions than those established by the federal baseline. But some of the new state laws fail to increase protections against economic development takings. According to our qualitative analysis, 14 of the 37 new laws are largely symbolic—favoring loopholes, exemptions, and vague definitions of public use and blight, over meaningful restrictions. This should not come as a big surprise when considering that legislation is given to compromise, plus organized interests have long ago formed around the political benefits imparted by development takings. Many state legislatures confront strong incentives toward status quo politics, leaving the powers of eminent domain largely intact while voicing reassurances to an agitated populace.

From our quantitative analysis we conclude that the legislative response to *Kelo* was responsive to measures of the backlash but only in the binary decision whether to pass a new law—any new law—in order to *do something* in the face of a popular backlash. The decision to pass a meaningful law was more a function of the prevailing political-economic circumstances within each state. Three variables dominate the explanatory power in these latter results: income (or racial) inequality, economic freedom, and the value of new housing.

Critics have argued that economic development takings disproportionately harm minorities and the poor. In Section 2 we discussed rational choice reasons for expecting state legislation to do little to alleviate that bias. Our empirical findings support that. States with high racial and income inequality are less likely to enact meaningful restrictions but more likely to do nothing or pass symbolic reform and not as soon. States like New York and New Jersey, which rank in the top seven in both racial and income inequality, have not enacted new takings legislation and are unlikely in the extreme to do so. In all, state legislative processes afford fewer protections against takings in states with high percentages of poor and non-white population. As we discussed earlier, theoretical and empirical research has shown that greater racial and economic fragmentation is associated with greater redistribution through government. Restrictions on *Kelo* style takings suggest they have more in common with redistribution than with development.

States with greater economic freedom are more likely to update their takings law, with more meaningful restrictions and sooner. In states where the populace is accustomed to greater economic liberties, governments will face greater resistance to takings for non-traditional uses like tax base and job growth. In states accustomed to heavy government involvement, development takings are closer to the norm, and policymakers may perceive a greater need preserve all available policy instruments toward that end, so that opponents gain less traction in influencing legislation.

That being said, the strongest economic influence over legislative responses resides with the value of new housing. While we unfortunately do not observe direct lobbying effort levels by homebuilders, the observed market value of new housing is a strong indicator of the industry's stake in restricting development takings. As documented earlier in the paper, homebuilders and realtors opposed the *Kelo* ruling. Our results are consistent with the idea that home builder groups exert more influence in states with more ongoing home construction and that legislatures respond with meaningful restrictions and sooner. An alternative interpretation is that new home construction is an indicator of economic growth, so that states with more current growth are more likely to update eminent domain laws so as not to suppress continued growth.

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Our third major results relates to the timing of states' decisions. The duration model helps form expectations about future moves by states that had not yet updated. Five states in particular have not yet enacted legislation but are predicted by the model to do so within the reasonably near future. These five "ripest" states are Massachusetts, Oklahoma, Washington, Mississippi and Rhode Island. In contrast, Hawaii, New York and New Jersey are unlikely in the extreme ever to enact restrictions on development takings. These latter three states are among the ten most active states in takings for private-to-private transfer (Somin, 2007), and they are the only three of these ten most active states that have not updated takings reform after *Kelo*.

Appendix: **Categorization of the Dependent Variable *Law Type***

Does the law:

1. Prohibit takings for tax revenue, jobs, economic growth, or general economic health (e.g., by more narrowly defining public use)?
2. Prohibit takings for transfer to private interest, or for public use that is merely a pretext for private?
3. Restrict only for "stated public purpose" or a "recognized public use"?
4. Prohibit condemnations even for blighted properties/areas?
5. Require compensation greater than market value?
6. Require governments to incur extra procedural costs (e.g., greater public notice, more public hearings, negotiation in good faith with landowners, and approval by elected governing bodies)?
7. Give property owners right of first refusal (in the event the government does not use the property as stated upon condemnation), or impose a waiting period on transferring to private interests?
8. Define blight more narrowly (e.g., as detrimental to public health/safety)?
9. Define "public use" more narrowly?
10. Impose an expiration on blight designations?
11. Use language of *solely* or *primarily* for economic development?
12. Create temporary moratorium?
13. Create task force/study commission in lieu of statutory change?
14. Create exemption for blight, where blight is broadly defined?
15. Create exemptions for previously created redevelopment districts or urban renewal?
16. Create exemptions for removing threat to public health/safety?
17. Create exemptions for other vague reasons?
18. Create broader definition of blight?

For each criterion, answer "no" is coded 0 and "yes" is coded 1. Let S_i = $\sum$(criteria 1 through 10) – $\sum$(criteria 11-18). Then,

$$\textit{Law Type} = 1 \text{ if } \quad S_i \leq 0 \quad (N = 13);$$
$$= 2 \text{ if } \quad S_i > 0. \quad (N = 21).$$

Stated verbally, if:

Law Type = 0 then the state did not update its eminent domain law after *Kelo*;
= 1 then the state enacted weak or largely symbolic legislation;
= 2 then the state enacted meaningful restrictions on development takings power.

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