The Impact of Government Power to Expropriate on Economic Growth and Inequality

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Abstract

This paper tests theories about the potential effects of government power to expropriate: insecure property rights leading to underinvestment, moral hazard leading to overinvestment, and public use leading to economic growth. We exploit random assignment of U.S. federal judges to demonstrate a causal relationship between legal precedent making government takings easier and economic outcomes. Making physical takings easier spurs property values and economic growth, but minorities become more likely to live in public housing, less likely to be employed, and more likely to be displaced relative to whites. These effects appear attributable to subsequent unlitigated takings, displacement of commercial tenants, and public use projects stimulating growth in construction, transportation, and government as well as agriculture, retail, and financial services. Making regulatory takings easier does not affect displacement or racial inequality and spurs property values and economic growth, benefiting mainly services, government, and financial services.

Keywords: Property Rights, Takings, Regulation, Anticommons, Inequality

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1 Introduction

Eminent domain or to what extent government should have the right to expropriate land and at what compensation is a controversial question in constitutional law, development economics, and the economics of growth. The impact of greater government rights and thus lesser rights for property owners is theoretically ambiguous: on the one hand, eminent domain could aid economic growth through public goods provision, blight removal, and commercial development (Roback 1982; Collins and Shester 2011) and overcome the hold-up problem between numerous property right owners that stymie socially optimal outcomes (Buchanan and Yoon 2000); on the other, eminent domain could decrease economic growth by reducing investment incentives (Blume, Rubinfeld and Shapiro 1984; Kaplow 1986; Epstein 2008) since landowners are undercompensated (Munch 1976; Chang 2010); and revenue-seeking governments may also collude with private developers at the expense of disadvantaged groups, such as the poor and racial minorities (Carpenter and Ross 2009; Frieden and Sagalyn 1989), who may become unemployed or displaced from their homes. We cannot randomly have different laws to test the causal effects of expanding government power to expropriate property. Fortunately, in the common law system, if judges are assigned randomly, then we have something coming close to randomly varying law.

Our identification strategy rests on variation in legal precedent stemming from judges interpreting the facts and the law differently and in a manner correlated with their demographic characteristics. We use the U.S. system of appellate courts with regional jurisdiction. These courts decide the vast majority of decisions that set new legal precedent and their decisions set legal precedent for tens of millions of people. We collect all appellate takings precedent from 1950-2008. We show that our court cases are subsequently cited by state statutes and treatises inside the appellate court's regional jurisdiction but not outside. We expand and update comprehensive data on U.S. judicial biographies to implement a sparse model for estimating treatment effects with high dimensional instruments (Belloni et al. 2012, forthcoming).

We motivate our instrumental variables by first showing that, consistent with minority property owners being disproportionately condemned and undercompensated, minority Democratic appointees, who may have had experience advocating for or identifying with these groups of property owners, are 20% more likely to favor the property owner in striking down a physical taking. Meanwhile, Republican prior U.S. Attorneys, who previously advocated on behalf of the government and are typically pro-growth, are 18% more likely to favor the government in upholding a physical taking. Using this variation, we analyze zip-code level

property prices, state GDP, housing, displacement, and labor market outcomes.

We find that decisions that make it easier for governments to take property spur annual economic growth by 1.1 percentage points per year and quarterly house price growth by 0.7 percentage points per quarter on average for four years after the initial decision. This result is consistent with related studies finding that the use of eminent domain in urban renewal projects stimulated by the Housing Act of 1949 corresponded with large increases in city-level income, property values, and employment rates (Collins and Shester 2011). However, we also find that minorities become 0.3% more likely to live in public housing and 1.7% less likely to be employed relative to whites, and minority within-county moves (but not out-county moves) increase by 0.1% relative to whites. These results are consistent with sixteen studies showing that displacement, e.g., from gentrification, led to displaced persons moving within the same city, rather than to a different city. Moreover, these projects do not necessarily employ minorities whose homes or businesses were displaced by eminent domain.

Theories about the effects of eminent domain focus on two mechanisms—public projects leading to economic growth and perceived risk of takings leading to under-investment—both of which we now explore. First, using the only publicly available data on eminent domain that we are aware of—state condemnations for federally funded transportation projects—we find that after decisions that make it easier for governments to take property, states increase compensation per parcel acquired by 25%. This increase appears due to a shift in displacement from smaller residential parcels (reduction of 7%) to larger, more expensive commercial parcels (increase of 12%), whose relocation costs increase by 16%. These results suggest that at least one kind of public project, federal transportation, responds to appellate decisions and a shift from displacing residential to displacing commercial tenants, who are more litigious (Chang 2010) and expensive to displace. Second, the effect of pro-government decisions on the original zip code or zip codes where the alleged takings took place is sizeable (0.5% points per quarter) and explains up to one-third of the precedential effects. Third, the growth effects of physical takings extend beyond construction, transportation, and government to agriculture, retail, and financial services (which includes finance, insurance, rental, and estate), though it hurts the service industry.

No data on perceived risk of takings exist, so we supplement our analysis with an artefactual field experiment. Data entry workers randomly exposed to eminent domain decisions increased their perceived risk of takings by 10%, regardless of whether the decision favored the government or the landowner. No difference was observed between those exposed to pro-government decisions or pro-landowner decisions. We next identify the causal effect of

the presence of an appellate decision using the random assignment of district court judges. District court judges' error rates vary, which can lead to different rates of appeal. We find that the presence of an appeal reduces quarterly growth in house prices by 0.6 percentage points per quarter and annual economic growth by 1.0 percentage points per year. Taken together, these results suggest a large part of the growth effects of physical takings precedent are due to subsequent takings unlitigated in appellate courts.

Expropriation need not be total, but could be partial such as environmental regulation or flooding. In the U.S., this is called a regulatory taking and also influenced by physical takings precedent. Examples of regulatory takings include zoning restrictions for the location of hotels (*Dexter 345 Inc. v. Cuomo*, 2011) and regulations shortening the fishing year *(Vandevere v. Lloyd,* 2011). We construct "placebo" laws using all regulatory takings precedent from 1979-2004. We find that, in contrast to physical takings precedent, precedents making it easier for local governments to regulate does not affect displacements for federal transportation projects or racial inequality in housing, employment status, or migration. Moreover, they increase quarterly house price growth by 0.3 percentage points and annual economic growth by 0.2 percentage points. While no empirical study of regulatory takings exists to our knowledge, six neighborhoods rezoned in one of the largest U.S. place based policies experienced 12-21% increase in total employment and 8-13% increase in weekly wages, amounting to $269 million per year (Busso et al. 2013). We augment these findings by showing that regulatory takings benefit the sectors of the economy associated with white-collar work—services, government, and financial services—while hurting manufacturing and wholesale.

We conclude with a simple model of takings that embeds these two mechanisms (public projects leading to growth and perceived takings risk leading to under-investment) and unites three prominent sets of models of eminent domain to explain our results. In the first class of models, primarily from the development literature, if the government compensates too little, then insecure property rights leads to under-investment (Besley 1995; Field 2005; Hornbeck 2010; Riddiough 1997). In the second class of models, from macroeconomics, the expropriability of capital and extractive capacity of the state, can lead to faster economic growth (Aguiar and Amador 2011). In the third set of models, from law and economics, because landowners receive fair market value, which does not take into account the future probability of a takings, property owners are over-insured, which leads to over-investment (Blume, Rubinfeld and Shapiro 1984; Miceli and Segerson 1994; Innes 1997; Kaplow 1986). Our model differs from these models in that they reference investment outcomes to a second-best benchmark where takings risks are fixed and compensation varies. Because our empirical framework exploits ex-

ogenous variation in takings risk, we instead reference our results to the first-best benchmark, where the social planner varies the probability of takings. A greater risk of takings weakly decreases growth, unless the public use benefits counteract.

The remainder of the paper is structured as follows: Section II provides background on the U.S. Courts of Appeals, empirical strategy, and the data used, Section III outlines our main results and tests for violations of the assumptions underlying our research design, Section IV examines mechanisms for our results, Section V develops a model of takings, Section VI describes our results on regulatory takings, and Section VII concludes.

2 Study Design

2.1 Institutional Background

A foundational understanding of the U.S. federal courts is important to the development of our identification strategy, which relies on the law-making function of common law courts, in which judges not only apply the law but also make the law. This making of law occurs since a judge's decisions in current cases become precedent for use in decisions in future cases in the same court and in lower courts of the same jurisdiction.

Jurisdictional boundaries in the United States are geographical, and the smallest geographical subdivision is the "district." The 94 U.S. District Courts serve as the general trial court, where a jury is drawn to decide *issues of facts*. The 12 U.S. Circuit Courts encompass between 5 and 13 judicial districts each. Figure 1 displays district court boundaries in dotted lines and circuit court boundaries in solid lines. Figure 2 shows a map for the location of original takings controversies.

The role of the appellate courts is to affirm or reverse the district courts; if the district court is reversed, the district court must then make a decision in a manner consistent with the law articulated by the appellate decision. Circuit courts decide many tens of thousands of cases per year, but less than 1 case per circuit per year is related to eminent domain. Only 2% of appellate cases get appealed again to the U.S. Supreme Court, so the circuit courts determine the vast majority of decisions each year that set legal precedent.

Circuit courts decide *issues of law* (rather than facts), providing new interpretations or distinctions of pre-existing precedents or statutes. These new distinctions can expand or contract the space under which an actor is found liable (Gennaioli and Shleifer 2007). For eminent domain, in *Martino v. Santa Clara Valley Water Dist.*, 703 F.2d 1141, the Ninth Circuit held that a taking had occurred, when the local government issued an ordinance

requiring that the landowner obtain permits and establish dedications for a flood control project before the landowner could develop his land. In *Moore v. Costa Mesa*, 886 F. 2d 260, which distinguished *Martino*, the Ninth Circuit subsequently held that a conditional variance that affects only a small portion of the landowner's property is not a taking. Circuit court decisions are *binding precedent* only within that circuit. When circuits choose to adopt the precedent of another circuit, it is typically with some delay: before an opinion can be issued in the new circuit, a case bringing the same issue of law must be filed in a district court, appealed to the circuit court, and decided upon.

Court decisions are endogenous. In the case of eminent domain decisions, if property prices are expected to increase, then courts may be less likely to rule that a condemnation or regulation meets the criteria for public use such as blight removal. Estimates of the effects of pro-takings decisions would be downward biased were we to only examine the correlation between appellate decisions and future property prices. Fortunately, for causal inference, the circuit court *randomly assigns three judges* to sit as a panel out of a pool of roughly 8 to 40 judges who are appointed with life tenure and available to be assigned to each case within each circuit. Details about random assignment are provided in the appendix.

2.2 First Stage

We exploit idiosyncratic year-to-year variation in the demographic composition of judges sitting on eminent domain panels. A large literature has now documented that judges exercise judicial discretion in interpreting the facts and the law and they do so in a manner often correlated with biographical characteristics, such as party of appointment. Party of appointment does not predict decisions in eminent domain cases (Sunstein et al. 2006), however. The Republican party platform has historically been pro-growth (commercial development) and pro-individual property rights (libertarian on economic issues), while the Democratic party platform has been pro-government and pro-disadvantaged (economic inequality), and these tendencies cut in opposite directions for takings law.

Instead, *minority* Democratic appointees may be more likely to favor property owners in takings cases and Republican prior U.S. Attorneys (who represent the U.S. federal government in cases litigated in U.S. district courts and circuit courts) would be used to viewing legal issues from the government perspective. Minority judges have been found to vote differently from non-minority judges on issues where minorities are disproportionately affected, such as affirmative action, race harassment, unions, and search and seizure cases (Kastellec 2011; Scherer 2004; Chew and Kelley 2009). Figure 3 provides a graphical intuition of

our identication strategy. The smoother line (expected number of minority Democratic appointees per seat) indicates the underlying variation in judge-specific characteristics within circuits over time. The jagged line (actual number of minority Democratic appointees per seat) indicates the random year-to-year variation in minority Democratic appointees per seat. Variation across the 12 Circuits in the smooth and jagged lines can be seen as well.

We find that minority Democratic appointees are 20% more likely to strike down a physical taking while Republican prior U.S. Attorneys are 18% more likely to uphold a physical taking. Similar patterns hold at the panel, circuit-year, and circuit-quarter level. The joint F-statistic is 42 (Table 2). To check whether our linear specifications miss important aspects of the data, Figure 4 presents nonparametric local polynomial estimates of the first stage. Estimation proceeds in two steps. In the first step, we regress the proportion pro-government on circuit and year fixed effects and we regress the instrument on the same. Next, we take the residuals from these two regressions and use a nonparametric local polynomial estimator to characterize the relationship between the instrument and pro-government decisions. We use an Epanechnikov kernel with the default bandwidths selected by Stata. The relationship is increasing between Republican prior U.S. Attorney judges and pro-government decisions while it is decreasing for minority Democratic appointee judges and pro-government decisions. These figures also show the tremendous variation across circuits and years, which will be useful in estimation.

We exploit random assignment of district court judges to identify the presence of an appeal. Some district judges may be prone to error or make more extreme decisions. Correlations between district judge demographic characteristics and reversal rates has been previously documented (Haire, Songer and Lindquist 2003; Sen 2011; Steinbuch 2009). Since we have a large number of valid instruments, we use LASSO (least absolute shrinkage and selection operator) to select instruments (Belloni et al. 2012, forthcoming). LASSO is a sparse model, which solves two disadvantages of OLS. First, OLS lacks sparseness: large subsets of covariates are deemed important, resulting in too many instruments, which makes 2SLS susceptible to a weak instruments problem. Second, OLS lacks continuity: small changes in the data results in different subsets of covariates deemed important. Formally, LASSO modifies OLS by adding a data penalty for having too many large coefficients. The model minimizes the sum of squares subject to the sum of the absolute value of the coefficients being less than a constant, which tends to set some coefficients to exactly 0 and hence reduces model complexity. To construct our potential LASSO instruments, we use 30 biographical

characteristics[1] and their interactions at the judge level and panel level (for example, the number of Democrat appointees per seat multiplied by the number of black judges per seat) yielding a total of 900 possible instruments. The results and corresponding F-statistics are displayed in Table 2.

2.3 Data

2.3.1 Legal Cases

We follow the methodology established in Sunstein et al. (2006) to collect and code all physical takings precedent from 1950-2008. We selected well-known Supreme Court precedent—*Berman v. Parker*, 348 U.S. 26 (1954); *Hawaii Housing Authority v. Midkiff*, 467 U.S. 229 (1984); *Loretto v. Teleprompter Manhattan CATV Corp.*, 458 U.S. 419 (1982); *Kelo v. City of New London*, 545 U.S. 469 (2005); *Yee v. City of Escondido*, 503 U.S. 519 (1992)—and followed their subsequent citations in circuit courts. We restricted cases to those that discussed whether the government had physically invaded, was present on the property, or had taken the property. This data includes a range of decisions regarding the use of eminent domain for development, such as, a government-built dam that flooded land, sewer construction that deprived landowners of well water, and the government diversion of a river. A vote is coded as pro-landowner if the judge voted to grant the party alleging a violation of the Takings Clause any relief. Background information on takings jurisprudence is in the appendix. Appendix Table 1 provides the list and coding of cases. Figure 5 plots the quantity of eminent domain cases that were decided pro-property owner or pro-government over time. We also collect cases decided by district court judges that cite the same Supreme Court precedent. Finally, we coded each of the appellate decisions to the zip code or zip codes where the alleged taking took place.

We verify that our physical takings precedent are followed within the circuit but not outside. State officials are instructed to establish and annually update a set of guidelines based on federal and state law to assist state agencies in identifying and analyzing actions that may result in a taking (Frost and Lindquist 2010). Interviews indicate that government actors adjust their acquisitions or land use regulations to avoid exposure to costly litigation

[1]Democrat, male, male Democrat, female Republican, minority, black, Jewish, Catholic, No religion, Mainline Protestant, Evangelical, bachelor's degree (BA) received from same state of appointment, BA from a public institution, JD from a public institution, having an LLM or SJD, elevated from district court, decade of birth (1910s, 1920s, 1930s, 1940s, or 1950s), appointed when the President and Congress majority were from the same party, ABA score, above median wealth, appointed by president from an opposing party, prior federal judiciary experience, prior law professor, prior government experience, previous assistant U.S. attorney, and previous U.S. attorney.

(Department U.S. Department of Transportation 2002; Pollak 2000). On average, subsequent citations by state statutes and treatises inside the circuit are 0.9 and 0.8, respectively, and 0 and 0.3 citations outside the circuit. Differences in citations per state are substantially larger, since there are many more states outside the circuit. Citations by subsequent federal cases inside the circuit are also an order of magnitude larger than citations per circuit outside the circuit. Moreover, state citations to cases where the state lost are statutory amendments complying with the appellate precedent or, in one case, distinguishing from the appellate decision but 15 years later, which suggests that the federal appellate precedents are presumed to be influential among state courts after the decision.

2.3.2 Judicial Biographies

We compiled information on judge characteristics from the Appeals Court Attribute Data, District Court Attribute Data[2], Federal Judicial Center, and our own data collection. The final dataset includes information on vital statistics, geographic history, education, occupational history, governmental positions, military service, religion[3], race, gender, political affiliations, and other variables. We filled in missing data by searching transcripts of Congressional confirmation hearings and other official or news publications on Lexis.

We calculated expected proportion per seat of judges from different demographic backgrounds. Senior judges sit less frequently and we weigh their characteristics accordingly. In our data analysis, the average circuit-year has 17.66 judges available for assignment to panels. Judges occasionally sit from district courts or specialized courts. We drop these outside judges from our probability calculations as they are rare. In expectation, there are 0.06 minority Democratic appointees per seat, and 0.04 Republican prior U.S. Attorneys per seat (Table 1). There are 0.33 physical takings appellate cases per circuit-year; 27% of circuit-years had physical takings cases; and 1.54 physical takings district cases per circuit-year.

2.3.3 Property Prices and GDP

Property values is a well-known summary measure of outcomes that varies at the same frequency of variation as in our legal database. We use Fiserv Case-Shiller Weiss house price indices at the zip code level, from which we construct a panel of roughly 40,000 zip codes followed quarterly from 1975 to 2008. The indices are based on repeat sales data on single-

[2]http://www.cas.sc.edu/poli/juri/attributes.html

[3]Raw data from Goldman (1997) were obtained directly from the author. Sisk's data are available at http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm. Judges whose religions remained missing or unknown were coded as having no publicly known religious affiliation.

family homes. Where zip code-specific price indices are unavailable, we substitute with the price index for the next geographic level, e.g., county, then division, CBSA, or state. We obtain zip-code specific population estimates calculated for 2005 from the U.S. Census.[4] We replicate our analyses with repeat mortgage transactions handled by Fannie Mae or Freddie Mac, which comes from the Federal Housing Finance Agency, as well as with house price indices in the 20 metropolitan areas provided by S&P Case-Shiller.

We use GDP data as a limitation of data based on transactions is that the type of land being sold may changes in response to takings law. We obtain state-level yearly GDP from the Bureau of Economic Analysis.[5] The average local GDP growth of 5% is very close to the annualized average quarterly change in log price index of 1.2% in our data. Sectoral GDP is obtained from the same source. Since industry categorization changes in 1998, we drop 1998 when examining first-differences.

2.3.4 Displacement and Housing

We use the March Current Population Survey (CPS) for whether an individual moved within the county and whether an individual moved outside the county in the last year, whether an individual lives in public housing, and whether an individual lives below the poverty line.

2.3.5 Labor and Employment

We use the Merged Outgoing Rotation Groups (MORG) CPS for employment status and log real weekly earnings. The CPS provides point-in-time measures of the individual-level variables, including age, sex, race, marital status, educational attainment, and the geographic location of the individual. Earnings are normalized to account for inflation and logs of real weekly earnings are taken of 1+earnings. Earnings are set to 0 if an individual is not employed or not in the labor force; we do this because actual wages, not reservation wages, are of normative interest. We drop individuals not employed or not in the labor force to investigate effects on the intensive margin. We restrict our sample to individuals between the ages of 18 and 65 and, for both CPS datasets, weight our analysis with the CPS-provided weights.

2.3.6 Condemnations and Acquisitions

We use the only publicly available nationwide data on exercises of eminent domain that we are aware of—annual state-level statistics on real property acquisitions, condemnations, compen-

[4]The Census data documentation is located at: http://www.census.gov/prod/cen2000/doc/sf1.pdf.
[5]We aggregate across all industries by year: http://www.bea.gov/regional/gsp/default.cfm#download.

sation, and displacement expenses for 1991-2009 from the Federal Highway Administration.[6] The Uniform Relocation Assistance and Real Property Acquisition Policies Act of 1970 ("Uniform Act") and its regulations require states to report statistics related to in-state real property acquisitions by governments for all highway and transportation projects receiving federal aid.

Our data includes aggregate compensation for all parcels acquired per state-year (whether through open market purchase, condemnation, or administrative settlement) and aggregate parcels acquired per state-year. We also have displacement and relocation costs (displaced entities are eligible to receive reimbursements for moving and relocation expenses and the added costs of becoming reestablished at the new location) separately for residential and commercial tenants. Compensation, displacements (a proxy for quantity), and relocation costs (a proxy for quality) together provide an aggregate measure of government power to expropriate land.

2.4 Specification

Our structural model is a distributed lag specification:

$$Y_{ict} = \beta_0 + \sum_n \beta_{1n} Law_{c(t-n)} + \sum_n \beta_{2n} \mathbf{1}[\mathrm{M_{c(t-n)}} > 0] + \beta_3 C_c + \beta_4 T_t + \beta_5 C_c * Time + \sum_n \beta_6 W_{c(t-n)} + \beta_7 X_{ict} + \varepsilon_{ict}$$

The dependent variable, Y_{ict}, is a measure of outcomes of state (or zip-code or individual) i in circuit c and year (or quarter) t. Law_{ct} is the proportion of appellate cases with a pro-government outcome when there is a case but 0 when there are no cases. We also control for $\mathbf{1}[M_{ct} > 0]$, the presence of an appeal (M_{ct} is the number of eminent domain cases).

In interpreting the coefficients from our estimates, Law_{ct} is typically 1 (100% pro-government) or 0 (100% pro-property owner) since most circuit-years have 1 or 0 cases. Hence Law_{ct} is the parameter of interest for a judge choosing to decide pro-government or pro-property owner, conditional on the case being in front of the judge. When $M_{ct} > 1$, the effect of pro-government and pro-property owner decisions are diluted. When we use M_{ct} as weights to account for the number of decisions occurring in a circuit-year, the estimates become more statistically significant.

When $M_{ct} = 1$, the sum of coefficients on Law_{ct} and M_{ct} describes the effect of legal precedent including the effect of the appeal itself, i.e. when the counterfactual is no precedent. Multiplying the coefficient on Law_{ct} by $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$, the typical proportion of decisions that are pro-government when there are appellate takings cases, and by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$,

[6] http://www.fhwa.dot.gov/realestate/rowstats/index.cfm.

the proportion of circuit-years with an appellate takings case, results in the effect of pro-government precedent in a typical circuit-year.

We test robustness to controls, such as: circuit fixed effects, C_c; time fixed effects, T_t; circuit-specific time trends, $C_c * Time$; state fixed effects; a vector of observable unit characteristics, X_{ict} (for example, at the individual level: age, gender, educational attainment, and race); and time-varying circuit-level controls, W_{ct}, such as the expected proportion of minority Democratic appointees per seat.

Economic changes may be misattributed to one legal rule when many legal rules are changing simultaneously and social trends may drive both the decision to appeal and the appellate decision itself. To identify the causal effects of takings law, we exploit variation in Law_{ct} and $\mathbf{1}[M_{ct} > 0]$ that arises from random deviation in the composition of judges assigned to eminent domain cases. Our instrument for Law_{ct} is the proportion of judges who are minority Democratic appointees in appellate takings cases and 0 when there are no cases:

$$p_{ct} = \begin{cases} N_{ct}/M_{ct} & \text{if } \mathbf{1}[\mathrm{M_{ct}} > 0] = 1 \\ 0 & \text{if } \mathbf{1}[\mathrm{M_{ct}} > 0] = 0 \end{cases}$$

where N_{ct} is the number of minority Democratic appointees per seat in takings cases in circuit c and year t. The jagged line in Figure 2 displays N_{ct}/M_{ct} and the smooth line displays $\mathbf{E}(N_{ct}/M_{ct})$. Note we do not need to include $\mathbf{E}(p_{ct})$ because $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}[\mathbf{E}(p_{ct})\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}(p_{ct})\mathbf{E}(\varepsilon_{ict}) = \mathbf{E}[p_{ct}\varepsilon_{ict}]$. Our instruments for $\mathbf{1}[M_{ct} > 0]$ are similarly constructed.

We use a distributed lag specification that includes up to four years (16 quarters) of lags and four years (4 quarters) of leads ($n = -4$ to 4) to allow for laws not being immediately capitalized in prices and agents needing time to adjust to judicial decisions. In principle, we have 408 (1,632) experiments (34 years x 12 circuits (x 4 quarters)). We robust cluster standard errors at the circuit level to address serial correlation of ε_{ict} and heteroskedasticity. We also execute a wild bootstrap (Cameron, Miller and Gelbach 2008) to address the small number of clusters. When we examine racial inequality, we interact Law_{ct}, $\mathbf{1}[M_{ct} > 0]$, and their respective instruments with race in the CPS.

Appellate eminent domain decisions that affirm or overturn a local taking of private property rights potentially have direct effects separate from precedential effects. We estimate $Y_{ict} = \beta_0 + \beta_1 Law_{ct} + \beta_2 LocalLaw_{ict} + \varepsilon_{ict}$, where we separately instrument for Law_{ct} and $LocalLaw_{ict}$ using the random assignment of judges in cases that occur in the zip code locally and in cases that occur in the circuit. We apply this specification only to property price data

since our other datasets are not available at the zip code level.

2.5 Discussion

We make four remarks about our instrumental variables specification. First, if the identity of judges on eminent domain panels only affects economic outcomes through legal precedent alone, the exclusion restriction holds. Second, our estimates are internally valid conditional on everything that happened before the judges got assigned to the case. Third, if monotonicity assumptions hold, then the estimates have a LATE interpretation. The IV estimates would rely on variation in cases that lack strong legal precedent, where judicial characteristics may matter for case outcomes. These are the types of cases judges might seek empirical guidance. Fourth, an advantage of examining the effects of precedent is that if we examined the effects of eminent domain, even if acts of eminent domain were randomized, we would only capture partial equilibrium effects; our empirical framework captures at an aggregate level all the possible responses, such as factor mobility, to actual and potential exercises eminent domain.

3 The Impact of Eminent Domain

3.1 Property Prices and GDP

Making it easier for the government to expropriate property leads to an increase in house price growth of 0.7% points per quarter. Table 3 displays three sets of estimates: OLS, 2SLS using only the appellate IV, and 2SLS using both appellate and district IV. The two 2SLS estimates are similar, so our discussion focuses on the last set of estimates. Panel A displays the average lag effects. Panel B displays a falsification check: the lead effects. Figure 6 plots the dynamic response to takings decisions. No effects are found before the decision. The lags do not sum to 0, suggesting that the effects of physical takings precedent persist, i.e., they look more like growth effects rather than level effects.

To help put the estimate in perspective, a recent RCT found that houses along unpaved paths that were randomly assigned to be paved experienced a 16% increase in appraised property values (Gonzalez-Navarro and Quintana-Domeque 2011), which is equivalent to roughly the cumulative effects of a takings decision over five years. Moreover, five extra years of legislation enabling government acquisitions was associated with 4% higher median property values and $100 per capita spending in Housing Act of 1949 grant funding was associated with a 7.7% difference in median property value 31 years later (Collins and Shester 2011).

The average effect of pro-government takings decisions is 1.1% points per year in GDP growth. Comparing across columns in Table 3 shows that IV estimates are larger than OLS estimates in the lag estimates (Panel A). The IV estimates are not necessarily larger than the OLS estimates for the lead estimates (Panel B). Appendix Table 3 shows that wild bootstrap does not change the inferences on the point estimates. In unreported results, the results are similar when we control for lagged dependant variables, use LASSO-selected instruments at the appellate level, and collapse the data by using population-weighted averages within the circuit-year. The magnitude of the effects are similar when we use repeat mortgage transactions and restrict our analysis to metropolitan areas provided by S&P Case-Shiller.

3.2 Displacement and Housing

Pro-government physical takings precedent increase within-county moves of non-whites by 0.1% more than they do of whites (Table 4). This difference is statistically significant at the 1% level. Non-whites are no more likely to make moves from outside the county than whites after pro-government takings decisions. These results are consistent with studies showing that displacement, e.g., from gentrification, led to displaced persons moving within the same city, rather than to a different city (LeGates and Hartman 1982). Whites were 0.2% more likely to make within-county moves and 0.007% more likely to make out-county moves after pro-government takings decisions. No effect is found before the decision (Panel B).

Non-whites are also 0.3% more likely to live in public housing than whites after pro-takings decisions (Table 5). No effect is found for whites. Whites are 0.1% more likely to live below the poverty line; non-whites are an additional 0.6% more likely to live below the poverty line than whites, though this difference is not statistically significant in Column 3, it is with LASSO-selected instruments at the appellate level.

3.3 Employment

Non-whites are 1.7% less likely to be employed than whites after pro-government takings decisions (Table 6). Whites are 1.0% more likely to be employed. The overall population is also more likely to be employed, consistent with the economic growth effects found earlier. To calculate, multiply the non-interacted average lag effect by 0.78, the proportion of the population that is white, and add the interaction effect multiplied by 0.22, the proportion of the population that is non-white.

Log real weekly earnings of non-whites are 11.6% lower than those of whites after

pro-government takings decisions. These results suggest that even if public use projects spur economic growth, they do not necessarily employ minorities in the same numbers as those whose homes or businesses are displaced by eminent domain. For example, in *Poletown Neighborhood Council v. City of Detroit*, General Motors was awarded property under the takings clause. Afterward, however, GM employed fewer people than the combined number of small businesses that it displaced (Somin 2004).

4 Mechanisms

Theories about the effects of eminent domain focus on two mechanisms—public projects leading to economic growth and perceived risk of takings leading to under-investment—both of which we now explore.

4.1 Public Projects

4.1.1 Condemnations

We begin our analysis of mechanisms by examining the local government response to appellate takings decisions. If courts are more likely to uphold a taking, government actors may either acquire more parcels, provide less compensation per parcel because of increased bargaining power, take different types of property, or all three. In our dataset of state condemnations for federally funded transportation projects, we find that after pro-government decisions, states increase compensation to property owners by 12.5% per year and reduce parcels acquired by 10.3% per year. This increase in compensation per parcel appears to be due to a shift in composition of parcels acquired. 6.5% fewer residential tenants are displaced but 12.2% more commercial tenants are displaced and their relocation costs are 16.3% higher (Table 7). No lead effects are found. These results suggest that governments do respond appellate takings decisions for at least one important set of public use projects.

4.1.2 Local Impacts

Another way to examine the public use channel for the effects of government power of eminent domain is to measure the response to appellate takings decisions at the location where the alleged takings occurred. The local effects are a sizeable 0.5 percentage point increase in quarterly house price growth (Table 8 Panel A); moreover, comparing specification 1 and 2

in Panel A suggests that the local effects explain up to one-third of the precedential effects, which fall from 1.0 to 0.7%.

Table 8 also displays several robustness checks where we show the distributed lag is robust to the inclusion of circuit-specific time trends, removing fixed effects, adding time-varying characteristics of the circuit pool of judges available for assignment, using population weights, dropping one circuit at a time, or varying the lag structure (Panel B). The point estimates of the individual lags are robust to the number of lags and leads, and, the standard errors on the leads specification (Panel C) are similar to those in the lag specification while the point estimates are small.

4.1.3 Sectoral Impacts

In terms of sectoral impacts, pro-government physical takings decisions spur annual growth in government by 0.3% points, transportation and utilities by 1.4% points, and construction by 3.9% points (Table 11 Panel C). They also spur annual growth in agriculture by 5.7% points, retail by 1.7% points, and finance, insurance, rental, and estate by 2.2% points. Growth in the service sector is adversely affected by 9.2% points. Leads are not significant. Broad-based growth across sectors is consistent with our analyses indicating similarly sized impacts for metropolitan areas vs. all areas more generally.

4.2 Perceived Takings Risk

4.2.1 Experiment

We assess whether perceived takings risk responds to eminent domain decisions. Newspaper accounts of prominent decisions, such as *Kelo v. City of New London*, made ordinary citizens feel vulnerable (Nadler et al. 2007). We conduct an experiment in which we randomly assign subjects to be exposed to newspaper reports of eminent domain decisions. Data on expectations can be used to validate assumptions about individuals' perceptions (Manski 2004).

We hired 266 workers to do data entry. All workers completed 3 paragraphs involving Tagalog translations of Adam Smith's *The Wealth of Nations*. After completing the lock-in task, workers in each of 4 treatment groups and 1 control group were asked to transcribe abbreviated newspaper summaries of an eminent domain decision that was either a regulatory or physical takings decision and either a pro-government or pro-landowner outcome; the control group proceeded immediately to the perceived takings risk question:

"What do you think is the probability that the government will deny you the

> right to use your property (land or house or any other physical property) in a way that you want? Provide a number from 0-100. A higher number indicates more certainty that the government will deny you your right."

We paid subjects 10 cents to complete each paragraph (a paragraph takes about 100 seconds to enter so the offered payment is equivalent to $86.40 per day). The web appendix provides the exact paragraphs[7] and the methodology is provided in more detail elsewhere (Chen and Yeh 2012; Chen and Horton 2009).

Data entry workers randomly exposed to any eminent domain decision increased their self-reported takings risk by 10% relative to the control group that was not exposed to eminent domain decisions. Figure 7 displays the distribution of responses for the control group and the treatment groups. Both OLS regressions and Wilcoxon-Mann-Whitney test for differences in distributions indicate that the effect is statistically significant at the 5% level. No significant difference was observed between pro-landowner and pro-government decisions.

4.2.2 Presence of Takings Appeals

We next identify the causal effect of the presence of an appellate decision. Table 9 Column 3 instruments the presence of an appeal with the demographic composition of judges randomly assigned to the district court cases. The presence of an appeal reduces quarterly growth in house prices by 0.6 percentage points per quarter and annual economic growth by 1.0 percentage points per year. These results suggest that a large part of the effect of an appellate precedent is through the effect of the precedent (pro-government vs. pro-landowner) rather than the effect of the presence of an appeal, which appear to drive perceived takings risks and negative growth effects. No lead effects are found.

5 Impacts of Regulatory Takings Precedent

Expropriation need not be total, but could be partial such as environmental regulation or flooding. In the U.S., this is called a regulatory taking and also influenced by physical takings precedent. We construct "placebo" laws using data on all appellate regulatory takings published decisions from 1979-2004 (Sunstein et al., 2006). Appellate regulatory takings cases were identified by tracking the citations of the following landmark Supreme Court decisions: *Lucas v. South Carolina Coastal Council*, 505 U.S. 1003 (1992); *Nollan v. California Coastal*

[7]Original newspaper articles are available on request.

Commission, 483 U.S. 825 (1987); *Keystone Bituminous Coal Ass'n v. DeBenedictis*, 480 U.S. 470 (1987); and *Penn Central Transportation Co. v. New York City*, 438 U.S. 104 (1978). This data includes a range of regulatory takings decisions regarding zoning restrictions on hotels and on gambling, noise regulations requiring enclosures on car racing facilities, and environmental regulations shortening the fishing year.

On average there are 0.71 regulatory takings panels per circuit-year for a total of 220 cases (Table 1 Panel B); 46% of circuit-years had regulatory takings panels (Appendix Table 4). In district courts, 498 regulatory takings cases from 1979-2004 cited the landmark regulatory takings precedents. Our appellate regulatory takings cases receive 0.7 citations by state statutes and 1.1 citations by treatises inside the circuit but only receive 0.03 citations by state statutes and 0.3 citations by treatises outside the circuit.

Precedents making it easier for local governments to regulate increase quarterly growth in house prices by 0.3 percentage points per quarter and annual economic growth by 0.2 percentage points per year (Table 10 Panel A). Moreover, in contrast to physical takings precedent, precedents making it easier for local governments to regulate have no effects on racial inequality in living in public housing, living below the poverty line, employment status, or within-county moves. Nor do they affect displacements for federal transportation projects (Panel B). Panel C reports the first stage F-statistic for the respective appellate and district-level IVs (listed in Appendix Table 5).

The proportion of circuit-years with cases occurs with different frequency for physical and regulatory takings precedent. To aid comparison, we calculate the effects in a typical circuit-year in Table 11 Panels A and B. The typical effects of physical takings precedent are 17% and regulatory takings precedent are 36% of the coefficients from our estimation.

Growth effects of regulatory takings precedent appear concentrated in industries associated with white collar work. Annual growth increases in services by 3.8% points, government by 0.04% points, and finance, insurance, rental, estate by 0.3% points. Annual growth declines in manufacturing by 0.9% points and wholesale by 0.6% points (Table 11 Panel C). No significant effects are found for construction or transportation and utilities. While no empirical study of regulatory takings exists to our knowledge, our results build on a recent study that finds six neighborhoods rezoned in one of the largest U.S. place based policies experienced 12-21% increase in total employment and 8-13% increase in weekly wages, amounting to $269 million per year (Busso et al. 2013).

6 Model

We conclude with a simple model of takings that embeds the two mechanisms (public projects leading to growth and perceived takings risk leading to under-investment) and unites three prominent sets of models of eminent domain to explain our results. In the first class of models, primarily from the development literature, if the government compensates too little, then insecure property rights leads to under-investment (Besley 1995; Field 2005; Hornbeck 2010; Riddiough 1997). In the second class of models, from macroeconomics, the expropriability of capital and extractive capacity of the state, can lead to faster economic growth (Aguiar and Amador 2011). In the third set of models, from law and economics, because landowners receive fair market value, which does not take into account the future probability of a takings, property owners are over-insured, which leads to over-investment (Blume, Rubinfeld and Shapiro 1984; Miceli and Segerson 1994; Innes 1997; Kaplow 1986).

6.1 Background

Through *stare decisis*, the legal doctrine by which judges must respect the precedents established by prior decisions, appellate court decisions affect the subsequent probability that a court allows the taking of a property right, and thus the likelihood of government actors initiating a taking. As a conceptual framework, we model a government actor proceeding with a taking if its expected net gain is above zero:

$$NB = \pi_p B_p + \pi_r B_r - \pi_p(TC + C) - FC \geq 0 \tag{1}$$

B_p and B_r are the exogenous benefits due to government action from a physical taking and regulation, respectively. π_p is the probability that the court allows a physical taking to occur and π_r is the probability that the court allows the regulation to occur; both probabilities are exogenous in our empirical implementation due to the random assignment of judges. In this stylized setting, the government actor is making a decision to take a physical property right (and provide compensation $C > 0$) or to limit a property right through uncompensated regulation ($C = 0$).

With physical condemnation, the government must bring an *in rem* action, so court fees accompany every physical taking. A regulation instead places the burden on the property owner to seek redress. Governments choose to litigate or regulate (Shleifer 2010) and, as major doctrinal developments and actual cases in our database indicate, the boundary between physical and regulatory takings is blurry. For example, the local government can build a beach

protection, which could constitute a physical taking, or require landowners to build a beach protection, which would be considered a regulation. Takings cost TC represents the additional cost of a physical taking over a regulation. FC is the fixed cost of planning.

For physical takings, if the court finds for the landowner, no benefits or additional costs exist. For regulations, if courts find for the landowner and hold the regulation to be a taking, compensation is required.[8] We assume that C for regulatory takings is negligible relative to B_r, since only a small fraction of property owners would seek redress and only a handful of land parcels whose productive uses are completely regulated out of existence would require compensation.

Because court decisions shape precedent, our empirical framework provides exogenous variation in π_p and π_r. π_p and π_r are increased with pro-government decisions, which lower the threshold for what constitutes public use. The Fifth Amendment of the U.S. Constitution allows governments to take land only for "public use" and only if there is "just compensation," but the vast majority of decisions in our data focus on whether there is public use to justify the takings and do not address just compensation. This also means in our welfare analysis, we use the first-best as the benchmark, where the social planner can vary the probability of takings, rather than the second-best, where the social planner can only vary the level of compensation. For simplicity, a single measure of benefit from government projects captures dynamic consequences of government takings (e.g. externalities, loss of tax revenue, etc.).

6.2 Landowner Investment

We now evaluate how takings law affects the landowner's investment incentives and investigate when investment differs from the first and second best benchmarks. We assume risk neutrality. Initially, we ignore any direct impact of public use on growth in order to isolate the channel through which eminent domain has its effects; we also assume that public use projects do not directly affect the marginal return on investment. Ignoring the public use channel, we show that making it easier for the government to take will weakly lead to lower growth.

The landowner invests I in her property to achieve $V(I)$, the return from investment. Compensation C is a function of investment and government policy, G, so $C = C(G, I)$.[9] Com-

[8]Courts are reluctant to simply reverse a regulation. Invalidation of a regulatory ordinance without payment of fair value for the use of the property during the period of the taking is considered a constitutionally insufficient remedy. *First English Evangelical Lutheran Church of Glendale v. County of Los Angeles*, 482 U.S. 304 (1987).

[9]The law requires the government to pay the landowner, taking into account a number of factors including book value (appraisal price of the property). Factors include market demand; proximity to areas already developed in a compatible manner with the intended use; economic development in the area; specific plans

pensation increases with investment, but at a decreasing rate: $C_I(G,I) > 0$ and $C_{II}(G,I) < 0$.

First best optimal investment occurs when marginal benefits equal marginal costs:

$$\max_I V(I) - I \quad \text{i.e.,} \quad V'(I) = 1 \tag{2}$$

Government benefit and costs of takings are constant with respect to I, so they drop out. Second best optimal investment (i.e., ignoring compensation, which is just a transfer) is achieved at:

$$\max_I (1 - \pi_p - \pi_r)V(I) + \pi_r(V(I) - L) - I \tag{3}$$

i.e.,

$$V'(I) = \frac{1}{(1 - \pi_p)} > 1$$

where L is the loss of investment value due to a regulation. With diminishing returns, $V''(I) < 0$, the second best investment level is below the first best investment level. The intuition is simply that a physical taking deprives all value from the original investment, making landowners less willing to invest. Total loss of $V(I)$ in a physical taking is not necessary to the result, nor is L required to be a fixed loss as opposed to a loss in proportional share. Any loss would mean that regulations make landowners less inclined to invest.

The landowner takes compensation into account and maximizes the expected return, ER:

$$\max_I ER = \max_I \{(1 - \pi_p - \pi_r)V(I) + \pi_p C(G,I) + \pi_r(V(I) - L) - I\} \tag{4}$$

The landowner's optimal investment is achieved when:

$$(1 - \pi_p)V'(I) + \pi_p C_I(G,I) = 1$$

so that

$$V'(I) = \frac{1 - \pi_p C_I(G,I)}{1 - \pi_p} < \frac{1}{1 - \pi_p} \tag{5}$$

Since litigants in regulatory takings cases would pursue a win only if their compensation

of businesses and individuals; actions already taken to develop land for that use; scarcity of land for that use; negotiations with buyers; absence of offers to buy property; and the use of the property at the time of the taking. (60 Am. Jur. Trials 447). The last factor in particular is likely to increase with landowner investment.

exceeds $V(I) - L$, we assume that litigants receive $V(I)$ in the event of a pro-landowner regulatory takings decision.

Equation 5 indicates that the landowner always over-invests compared to the second best optimal investment. The only way to eliminate over-investment is to set $C_I(G, I) = 0$, which is completely contrary to the doctrine of "just compensation." This prediction is in line with law and economics models (Blume et al. 1984; Kaplow 1986). However, these models *assume the (positive) probability of taking as fixed* and therefore use the second best as benchmark. With the exogenous variation in takings risk in our empirical framework, we use the first best as benchmark. From the equations above, we can see that, compared to the first best, "just compensation," $C(G, I) = V(I)$, results in optimal investment.

6.3 Perceived Takings Risk

The landowner perceives the probability π of government action, so the landowner's expected return is:

$$\max_I ER = \max_I \{(1-\pi)(V(I) - I) + \pi[(1-\pi_p)V(I) + \pi_p C(G, I) - \pi_r L - I]\} \tag{6}$$

With the additional uncertainty of government action, the landowner's optimal investment is achieved at:

$$V'(I) - 1 - \pi\pi_p V'(I) + \pi\pi_p C_I(G, I) = 0 \text{ so that } \quad V'(I) = \frac{1 - \pi\pi_p C_I(G, I)}{1 - \pi\pi_p} \tag{7}$$

and we can see that landowners still overinvest relative to the second-best benchmark. Taking the total derivative of Equation 7 gives:

$$dI = \frac{V'(I) - C_I(G, I)}{(1 - \pi\pi_p)V''(I) + \pi\pi_p C_{II}(G, I)}(\pi_p d\pi + \pi d\pi_p) \tag{8}$$

With "just compensation,"$C_I(G, I) = 1$, it follows that $\dfrac{dI}{d\pi} = \dfrac{dI}{d\pi_p} = 0$, so investment, property prices, GDP, and employment *should each be independent of the probability of a taking.* Any differences in these outcomes would be due solely to the impacts of public use projects.

Both over- and under-investment relative to first best can occur depending on whether $C_I(G, I)$ is, respectively, bigger or smaller than 1. For example, if $C_I(G, I) < 1 < V'(I)$,

then $\frac{dI}{d\pi}$ and $\frac{dI}{d\pi_p} < 0$ because $V''(I) < 0$ and $C_{II}(G, I) < 0$. Decisions making it easier for the government to take (which increases the probability that subsequent courts rule in favor of the government and possibly the perceived probability of government action) lead to under-investment only if there is under-compensation. Over-investment relative to first best occurs only if there is over-compensation, i.e., $C_I(G, I) > 1$.

In the U.S. context, under-compensation is the presumption (Radin 1982; Fennell 2004) and especially among minority landowners. A large literature, as well as Justice Clarence Thomas's *Kelo* dissent, documents that minority landowners are disproportionately expropriated, displaced, and receive less compensation (Carpenter and Ross 2009; Frieden and Sagalyn 1989). Comparing to the first best benchmark, insecure property rights lead to under-investment, consistent with work in eceonomic devleopment (Besley 1995; Field 2005; Hornbeck 2010; Riddiough 1997). We should expect adverse outcomes for minorities. Regulations, which can lead to uncompensated losses in property rights, may also have smaller effects on prices and growth.

Because of the investment distortions that arise from under-compensation, making it easier for the government to exercise *eminent domain stimulates economic growth only if the social benefits exceed the distortions from the increased risk of taking.* Social benefits (B_r and B_p) from public use projects could be capitalized and directly impact prices, growth, and employment. Higher benefits, B_p and B_r, or lower costs, TC and FC, increase the government's probability of initiating a taking, π.

7 Conclusion

Is state taking of private property rights justified? From John Locke to Jeremy Waldron, economists and philosophers have long inquired whether a society that fails to protect property rights against legislative restriction also fails to support the rule of law. Deadly riots in India and China have followed government takings of land on behalf of commercial developers,[10] and in the former Soviet bloc, legislation allowing governments to take land for the establishment of privately-owned industrial parks is pending. Different legal systems refer to government takings as eminent domain, compulsory purchase, compulsory acquisition, or

[10]In China alone, the government has taken land from an estimated 40 million households, many of whom have been under-compensated and as a result remain landless, unemployed, and politically restless (Cao et al. 2008). See http://www.nytimes.com/2011/02/23/world/asia/23india.html, and http://www.nytimes.com/2011/12/26/world/asia/in-china-the-wukan-revolt-could-be-a-harbinger.html?hp. The large number of displacements is at least partly due to the lack of a market for housing.

expropriation.[11] In the U.S., the *Charles River Bridge* case of 1837 represents a watershed moment. There, the Massachusetts government revoked exclusive property rights originally granted to private investors to bridge traffic across a river by building a free bridge nearby, touching off a dispute in which each side claimed to generate the socially optimal outcome (Lamoreaux 2011). Liberty issues aside, little is empirically known about the consequences of government takings, despite a large theoretical literature regarding their potential consequences (Blume, Rubinfeld and Shapiro 1984; Kaplow 1986; Epstein 2008).

We find that rulings making it easier to take physical property rights spur economic growth and property values, but increase racial inequality as minorities become less likely to be employed, live above the poverty line, or live outside public housing. We explore several mechanisms for these results. First, states displace larger and more expensive commercial tenants for federal transportation projects. Second, property values in the local zip code or zip codes where the original takings occurred also increases. Third, the economic growth impacts are concentrated in construction, transportation and utilities, and government, as one would expect with physical takings, but also in agriculture, retail, and finance, insurance, rental, and estate. Fourth, the presence of a decision increases perceived takings risk and reduces growth in house prices and GDP. Taken together, these results suggest that the effects are attributable to subsequent unlitigated (in appellate courts) public projects responding to appellate precedent making it easier for government actors to take physical property rights.

In contrast to physical takings precedent, decisions making it easier to regulate without having to compensate affects neither displacements for federal transportation projects nor racial inequality in employment status, living above the poverty line, or living in public housing. Economic growth effects are smaller and concentrated in services, government, and finance, insurance, rental, and estate. We conclude with a simple model of takings uniting several sets of models of eminent domain. Our model suggests that making it easier for the government to take property rights, whether compensated or not, almost always leads to lower economic growth because of distortion in investment incentives, unless the public use channel dominates.

We hope the use of randomization of federal appellate *and* district court judges can be a tool for judges facing hard cases with no strong legal precedent. We cannot ask judges to conduct prospective evaluations by randomizing decisions, so judges typically rely on policy arguments, not formal models and empirical evidence on the effects of their decisions, which would suffer from omitted variables and reverse causality. Our robustness checks explore

[11] Eminent domain (United States), compulsory purchase (United Kingdom, New Zealand, Ireland), resumption/compulsory acquisition (Australia), expropriation (South Africa and Canada).

whether the empirical framework addresses common concerns about the use of randomization in the field. We investigate whether the empirical framework provides causal estimates of court precedent holding all else equal including unobserved factors, overcoming the issues of omitted variables and reverse causality, by varying covariates and lag structure. We also consider whether the exclusion restriction is likely to hold, whether the LATE interpretation of IV estimates is policy relevant, and whether the general equilibrium effects are those that we would want to include. Moreover, we show how data collection from both appellate and district courts allows estimating two separate parameters of policy interest, one where the counterfactual is the opposite precedent, conditional on a case being in front of the judge who must make a decision, and one where the counterfactual is no precedent, which may inform debates about the role of court decisions in economic and social change.

A limitation of our approach is that we do not capture the effects of eminent domain projects that stimulate trade and growth in multiple circuits. Nor can our estimates distinguish between government actors deferring planned eminent domain acts until a favorable legal regime comes into existence from legal regimes spurring new exercises of eminent domain. We leave for future research whether changing the bargaining procedure for minority-owned land would affect eminent domain's disparate impact.

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A Randomization

Appellate courts indicate that the assignment of judges to panels is random. In some courts, two to three weeks before the oral argument, a computer program is used to randomly assign available judges to panels that will hear cases. In other courts, panels of judges are set up to hear cases on a yearly basis, randomly assigned together by computer program and given dates for hearings; as cases arise, they are randomly assigned to panels. Judges are revealed very late, after litigants file their briefs, sometimes only a few days before the hearing, if there is a hearing, giving little opportunity for settlement upon hearing the identity of the panel. Chen and Sethi (2011) checks that case characteristics as determined by the lower courts are orthogonal to the appellate instrument.

We assess deviations from random assignment by examining whether the sequence of proportions of judges is like a random process. Figure 2 suggests visually that panel composition is not serially correlated. Formally, we:

1. Propose a statistic that can be computed from the sequence of numbers of minority Democratic appointees per seat within a circuit.

2. Compute the statistic for the actual sequence, s^*.

3. Compute the statistic for each of 1,000 bootstrap samples from the actual sequence, i.e., s_1, s_2, s_3 . . . s_n. Since there were changes in the expected number of minority Democratic appointees per seat over time, we treat our bootstrap samples as a vector of realized random variables, with the probability based on the expectation during the circuit-year.

4. Compute the empirical p-value, p_i by determining where s^* fits into s_1, s_2, s_3 . . . s_n.

5. Repeat steps 1-4 and calculate p_i for each unit.

We use the following statistics:

Autocorrelation: We see if the value in the j^{th} case depends on the outcome in the j-1^{th}case. This statistic can detect whether judicial assignments are "clustered," meaning a higher than expected number of back-to-back high proportion of seat assignments to a particular type of judge. This test tells us whether certain judges sought out eminent domain cases, perhaps in sequence.

Mean-Reversion: We test whether there is any form of mean reversion in the sequence, meaning that the assignment in the n^{th} case is correlated with the assignment in previous $n-1$ cases. This test tells us whether judges or their assignors were attempting to equilibrate their presence, considering whether a judge was "due" for an eminent domain case.

Longest-Run: We test whether there are abnormally long "runs" of certain types of judges per seat. This test tells us whether certain circuits may have assigned certain judges with eminent domain cases during certain time periods, for example, to achieve specialization.

With a truly random process, the collection of all unit p-values should be uniformly distributed. (Imagine that you generate summary statistics for 1000 random strings. The 1001^{th} random string should have a summary statistic that is equally likely to be anywhere from 1 to 1000.) A visual examination suggests that the empirical distributions for our p-values for physical and regulatory takings approach the CDF of a uniform distribution (Appendix Figure 1), which we formally test using a one-sided Kolmogorov-Smirnov Test (Appendix Table 2 Panel A).

A final check of randomization is displayed in Appendix Table 2 Panel B. One or two years before the true instrument, judicial decision-making is not correlated with future judicial assignment.

B Eminent Domain Doctrine

Major developments in appellate takings doctrine interpret the Takings Clause of the 5th Amendment in the U.S. Constitution, which states, "...nor shall private property be taken for public use, without just compensation."

B.1 Major Shifts in Physical Takings Jurisprudence

Berman v. Parker (1954)- Expanded the definition of "public use" to include "public purpose" based on physical, aesthetic, and monetary benefits. Held that eradication of blighted neighborhood qualified as public purpose, and therefore made the taking constitutional.

Hawaii Housing Authority v. Midkiff (1984)- Held that a state can use its eminent domain powers to take land that is owned by a small group of private landowners and redistribute land to a wide group of private residents. Held that the purpose the government puts forth need only be "conceivable."

Kelo v. City of New London (2005)- Held that a transfer of private property to a private entity for the purpose of economic development satisfies tthe public use requirement.

B.2 Major Shifts in Regulatory Takings Jurisprudence

Pennsylvania Coal Co. v. Mahon (1922)- This case started the doctrine of regulatory takings. Before, the Takings Clause applied only to physical takings. Court held that whether a regulation constitutes a taking that requires compensation depends on the extent of the diminution of the value of the property. Created the "diminution-of-value test" to decide if a regulatory taking had occurred (has since been replaced with subsequent tests).

Penn Central Transportation Co. v. New York City (1978)- Regulations that do not cause a landowner to discontinue to use their property to their benefit, like landmark status, do not constitute a regulatory taking.

Loretto v. Teleprompter Manhattan CATV Corp. (1982)- Created the "permanent physical presence test" for regulatory takings. A regulation that is a permanent physical occupation of property is a regulatory taking to the extent of the occupation, regardless of whether there is a public benefit or if the interference to the owner is only minimal.

Lucas v. South Carolina Coastal Council (1992)- Created the "total takings test" for deciding whether a regulation constitutes a regulatory taking. A regulation that deprives the owner of all economically beneficial uses of land is a taking unless the use interest was never part of the title to begin with.

Palazzolo v. Rhode Island (2001)- An owner does not waive his right to challenge a regulation as a taking because he purchased the property after the regulation was enacted.

C Artefactual Field Experiment

1 of 3 Lock-in Tasks: Kaya sa isip o diwa na tayo ay sa mga ito, excites ilang mga antas ng parehong damdamin, sa proporsyon ng kasiglahan o dulness ng kuru-kuro. Ang labis na kung saan sila magbuntis sa kahirapan ng mga wretches nakakaapekto sa partikular na bahagi sa kanilang mga sarili ng higit pa sa anumang iba pang; dahil sa takot na arises mula sa kathang isip nila kung ano ang kani-kanilang mga sarili ay magtiis, kung sila ay talagang ang wretches kanino sila ay naghahanap sa, at kung sa partikular na bahagi sa kanilang mga sarili ay talagang apektado sa parehong miserable paraan. Ang tunay na puwersa ng mga kuru-kuro na ito ay sapat na, sa kanilang mga masasaktin frame, upang gumawa ng na galis o hindi mapalagay damdam complained ng.

Regulatory Pro-Landowner (Hamilton Bank of Johnson City v. Williamson Cty Reg. Planning (1984)): A local developer had received preliminary approval to develop houses on his land in the Northern section of Williamson County, Tennessee. After the developer had incurred substantial costs and developed most of the subdivision, the county changed its zoning ordinance. Hamilton Bank bought the remaining acres of undeveloped land through foreclosure sale. It reapplied for permission to build the full complement of houses, which the planning commission denied because of the new zoning regulations. Claiming that the commission's denial amounted to a taking of its property in violation of the Fourteenth and Fifteenth Amendments to the Constitution, Hamilton Bank argued before a District jury court that zoning regulation had rendered the land economically useless, and it would lose at least $1 million because profits from the reduced number of houses would not even cover the costs of developing the land. The District Court found the commission's regulations violated the Just Compensation Clause of the Fifth Amendment, and awarded the bank $30,000. The US Court of Appeals upheld the argument.

Regulatory Pro-Government (Rector, Wardens & Members of Vestry of St. Bart's Church (1990)): The Federal Court of Appeals upheld the landmark designation of St. Bartholomew's Church in New York City against a constitutional challenge by the Episcopal Parish. The parish argued that landmark status interfered with its property rights. The church had applied for permission to demolish its landmark Community House, to make way for a new office tower, income from which would support church activities. However, in

affirming a judgment by a lower court, the Second Circuit Court states that the New York City Landmarks law did not violate the Church's Fifth Amendment right against government takings of property without just compensation, because the church had failed to prove that it could not continue its religious practice in its existing facilities.

Physical Pro-Landowner (Hall v. City of Santa Barbara (1986)): The U.S. 9th Circuit Court of Appeals ruled that a Santa Barbara's mobile home rent control ordinance may violate the U.S. Constitution by giving tenants an interest in landlord's property without just compensation for the landlords. The ordinance requires mobile park operators to offer their tenants leases of unlimited duration, where the tenant may end the lease at will but the mobile home operator only for a cause narrowly defined in the ordinance. Rent increases are also strictly limited. William and Jean Hall, owner of Los Amigos Mobile Home Estates, a mobile home park within the City of Santa Barbara, challenged the ordinance on the ground it effected a taking of their property and that such taking was neither for a public purpose nor justly compensated.

Regulatory Pro-Government (Building Owners and Managers Ass'n Intern. v. F.C.C. (2001)): A federal appeals court here has ruled that property renters have a right to install direct-broadcast satellite dishes in locations under their control, even if such action is prohibited by a lease agreement with the landlord. Real estate owners had sought to control renters' ability to use their balconies and patios as dish-installation sites. The Building Owners and Managers Association claimed that the government's protection of renters' rights was against the Fifth Amendment prohibiting the taking of private property without just compensation. The court however, rejected the argument.

Table 1 - Summary Statistics of Takings Precedent

Circuit-Year Level	Mean [Standard Deviation]
Panel A: Physical Takings Cases (1975-2008)	
Number of Judges	17.66 [7.72]
Number of Physical Takings Panels	0.33 [0.63]
Proportion of Circuit-Years with Physical Takings Panels	27%
Proportion of Pro-Government Physical Takings Decisions when Circuit-Year has Panels	66%
Expected # of Minority Democratic Appointees per Seat when Circuit-Year has Panels	0.06 [0.06]
Expected # Republican Prior U.S. Attorneys per Seat when Circuit-Year has Panels	0.04 [0.06]
Number of Physical Takings District Cases	1.54 [1.96]
N (circuit-years)	402
Panel B: Regulatory Takings Cases (1979-2004)	
Number of Judges	17.81 [7.46]
Number of Regulatory Takings Panels	0.71 [0.99]
Proportion of Circuit-Years with Regulatory Takings Panels	46%
Proportion of Pro-Government Regulatory Takings Decisions when Circuit-Year has Panels	78%
Expected # of Judges with ABA scores of well-qualified or better per Seat when Circuit-Year has Panels	0.64 [0.13]
Number of Regulatory Takings District Cases	1.58 [1.55]
N (circuit-years)	310

Table 2 - First Stage: Relationship Between Pro-Government Physical Takings Appellate Precedent and Composition of Physical Takings Panels, 1975-2008

Panel A	Outcome: Pro-Takings						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Minority Democratic Appointee Variable	-0.203		-0.570	-0.615	-0.666	-0.518	-0.534
	(0.0686)		(0.186)	(0.193)	(0.177)	(0.184)	(0.174)
Republican Prior U.S. Attorney Variable		0.176	0.677	0.929	0.963	0.553	0.540
		(0.0741)	(0.235)	(0.272)	(0.231)	(0.215)	(0.216)
N	394	307	134	107	402	357691	4054704
R-sq	0.017	0.008	0.076	0.108	0.693	0.062	0.686
F-statistic	8.800	5.638	12.540	9.010	15.220	34.975	42.747
Pro-Takings measure	Judge Vote	Judge Vote	Panel Vote	Percentage	Percentage	Percentage	Percentage
Controls	No	No	No	No	Yes	No	Yes
Analysis level	Judge	Judge	Panel	Circuit-year	Circuit-year	Circuit-quarter zip	Circuit-quarter zip

Notes: Heteroskedasticity-robust standard errors are in parentheses and clustered at the circuit level. Controls are dummy indicators for circuit, year (and quarter), expected number of minority Democratic appointees per seat, expected number of Republican Prior U.S. Attorneys per seat, and a dummy indicator for when there are no cases in a circuit-year (or quarter).

Panel B	District-level LASSO Instruments	
	Outcome: Presence of Appellate Case	F-statistic
Fiserv (Zip-Year)	Born in 1920s and attended public institution for baccalaureate (BA), Evangelical * Born in 1940s	27.56
GDP (State-Year)	Born in 1920s and attended public institution for BA, Born in 1920s and above median wealth	9.15
CPS (Individual-Year)	Born in 1920s and attended public institution for BA, Black Prior Law Professor	29.00
FHWA (State-Year)	BA from state of appointment, Attended public institution from state of appointment for BA	6.66

Notes: LASSO selected optimal instruments from the following judge characteristics and their interactions at the judge and circuit-year level for a total of 900 possible instruments: Democrat, male, male Democrat, female Republican, minority, Black, Jewish, Catholic, Secular, Mainline Protestant, Evangelical, baccalaureate (BA) from appointment state, public baccalaureate, JD from a public institution, having an LLM or SJD, elevated from district court, decade of birth (1910s, 1920s, 1930s, 1940s, or 1950s), appointed when the President and Congress majority were from the same party, ABA score of well-qualified or better, above median wealth, appointed by president from an opposing party, prior federal judiciary experience, prior law professor, prior government experience, prior assistant U.S. attorney, and prior U.S. attorney. The symbol ".*" indicates a circuit-year level interaction.

Table 3 - House Prices and GDP Impacts

Panel A *Average Lag Effect*	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	Obs (4)	Mean Dependent Variable (5)
ΔLog Quarterly Price Index	0.002	0.012	0.007	3989626	0.012
Joint P-value	0.032	0.000	0.001		
ΔLog Annual GDP	0.001	0.011	0.011	1671	0.066
Joint P-value	0.254	0.000	0.009		
Panel B *Average Lead Effect*					
ΔLog Quarterly Price Index	0.004	0.003	0.002	3989626	0.012
Joint P-value	0.108	0.505	0.684		
ΔLog Annual GDP	0.001	0.002	0.005	1671	0.066
Joint P-value	0.890	0.810	0.453		

Notes: Data consist of Fiserv Case-Shiller zip-code level price indices. State-level GDP data are from the Bureau of Economic Analysis. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year.

Table 4 - Displacement Impacts

Panel A	OLS	Appellate IV	Appellate and District IV	Obs	Mean Dep. Variable Non-White	White
Average Interaction Lag Effect	(1)	(2)	(3)	(4)	(5)	(6)
Within-County Move in Last Year	0.001	0.001	0.001	3451505	0.115	0.090
Joint P-value	0.378	0.000	0.000			
Out-County Move in Last Year	-0.001	-0.001	-0.002	3451505	0.062	0.061
Joint P-value	0.818	0.161	0.476			
Average Level Lag Effect						
Within-County Move in Last Year	0.001	0.003	0.002	3451505	0.115	0.090
Joint P-value	0.011	0.298	0.000			
Out-County Move in Last Year	-0.0003	-0.001	0.00007	3451505	0.062	0.061
Joint P-value	0.198	0.188	0.023			
Panel B						
Average Interaction Lead Effect						
Within-County Move in Last Year	0.006	0.010	0.022	3451505	0.115	0.090
Joint P-value	0.222	0.553	0.343			
Out-County Move in Last Year	0.004	0.007	0.005	3451505	0.062	0.061
Joint P-value	0.240	0.025	0.123			
Average Level Lead Effect						
Within-County Move in Last Year	-0.001	0.003	0.003	3451505	0.115	0.090
Joint P-value	0.401	0.180	0.321			
Out-County Move in Last Year	0.001	0.001	-0.001	3451505	0.062	0.061
Joint P-value	0.338	0.498	0.814			

Notes: Data come from March CPS. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include individual controls (age, race dummies, educational attainment dummies, and a marital status dummy), circuit fixed effects, year fixed effects, circuit-specific time trends, and a dummy for whether there were no cases in that circuit-year.

Table 5 - Housing Impacts

Panel A	OLS	Appellate IV	Appellate and District IV	Obs	Mean Dep. Variable Non-White	White
Average Interaction Lag Effect	(1)	(2)	(3)	(4)	(5)	(6)
Live in Public Housing	0.009	0.005	0.003	4098609	0.079	0.017
Joint P-value	0.016	0.000	0.000			
Living Below Poverty Line	0.013	0.006	0.006	4098609	0.266	0.117
Joint P-value	0.000	0.003	0.328			
Average Level Lag Effect						
Live in Public Housing	-0.001	0.000	0.000	4098609	0.079	0.017
Joint P-value	0.002	0.647	0.534			
Living Below Poverty Line	-0.001	0.005	0.001	4098609	0.266	0.117
Joint P-value	0.076	0.020	0.001			
Panel B						
Average Interaction Lead Effect						
Live in Public Housing	0.002	0.010	0.006	4098609	0.079	0.017
Joint P-value	0.656	0.230	0.479			
Living Below Poverty Line	0.001	0.001	0.005	4098609	0.266	0.117
Joint P-value	0.934	0.963	0.743			
Average Level Lead Effect						
Live in Public Housing	-0.001	-0.001	-0.001	4098609	0.079	0.017
Joint P-value	0.242	0.623	0.591			
Living Below Poverty Line	0.001	0.008	0.007	4098609	0.266	0.117
Joint P-value	0.882	0.040	0.133			

Notes: Data come from March CPS. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include individual controls (age, race dummies, educational attainment dummies, and a marital status dummy), circuit fixed effects, year fixed effects, circuit-specific time trends, and a dummy for whether there were no cases in that circuit-year.

Table 6 - Employment Impacts

Panel A	OLS	Appellate IV	Appellate and District IV	Obs	Mean Dep. Variable Non-White	White
Average Interaction Lag Effect	(1)	(2)	(3)	(4)	(5)	(6)
Employment Status	-0.015	-0.021	-0.017	6720948	0.655	0.742
Joint P-value	0.016	0.011	0.001			
Log Real Weekly Earnings	-0.091	-0.130	-0.116	6154598	3.792	4.348
Joint P-value	0.019	0.013	0.000			
Average Level Lag Effect						
Employment Status	0.005	0.012	0.010	6720948	0.655	0.742
Joint P-value	0.158	0.000	0.000			
Log Real Weekly Earnings	0.032	0.071	0.065	6154598	3.792	4.348
Joint P-value	0.342	0.681	0.000			
Panel B						
Average Interaction Lead Effect						
Employment Status	-0.019	-0.030	-0.018	6720948	0.655	0.742
Joint P-value	0.011	0.067	0.108			
Log Real Weekly Earnings	-0.102	-0.187	-0.118	6154598	3.792	4.348
Joint P-value	0.009	0.076	0.109			
Average Level Lead Effect						
Employment Status	0.004	0.005	0.002	6720948	0.655	0.742
Joint P-value	0.131	0.356	0.622			
Log Real Weekly Earnings	0.025	0.032	0.018	6154598	3.792	4.348
Joint P-value	0.272	0.461	0.612			

Notes: Data come from MORG CPS. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include individual controls (age, race dummies, educational attainment dummies, and a marital status dummy), circuit fixed effects, year fixed effects, circuit-specific time trends, and a dummy for whether there were no cases in that circuit-year. Logs are taken of 1+earnings; earnings are set to 0 if not employed or not in the labor force.

Table 7 - Parcels Acquired for Federal Transportation Projects Impacts

Average Lag Effect	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	Obs (4)	Mean Dependent Variable (5)
Log Compensation	0.187	0.023	0.125	572	16.746
Joint P-value of lags	0.076	0.004	0.002		
Joint P-value of leads	0.764	0.317	0.153		
Log Parcels Acquired	-0.003	-0.056	-0.103	663	6.456
Joint P-value of lags	0.043	0.000	0.000		
Joint P-value of leads	0.223	0.462	0.660		
Log Residential Displacements	-0.134	-0.199	-0.065	663	3.508
Joint P-value of lags	0.195	0.129	0.044		
Joint P-value of leads	0.451	0.758	0.608		
Log Residential Relocation Costs	-0.156	-0.302	-0.091	663	12.587
Joint P-value of lags	0.282	0.087	0.000		
Joint P-value of leads	0.053	0.164	0.191		
Log Replacement Housing Costs	-0.251	-0.372	-0.126	663	12.357
Joint P-value of lags	0.316	0.011	0.120		
Joint P-value of leads	0.229	0.103	0.583		
Log Commercial Displacements	0.031	0.025	0.122	663	3.139
Joint P-value of lags	0.027	0.000	0.000		
Joint P-value of leads	0.053	0.909	0.979		
Log Commercial Relocation Costs	0.099	0.138	0.163	663	12.117
Joint P-value of lags	0.088	0.012	0.009		
Joint P-value of leads	0.800	0.581	0.638		

Notes: Data come from FHWA (http://www.fhwa.dot.gov/realestate/49cfr24fr.pdf). Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year. All values are in logs of the underlying value plus one. Data range: 1991-2003, except compensation: 1995-2003.

Table 8 -- Dynamic Housing Price Response

	ΔLog Quarterly Price Index					
Panel A: Quarterly Lags	(q0)	(q4)	(q8)	(q12)	(q16)	Mean
1. Circuit-quarter laws	0.009	0.003	0.017	0.008	0.003	0.010
	(0.003)	(0.007)	(0.008)	(0.007)	(0.006)	
2. Circuit-quarter laws (Law_{ct})	0.009	-0.000	0.011	0.004	-0.000	0.007
controlling for	(0.004)	(0.008)	(0.005)	(0.006)	(0.005)	
Local takings decision ($LocalLaw_{ict}$)	-0.018	0.014	-0.000	-0.013	0.010	0.005
	(0.025)	(0.022)	(0.029)	(0.040)	(0.023)	
Panel B: Yearly Lags	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
1. Add Circuit-Specific Trends	0.010	0.013	0.019	0.014	0.006	
	(0.006)	(0.004)	(0.004)	(0.005)	(0.002)	
2. No Fixed Effects	-0.000	-0.003	0.015	0.018	0.001	
	(0.007)	(0.004)	(0.009)	(0.010)	(0.006)	
3. State Cluster	0.010	0.014	0.019	0.012	0.006	
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	
4. Control for Expectation	0.016	0.021	0.023	0.015	0.010	
	(0.009)	(0.006)	(0.003)	(0.004)	(0.003)	
5. Use Population Weights	0.014	0.019	0.023	0.014	0.005	
	(0.007)	(0.006)	(0.005)	(0.004)	(0.002)	
6. Drop Circuit 1	0.008	0.013	0.019	0.012	0.005	
	(0.006)	(0.004)	(0.003)	(0.004)	(0.002)	
Drop Circuit 2	0.006	0.011	0.017	0.009	0.006	
	(0.006)	(0.005)	(0.005)	(0.004)	(0.001)	
Drop Circuit 3	0.012	0.016	0.019	0.012	0.006	
	(0.006)	(0.003)	(0.003)	(0.004)	(0.002)	
Drop Circuit 4	0.010	0.014	0.019	0.012	0.006	
	(0.006)	(0.004)	(0.003)	(0.004)	(0.001)	
Drop Circuit 5	0.012	0.013	0.019	0.015	0.004	
	(0.006)	(0.004)	(0.004)	(0.004)	(0.002)	
Drop Circuit 6	0.008	0.011	0.018	0.013	0.007	
	(0.006)	(0.004)	(0.002)	(0.003)	(0.002)	
Drop Circuit 7	0.010	0.014	0.023	0.015	0.007	
	(0.006)	(0.004)	(0.003)	(0.004)	(0.002)	
Drop Circuit 8	0.010	0.013	0.018	0.013	0.005	
	(0.006)	(0.005)	(0.004)	(0.004)	(0.002)	
Drop Circuit 9	0.007	0.011	0.018	0.011	0.005	
	(0.006)	(0.010)	(0.009)	(0.009)	(0.009)	
Drop Circuit 10	0.011	0.015	0.019	0.012	0.006	
	(0.005)	(0.004)	(0.003)	(0.004)	(0.002)	
Drop Circuit 11	0.012	0.016	0.020	0.013	0.005	
	(0.007)	(0.004)	(0.004)	(0.005)	(0.003)	
Drop Circuit 12	0.010	0.014	0.019	0.012	0.006	
	(0.006)	(0.004)	(0.003)	(0.004)	(0.002)	
7. 1 Lag	0.004	0.004				
	(0.003)	(0.003)				
2 Lags	0.004	0.010	0.016			
	(0.003)	(0.004)	(0.003)			
2 Leads, 4 Lags	0.010	0.016	0.018	0.010	0.004	
	(0.006)	(0.005)	(0.003)	(0.004)	(0.002)	
1 Lead, 5 Lags	0.011	0.012	0.017	0.014	0.003	-0.005
	(0.005)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
Panel C: Yearly Leads	(t1)	(t0)	(f1)	(f2)	(f3)	(f4)
4 Leads, 1 Lag	0.005	0.004	-0.004	-0.005	-0.004	0.001
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)

Notes: Data consist of Fiserv Case-Shiller/FHFA zip-code level price indices. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include circuit fixed effects, year and quarter fixed effects, and a dummy for whether there were no cases in that circuit-year. The baseline regression is an instrumental variables specification with one lead and four lags of appellate physical takings precedent, corresponding to column 2 in Table 3.

Table 9 - Impacts of Presence of Appeals

Panel A *Average Lag Effect*	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	Obs (4)	Mean Dependent Variable (5)
ΔLog Quarterly Price Index	-0.003	-0.010	-0.006	3989626	0.012
Joint P-value of lags	0.094	0.000	0.153		
Joint P-value of leads	0.732	0.706	0.861		
ΔLog Annual GDP	-0.002	-0.009	-0.010	1671	0.066
Joint P-value of lags	0.040	0.000	0.000		
Joint P-value of leads	0.886	0.620	0.414		

Notes: Data consist of Fiserv Case-Shiller/FHFA zip-code level price indices. State-level GDP data are from the Bureau of Economic Analysis. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year.

Table 10 - Impacts of Regulatory Takings Precedent

	Average Lag Effect			
Panel A	OLS	IV	Obs	Mean Dep. Variable
House Prices and GDP	(1)	(2)	(3)	(4)
ΔLog Quarterly Price Index	0.002	0.003	2486744	0.11
Joint P-value of lags	0.086	0.000		
Joint P-value of leads	0.005	0.333		
ΔLog Annual GDP	0.005	0.002	1065	0.56
Joint P-value of lags	0.024	0.017		
Joint P-value of leads	0.897	0.918		
Panel B	Average Interaction Lag Effect		Obs	Non-White	White
Displacement					
Within-County Move in Last Year	0.002	-0.002	2916474	0.118	0.091
Joint P-value of lags	0.003	0.692			
Joint P-value of leads	0.816	0.194			
Out-County Move in Last Year	-0.000	0.002	2916474	0.063	0.061
Joint P-value of lags	0.031	0.000			
Joint P-value of leads	0.523	0.316			
Live in Public Housing	0.010	0.008	3227637	0.080	0.017
Joint P-value of lags	0.016	0.205			
Joint P-value of leads	0.243	0.442			
Living Below Poverty Line	0.018	0.028	3227637	0.266	0.119
Joint P-value of lags	0.035	0.607			
Joint P-value of leads	0.953	0.097			
Employment Status	-0.011	0.004	5341620	0.660	0.750
Joint P-value of lags	0.169	0.958			
Joint P-value of leads	0.115	0.476			
Log Real Weekly Earnings	-0.064	0.013	4892691	3.817	4.405
Joint P-value of lags	0.002	0.951			
Joint P-value of leads	0.158	0.539			
	Average Lag Effect				
Log Residential Displacements	0.202	1.127	663	3.508	
Joint P-value of lags	0.383	0.496			
Joint P-value of leads	0.594	0.719			
Log Commercial Displacements	0.203	-0.209	663	3.139	
Joint P-value of lags	0.182	0.777			
Joint P-value of leads	0.683	0.687			
Panel C	Fiserv	GDP	CPS	FHWA	
First stage F-statistic					
Appellate LASSO IV	48.36	37.48	50.39	30.01	
District LASSO IV	6.53	6.43	11.51	9.24	

Table 11 - Impact Analysis

Average Lag Effect *Panel A* Physical Takings Precedent	Appellate and District IV (1)	$E(Law_{ct}\|M_{ct} > 0)$ (2)	$E(1[M_{ct} > 0])$ (3)	Typical Effect (4)
ΔLog Quarterly Price Index	0.007	0.66	0.27	0.001
ΔLog Annual GDP	0.011	0.66	0.27	0.002
Within-County Move in Last Year Nonwhite-White Inequality	0.001	0.66	0.27	0.0002
Live in Public Housing Nonwhite-White Inequality	0.003	0.66	0.27	0.001
Employment Status Nonwhite-White Inequality	-0.017	0.66	0.27	-0.003
Log Real Weekly Earnings Nonwhite-White Inequality	-0.116	0.66	0.27	-0.021
Log Federal Compensation	-0.474	0.66	0.27	-0.084
Panel B Regulatory Takings Precedent				
ΔLog Quarterly Price Index	0.003	0.78	0.46	0.001
ΔLog Annual GDP	0.002	0.78	0.46	0.001

Panel C	Sectoral Impacts					
ΔLog Sectoral Annual GDP	Physical Takings	Joint P-value		Regulatory Takings	Joint P-value	
	Appellate and District IV	Lags	Leads	Appellate and District IV	Lags	Leads
Construction	0.039	0.001	0.227	-0.016	0.145	0.405
Manufacturing	0.007	0.784	0.169	-0.009	0.000	0.844
Retail	0.017	0.001	0.768	0.002	0.152	0.274
Services	-0.092	0.001	0.456	0.038	0.000	0.830
Wholesale	0.013	0.213	0.300	-0.006	0.033	0.756
Mining	0.018	0.690	0.236	-0.062	0.735	0.465
Agriculture	0.057	0.000	0.674	-0.076	0.312	0.303
Transportation and Utilities	0.014	0.005	0.311	0.006	0.885	0.725
Finance, insurance, rental, estate	0.022	0.014	0.919	0.003	0.002	0.850
Government	0.003	0.002	0.470	0.0004	0.027	0.312

Figure 1

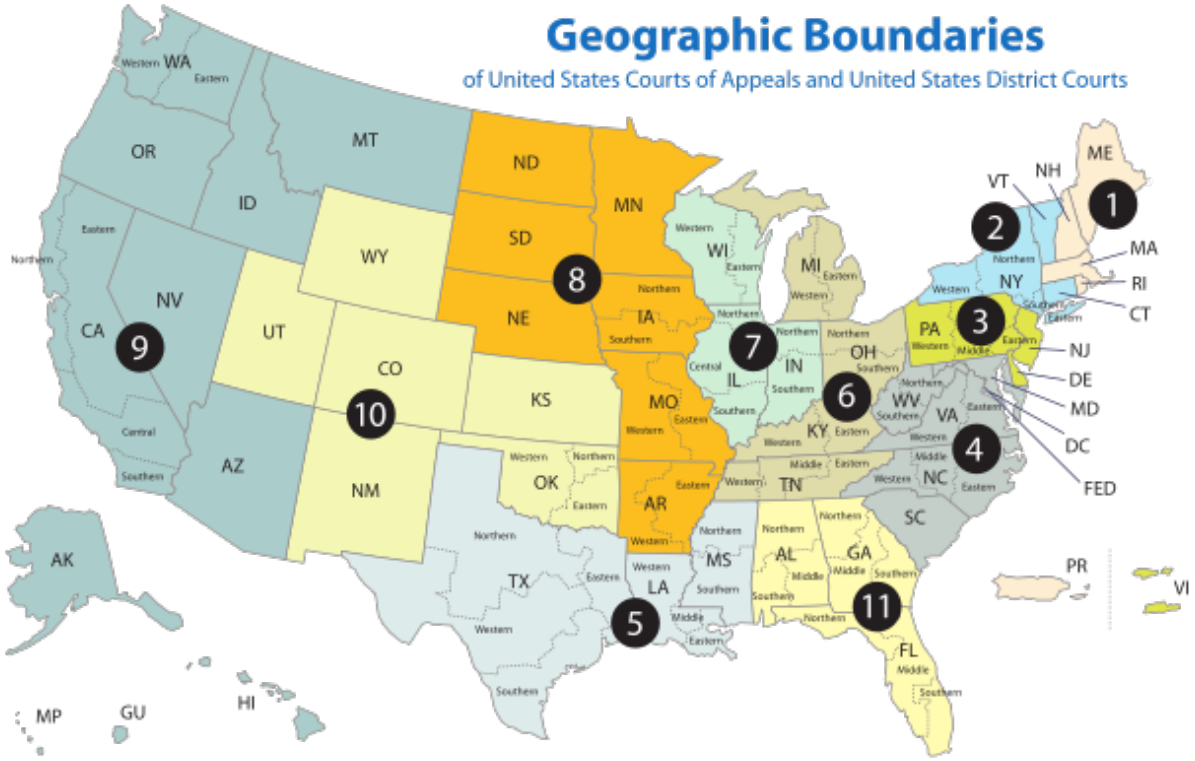


Figure 2 – Map of Alleged Takings

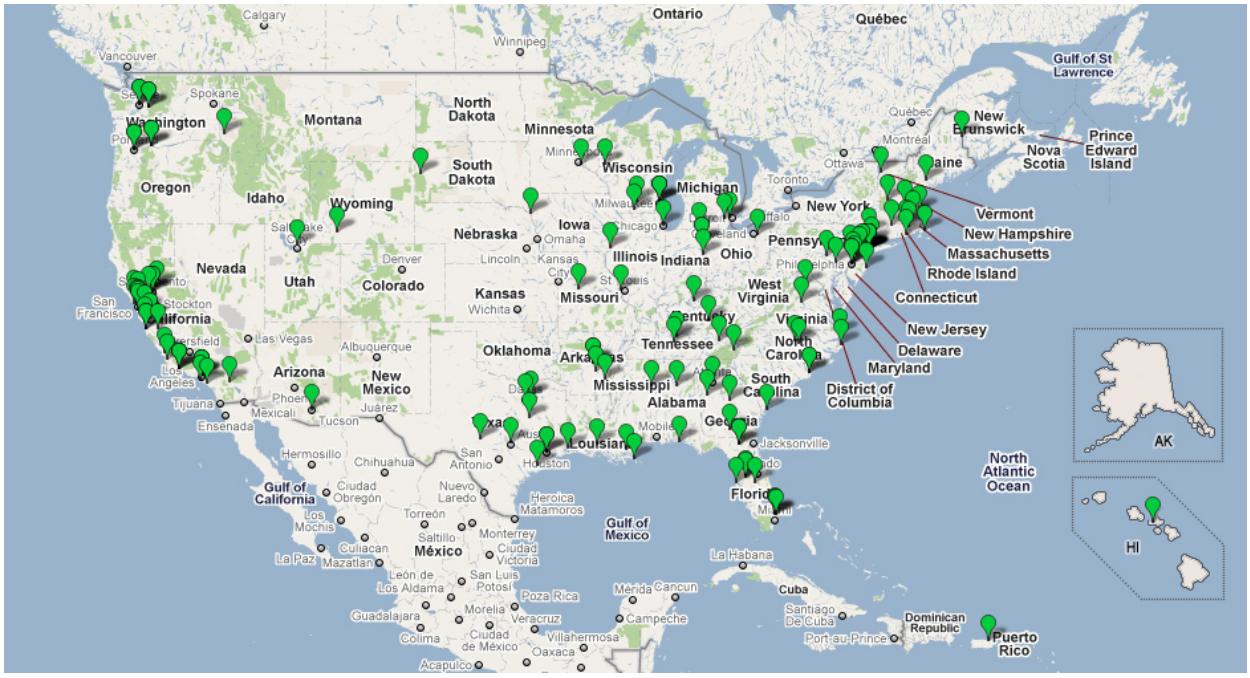
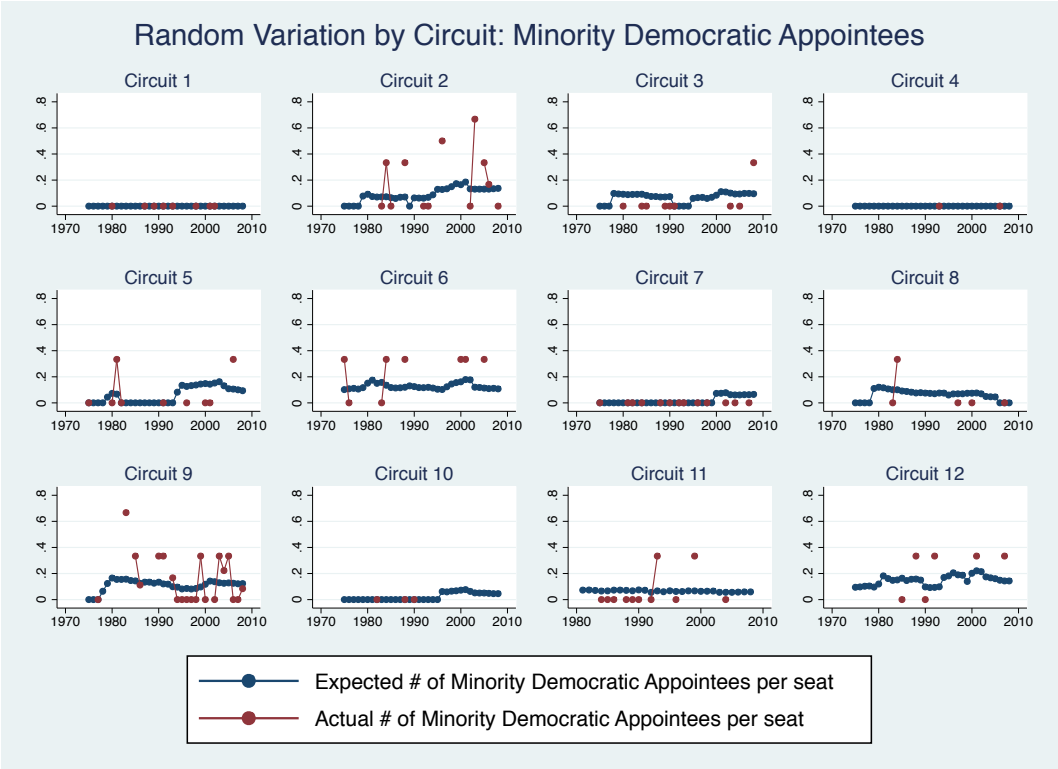


Figure 3 – Random Variation in Judicial Composition



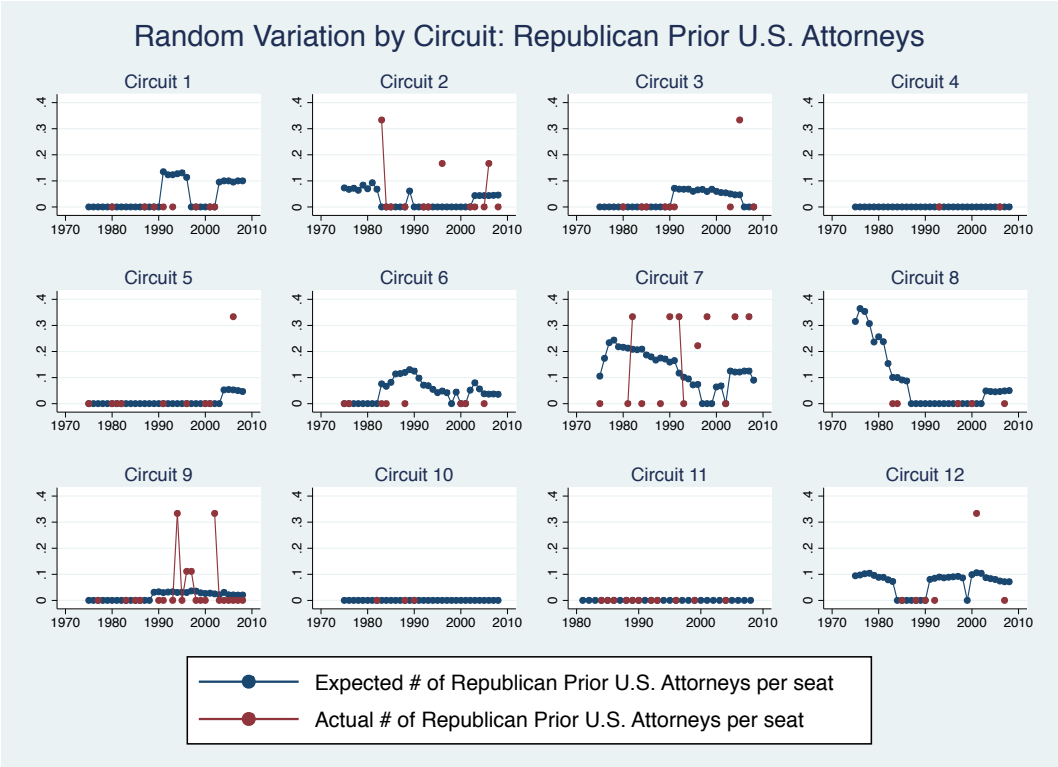
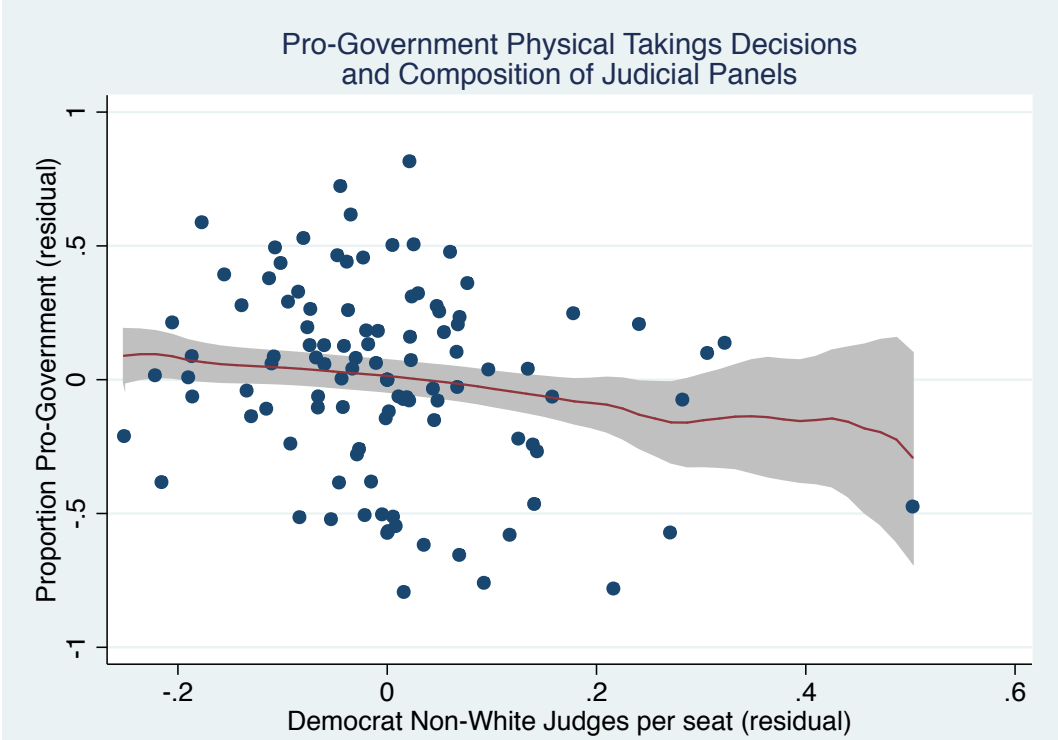
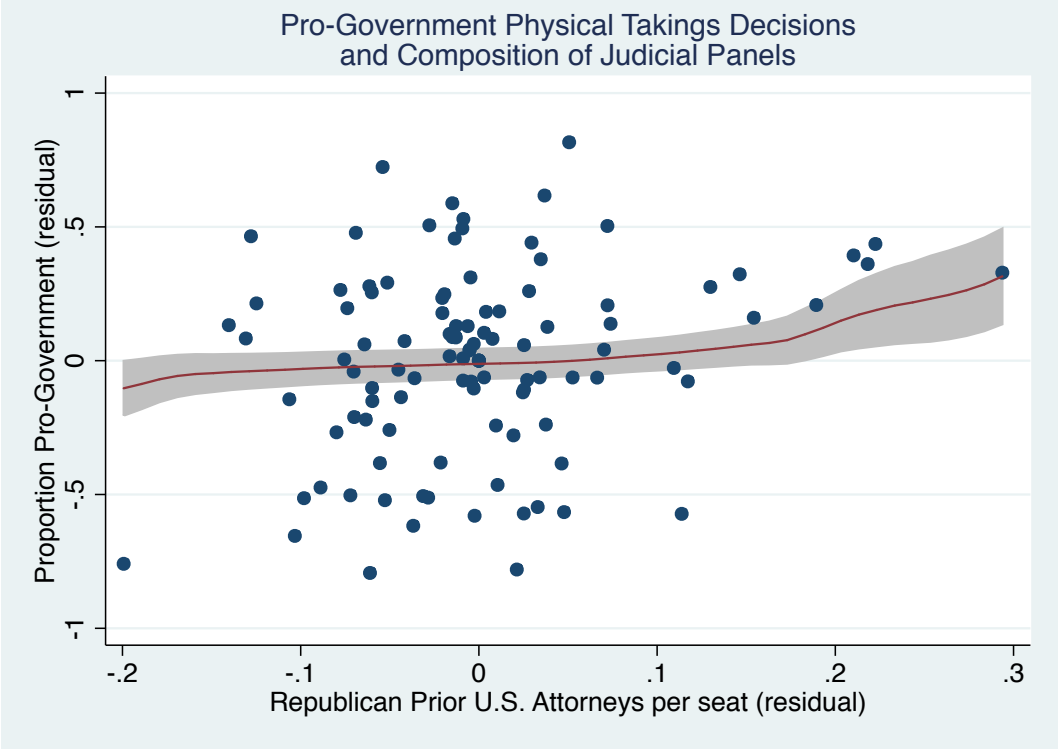


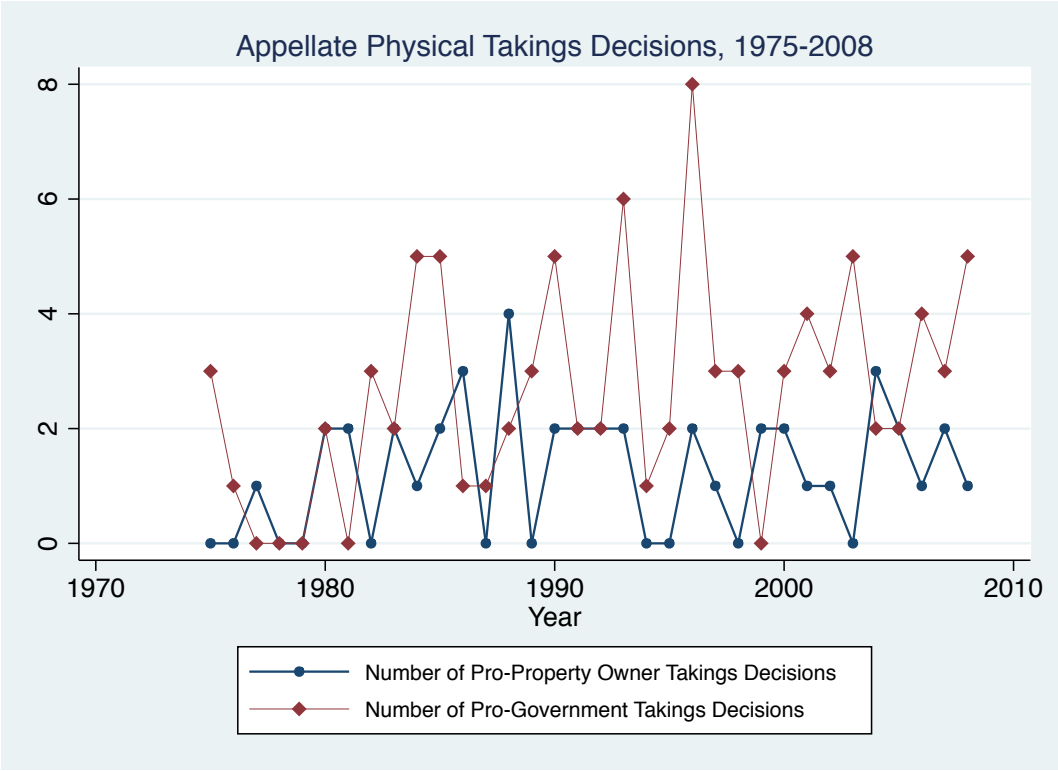
Figure 4 – Local Polynomial Estimates of First Stage





Nonparametric local polynomial estimates are computed using an Epanechnikov kernel. Rule-of-thumb bandwidth is used. Shaded area indicates 90 percent confidence bands. The residuals are calculated removing circuit and year fixed effects.

Figure 5 – Number of Decisions



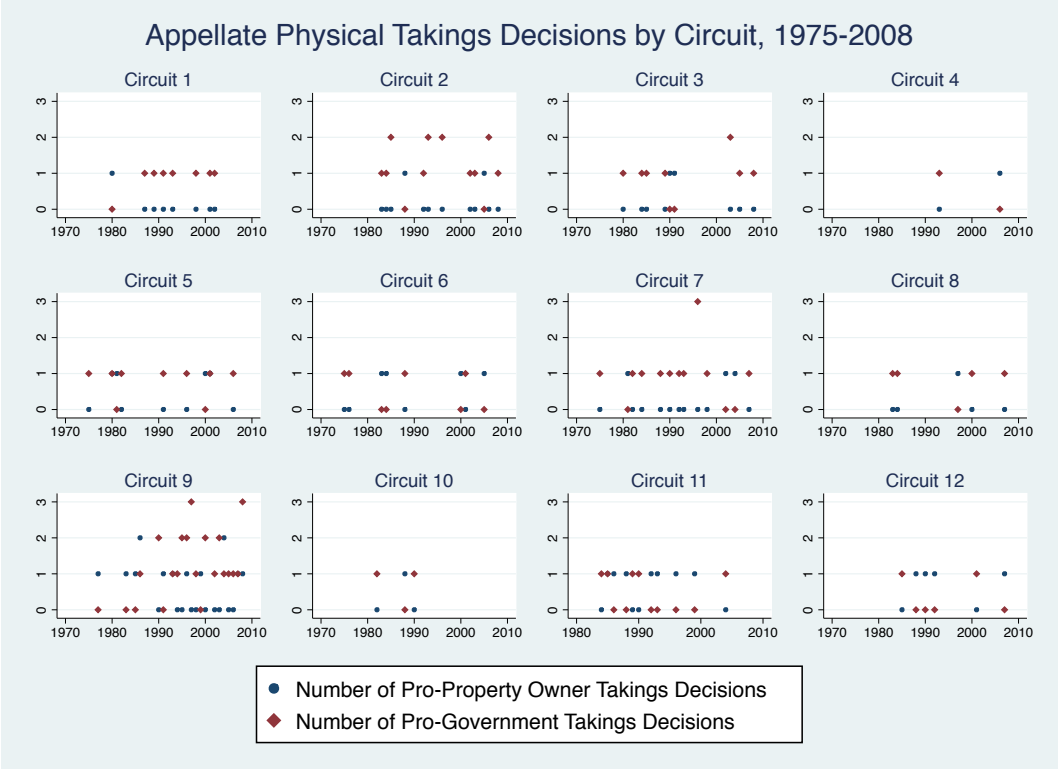


Figure 6: Dynamic Reponse to Takings Predecent

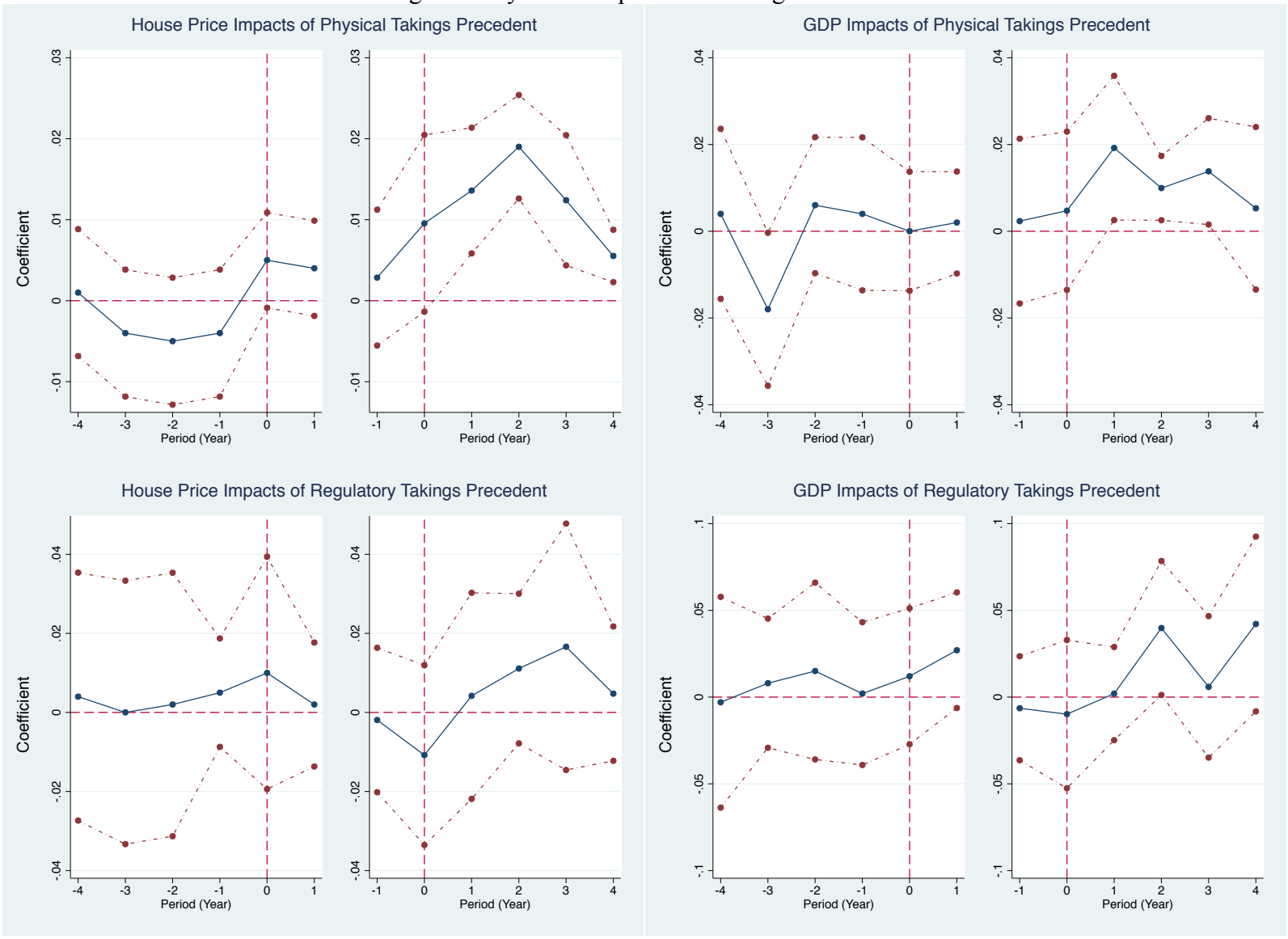
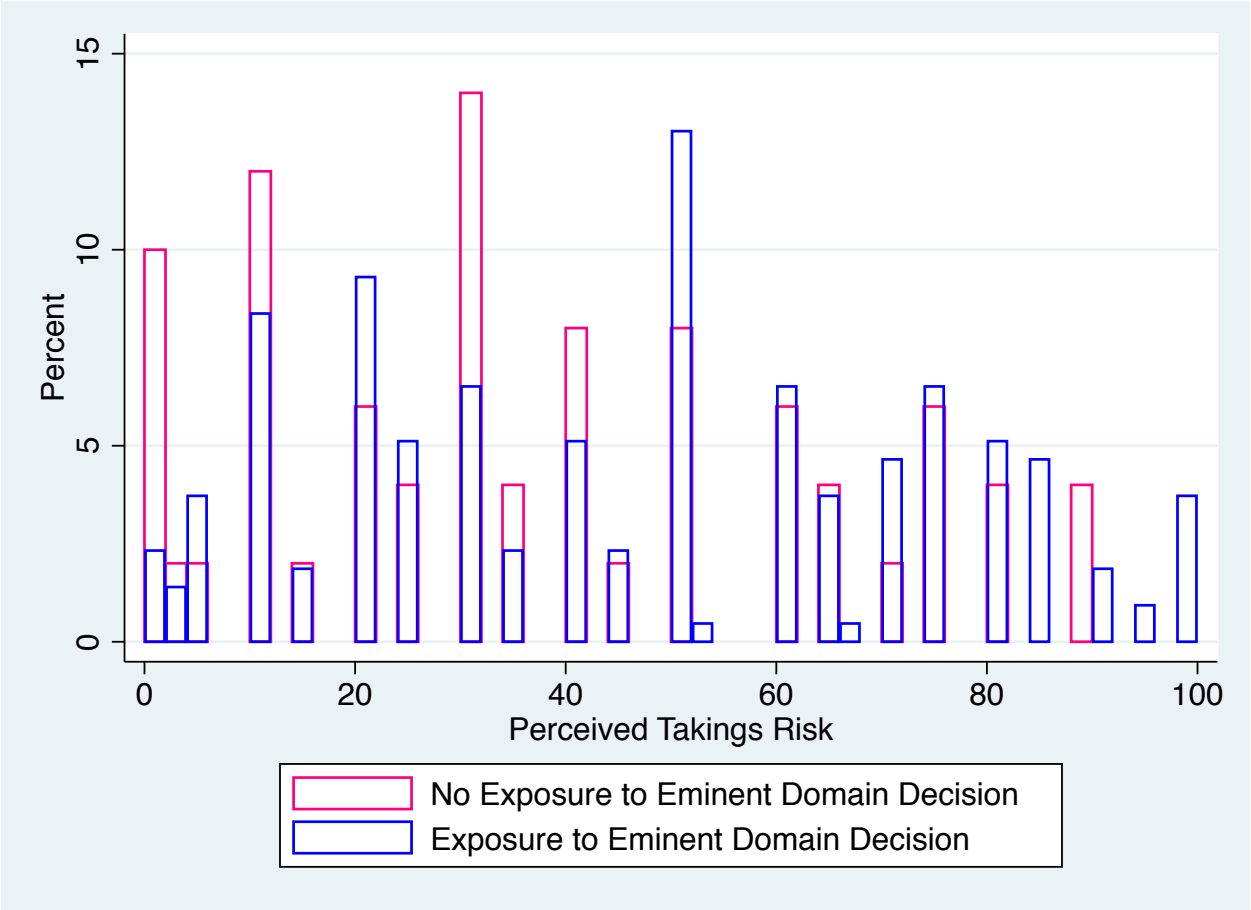


Figure 7: Perceived Takings Risk in Response to Eminent Domain Decisions



Appendix Table 1: List of Physical Takings Appellate Precedent

Citation	Case Name	Circuit	Year	Pro-Property Owner
228 F.2d 280	U.S. v. Certain Parcels of Land in Fairfax County, of Va.	4	1955	1
229 F.2d 675	Anderson v. U.S.	5	1956	0
235 F.2d 864	District of Columbia Redevelopment Land Agency v. 61 Parcels of Land in Squares	12	1956	0
249 F.2d 811	Richmond Inv. Co. v. U.S.	9	1957	0
269 F.2d 546	Donnelly v. District of Columbia Redevelopment Land Agency	12	1959	0
287 F.2d 141	Paper v. District of Columbia Redevelopment Land Agency	12	1960	0
284 F.2d 221	Mamer v. District of Columbia Redevelopment Land Agency	12	1960	0
285 F.2d 628	U.S. v. Mischke	8	1961	0
296 F.2d 438	Leeaye, Inc. v. District of Columbia Redevelopment Land Agency	12	1961	0
310 F.2d 99	Harrison-Halsted Community Group, Inc. v. Housing and Home Finance Agency	7	1962	0
302 F.2d 880	Maiatico v. U.S.	12	1962	1
322 F.2d 139	U.S. v. Agee	6	1963	0
316 F.2d 791	Harwell v. U.S.	10	1963	0
334 F.2d 229	U.S. v. 91.69 Acres of Land, More or Less, in Oconee County, State of S. C.	4	1964	0
328 F.2d 115	U.S. v. Cobb	9	1964	0
350 F.2d 356	2,953.15 Acres of Land, More or Less, in Russell County, State of Ala. v. U.S.	5	1965	1
347 F.2d 970	Maun v. U.S.	9	1965	1
350 F.2d 901	Wilson v. U. S.	10	1965	0
367 F.2d 768	U.S. v. Bowman	7	1966	0
367 F.2d 161	Southern Pac. Land Co. v. U.S.	9	1966	0
374 F.2d 218	West, Inc. v. U.S.	5	1967	0
395 F.2d 920	Norwalk CORE v. Norwalk Redevelopment Agency	2	1968	1
390 F.2d 388	Scott Lumber Co. v. U.S.	9	1968	1
409 F.2d 932	Lake Charles Harbor and Terminal Dist. v. Henning	5	1969	0
432 F.2d 1286	U.S. v. 2,606.84 Acres of Land in Tarrant County, Tex.	5	1970	0
426 F.2d 955	Woodland Market Realty Co. v. City of Cleveland	6	1970	0
443 F.2d 104	U.S. v. 3,317.39 Acres of Land, More or Less, in Jefferson County, Ark.	8	1971	1
448 F.2d 980	U.S. v. 80.5 Acres of Land, More or Less, in Shasta County, State of Cal.	9	1971	0
456 F.2d 264	U. S. ex rel. and for Use of Tennessee Val. Authority v. Two Tracts of Land	6	1972	0
491 F.2d 301	U.S. v. 21.54 Acres of Land, More or Less, in Marshall County, State of W. Va.	4	1973	1
479 F.2d 404	U.S. v. 6,321 Acres of Land More or Less In Suffolk County	1	1973	0
478 F.2d 1055	U.S. v. 58.16 Acres of Land, More or Less In Clinton County, State of Ill.	7	1973	1
478 F.2d 484	U.S. v. 20.53 Acres of Land, More or Less, in Osborne County, Kansas	10	1973	0
514 F.2d 38	Gardner v. Nashville Housing Authority	6	1975	0
525 F.2d 450	U.S. v. 416.81 Acres of Land	7	1975	0
516 F.2d 1051	Maher v. City of New Orleans	5	1975	0
532 F.2d 1083	U.S. ex rel. Tennessee Val. Authority v. Two Tracts of Land	6	1976	0
561 F.2d 1327	Richmond Elks Hall Ass'n v. Richmond Redevelopment Agency	9	1977	1
616 F.2d 680	Rogin v. Bensalem Tp.	3	1980	0
616 F.2d 762	U.S. v. 101.88 Acres of Land, More or Less, Situated in St. Mary Parish	5	1980	1
639 F.2d 6	John Donnelly & Sons v. Campbell	1	1980	1
613 F.2d 1285	Stansberry v. Holmes	5	1980	0
665 F.2d 138	Devines v. Maier	7	1981	1
639 F.2d 299	U.S. v. 162.20 Acres of Land, More or Less, Situated in Clay County	5	1981	1
694 F.2d 476	Barbian v. Panagis	7	1982	0
678 F.2d 24	National Western Life Ins. Co. v. Commodore Cove Imp. Dist.	5	1982	0
691 F.2d 474	U.S. v. 82.46 Acres of Land, More or Less, Situate in Carbon County, Wyo	10	1982	0
718 F.2d 789	Amen v. City of Dearborn	6	1983	1
712 F.2d 349	Lower Brule Sioux Tribe of South Dakota v. U.S.	8	1983	0
702 F.2d 788	Midkiff v. Tom	9	1983	1
710 F.2d 895	Kohl Indus. Park Co. v. Rockland County	2	1983	0
748 F.2d 1486	Charles J. Arndt, Inc. v. City of Birmingham	11	1984	0
728 F.2d 876	Devines v. Maier	7	1984	0
746 F.2d 135	Park Ave. Tower Associates v. City of New York	2	1984	0
732 F.2d 1375	Story v. Marsh	8	1984	0
727 F.2d 287	Troy Ltd. v. Renna	3	1984	0
729 F.2d 402	Hamilton Bank of Johnson City v. Williamson County Regional Planning Com'n	6	1984	1
753 F.2d 1468	Robinson v. Ariyoshi	9	1985	1
770 F.2d 288	In re G. & A. Books, Inc.	2	1985	0
771 F.2d 44	Rosenthal & Rosenthal Inc. v. New York State Urban Development Corp.	2	1985	0
777 F.2d 47	Hilton Washington Corp. v. District of Columbia	12	1985	0
772 F.2d 1537	Florida Power Corp. v. F.C.C.	11	1985	1
764 F.2d 796	Rymer v. Douglas County	11	1985	0
771 F.2d 707	Keystone Bituminous Coal Assn. v. Duncan	3	1985	0
779 F.2d 1553	Henley v. Herring	11	1986	1
797 F.2d 1493	Hall v. City of Santa Barbara	9	1986	1
781 F.2d 1349	Martori Bros. Distributors v. James-Massengale	9	1986	0
792 F.2d 1453	McMillan v. Goleta Water Dist.	9	1986	1
811 F.2d 677	Wood v. City of East Providence	1	1987	0
850 F.2d 1483	A.A. Profiles, Inc. v. City of Ft. Lauderdale	11	1988	1
844 F.2d 461	Coniston Corp. v. Village of Hoffman Estates	7	1988	0
847 F.2d 304	Calvert Investments, Inc. v. Louisville and Jefferson County Metropolitan Sewer	6	1988	0
854 F.2d 591	Alliance of American Insurers v. Cuomo	2	1988	1
836 F.2d 498	U.S. v. 2,560.00 Acres of Land, More or Less, Situate in Washington County	10	1988	1
850 F.2d 694	National Wildlife Federation v. I.C.C.	12	1988	1
868 F.2d 433	Wendy's Intern., Inc. v. City of Birmingham	11	1989	0
885 F.2d 1119	U.S. v. Frame	3	1989	0

Citation	Case Name	Circuit	Year	Pro-Property Owner
889 F.2d 1181	Duty Free Shop, Inc. v. Administracion De Terrenos De Puerto Rico	1	1989	0
898 F.2d 347	Pinewood Estates of Michigan v. Barnegat Tp. Leveling Bd.	3	1990	1
911 F.2d 743	Boston and Maine Corp. v. I.C.C.	12	1990	1
912 F.2d 467	Kurr v. Village of Buffalo Grove	7	1990	0
900 F.2d 1434	Oberndorf v. City and County of Denver	10	1990	0
922 F.2d 498	Southern Pacific Transp. Co. v. City of Los Angeles	9	1990	0
902 F.2d 905	Centel Cable Television Co. of Florida v. Thomas J. White Development Corp.	11	1990	0
919 F.2d 593	Mountain Water Co. v. Montana Dept. of Public Service Regulation	9	1990	0
932 F.2d 51	Gilbert v. City of Cambridge	1	1991	0
940 F.2d 925	Samaad v. City of Dallas	5	1991	0
948 F.2d 575	Azul Pacifico, Inc. v. City of Los Angeles	9	1991	1
945 F.2d 594	Hughes v. Consol-Pennsylvania Coal Co.	3	1991	1
978 F.2d 1269	Nixon v. U.S.	12	1992	1
953 F.2d 600	Cable Holdings of Georgia, Inc. v. McNeil Real Estate Fund VI, Ltd.	11	1992	1
956 F.2d 670	Rose Acre Farms, Inc. v. Madigan	7	1992	0
980 F.2d 84	Southview Associates, Ltd. v. Bongartz	2	1992	0
985 F.2d 573	Pacific Power and Light Co. v. Surprise Valley Electrification Corp.	9	1993	1
997 F.2d 1369	Corn v. City of Lauderdale Lakes	11	1993	1
998 F.2d 680	Levald, Inc. v. City of Palm Desert	9	1993	0
6 F.3d 867	AMSAT Cable Ltd. v. Cablevision of Connecticut Ltd. Partnership	2	1993	0
993 F.2d 962	Washington Legal Foundation v. Massachusetts Bar Foundation	1	1993	0
987 F.2d 913	Garelick v. Sullivan	2	1993	0
991 F.2d 1169	Media General Cable of Fairfax, Inc. v. Sequoyah Condominium Council of Co-Owners	4	1993	0
5 F.3d 285	Gamble v. Eau Claire County	7	1993	0
37 F.3d 468	Carson Harbor Village Ltd. v. City of Carson	9	1994	0
53 F.3d 338	Karagozian v. City of Laguna Beach	9	1995	0
57 F.3d 781	Hoeck v. City of Portland	9	1995	0
95 F.3d 1422	Del Monte Dunes at Monterey, Ltd. v. City of Monterey	9	1996	1
101 F.3d 1095	Texas Manufactured Housing Ass'n, Inc. v. City of Nederland	5	1996	0
83 F.3d 45	Federal Home Loan Mortg. Corp. v. New York State Div. of Housing and Community	2	1996	0
107 F.3d 3 (Table)	October Twenty-Four, Inc. v. Town of Plainville	2	1996	0
84 F.3d 865	Hager v. City of West Peoria	7	1996	0
85 F.3d 422	Broad v. Sealaska Corp.	9	1996	0
87 F.3d 290	Fireman's Fund Ins. Co. v. Quackenbush	9	1996	0
89 F.3d 1481	Bickerstaff Clay Products Co., Inc. v. Harris County, Ga. By and Through Bd.	11	1996	1
93 F.3d 301	Porter v. DiBlasio	7	1996	0
95 F.3d 1359	Wisconsin Cent. Ltd. v. Public Service Com'n of Wisconsin	7	1996	0
105 F.3d 1281	Bay View, Inc. on behalf of AK Native Village Corporations v. Ahtna, Inc.	9	1997	0
124 F.3d 1150	Richardson v. City and County of Honolulu	9	1997	0
112 F.3d 313	McKenzie v. City of White Hall	8	1997	1
109 F.3d 1493	U.S. v. 0.59 Acres of Land	9	1997	0
153 F.3d 356	International College of Surgeons v. City of Chicago	7	1998	0
147 F.3d 802	Garneau v. City of Seattle	9	1998	0
160 F.3d 834	South County Sand & Gravel Co., Inc. v. Town of South Kingstown	1	1998	0
165 F.3d 692	Thomas v. Anchorage Equal Rights Com'n	9	1999	1
187 F.3d 1324	Gulf Power Co. v. U.S.	11	1999	1
214 F.3d 573	John Corp. v. City of Houston	5	2000	1
216 F.3d 764	Tahoe-Sierra Preservation Council, Inc. v. Tahoe Regional Planning Agency	9	2000	0
230 F.3d 355	Milligan v. City of Red Oak, Iowa	8	2000	0
224 F.3d 1030	Chevron USA, Inc. v. Cayetano	9	2000	0
226 F.3d 758	Montgomery v. Carter County, Tennessee	6	2000	1
31 Fed.Appx. 159	Kamman Inc. v. City of Hewitt	5	2001	0
266 F.3d 487	Anderson v. Charter Tp. of Ypsilanti	6	2001	0
254 F.3d 89	Building Owners and Managers Ass'n Intern. v. F.C.C.	12	2001	0
267 F.3d 45	Philip Morris, Inc. v. Reilly	1	2001	0
270 F.3d 180	Washington Legal Foundation v. Texas Equal Access to Justice Foundation	5	2001	1
285 F.3d 142	Deniz v. Municipality of Guaynabo	1	2002	0
31 Fed.Appx. 19	West 95 Housing Corp. v. New York City Dept. of Housing Preservation	2	2002	0
288 F.3d 375	Daniel v. County of Santa Barbara	9	2002	0
306 F.3d 445	Daniels v. Area Plan Com'n of Allen County	7	2002	1
353 F.3d 651	Hacienda Valley Mobile Estates v. City of Morgan Hill	9	2003	0
344 F.3d 959	Hotel & Motel Ass'n of Oakland v. City of Oakland	9	2003	0
57 Fed.Appx. 939	Jones v. Philadelphia Police Dept.	3	2003	0
316 F.3d 308	Tancredi v. Metropolitan Life Ins. Co.	2	2003	0
342 F.3d 222	Borough of Columbia v. Surface Transp. Bd.	3	2003	0
97 Fed.Appx. 698	Los Altos El Granada Investors v. City of Capitola	9	2004	0
374 F.3d 887	Cashman v. City of Cotati	9	2004	1
366 F.3d 1186	Garvie v. City of Ft. Walton Beach, Fla.	11	2004	0
361 F.3d 934	Greenfield Mills, Inc. v. Macklin	7	2004	1
363 F.3d 846	Chevron USA, Inc. v. Bronster	9	2004	1
411 F.3d 697	Warren v. City of Athens, Ohio	6	2005	1
419 F.3d 1036	M&A Gabaee v. Community Redevelopment Agency of City of Los Angeles	9	2005	0
143 Fed.Appx. 439	Ash v. Redevelopment Authority of Philadelphia	3	2005	0
434 F.3d 121	Brody v. Village of Port Chester	2	2005	1
464 F.3d 362	Buffalo Teachers Federation v. Tobe	2	2006	0
464 F.3d 480	Presley v. City Of Charlottesville	4	2006	1

Citation	Case Name	Circuit	Year	Pro-Property Owner
202 Fed.Appx. 670	Western Seafood Co. v. U.S.	5	2006	0
173 Fed.Appx. 931	Didden v. Village of Port Chester	2	2006	0
203 Fed.Appx. 70	U.S. v. 1,402 Acres of Land	9	2006	0
502 F.3d 616	St. John's United Church of Christ v. City of Chicago	7	2007	0
509 F.3d 1020	Action Apartment Ass'n, Inc. v. Santa Monica Rent Control Bd.	9	2007	0
474 F.3d 528	Cormack v. Settle-Beshears	8	2007	0
487 F.3d 941	Rumber v. District of Columbia	12	2007	1
497 F.3d 902	Vacation Village, Inc. v. Clark County, Nev	9	2007	1
516 F.3d 50	Goldstein v. Pataki	2	2008	0
2008 WL 2225684	Surf and Sand, LLC v. City of Capitola	9	2008	0
289 Fed.Appx. 232	Besaro Mobile Home Park, LLC v. City of Fremont	9	2008	0
547 F.3d 943	U.S. v. 14.02 Acres of Land More or Less in Fresno County	9	2008	0
512 F.3d 1148	Matsuda v. City and County of Honolulu	9	2008	1
550 F.3d 302	Carole Media LLC v. New Jersey Transit Corp.	3	2008	0

Table 2 - Randomization Check

Panel A: P-Values from Kolmogorov Smirnov Test for Deviations from Uniform CDF

	Physical Takings (Minority Democrat Appointees)				
	distance	size	90%	95%	99%
Autocorrelation	0.180666667	9	0.3392	0.3874	0.4795
Mean Reversion	0.318	8	0.3583	0.4097	0.5068
Longest Run	0.200888889	9	0.3392	0.3874	0.4795
	Regulatory Takings (Judges with ABA scores of well-qualified or better)				
	distance	size	90%	95%	99%
Autocorrelation	0.218333333	12	0.2958	0.3382	0.4192
Mean Reversion	0.215333333	12	0.2958	0.3382	0.4192
Longest Run	0.179	11	0.3083	0.3524	0.4367

Panel B: Falsification Test of Instrument: Relationship Between Pro-Government Takings Decisions and Composition of Takings Panels in Other Years, 1975-2008

Circuit-Year Level	Outcome: Proportion of Pro-Government Physical Takings Decisions$_t$			
	(1)	(2)	(3)	(4)
Minority Democratic Appointees per Seat$_t$	-0.701	-0.645	-0.694	-0.684
	(0.220)	(0.240)	(0.211)	(0.225)
Minority Democratic Appointees per Seat$_{t-1}$	0.554	0.535		
	(0.370)	(0.362)		
Minority Democratic Appointees per Seat$_{t-2}$		-0.525		
		(0.276)		
Minority Democratic Appointees per Seat$_{t+1}$			-0.0788	-0.0824
			(0.398)	(0.394)
Minority Democratic Appointees per Seat$_{t+2}$				0.306
				(0.581)
N	104	103	104	100
R-sq	0.077	0.089	0.064	0.057
Circuit-Year Level	Outcome: Proportion of Pro-Government Regulatory Takings Decisions$_t$			
	(5)	(6)	(7)	(8)
Judges with ABA scores of well-qualified or better per Seat$_t$	0.335	0.328	0.261	0.262
	(0.139)	(0.153)	(0.154)	(0.157)
Judges with ABA scores of well-qualified or better per Seat$_{t-1}$	-0.164	-0.159		
	(0.0912)	(0.0913)		
Judges with ABA scores of well-qualified or better per Seat$_{t-2}$		0.0287		
		(0.137)		
Judges with ABA scores of well-qualified or better per Seat$_{t+1}$			0.0115	0.00911
			(0.101)	(0.106)
Judges with ABA scores of well-qualified or better per Seat$_{t+2}$				-0.00270
				(0.111)
N	142	137	137	135
R-sq	0.217	0.208	0.209	0.207

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. Proportions of pro-government takings decisions are set to missing in circuit-years with no cases.

Appendix Table 3 -- Wild Bootstrap

	ΔLog Annual GDP					
Yearly Lags	(f1)	(t0)	(t1)	(t2)	(t3)	(t4)
Coefficient	0.005	0.011	0.018	0.008	0.011	0.007
Standard Error	(0.007)	(0.006)	(0.009)	(0.007)	(0.008)	(0.009)
Main percentile	0.452	0.077	0.046	0.205	0.148	0.472
Wild Bootstrap percentile	0.358	0.040	0.109	0.318	0.358	0.378

Notes: State-level GDP data are from the Bureau of Economic Analysis. Heteroskedasticity-robust standard errors are clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year. Wild bootstrap percentiles are displayed for 200 iterations.

Appendix Table 4: List of Regulatory Takings Appellate Precedent

Citation	Case Name	Circuit	Year	Pro-Property Owner
605 F.2d 1117	Willam C. H1s & Co. v. San Francisco	9	1979	0
613 F.2d 73	Chatham v. Jackson	5	1980	0
626 F.2d 966	FTC v. Owens-Corning Fiberglas Corp.	12	1980	0
616 F.2d 680	Rogin v. Bensalem Twp.	3	1980	0
632 F.2d 1014	Union Carbride Agricultural Products Co. v. Costle	2	1980	0
653 F.2d 364	Amer. Sav. & Loan Asso. v. County of Marin	9	1981	1
652 F.2d 585	Couf v. De Blaker	5	1981	0
665 F.2d 138	Devines v. Maier	7	1981	1
643 F.2d 1188	Hernandez v. LaFayette	5	1981	1
666 F.2d 687	Melo-Tone Vending, Inc. v. US	1	1981	0
660 F.2d 1240	Minnesota by Alexander v. Block	8	1981	0
645 F.2d 701	Nance v. EPA	9	1981	0
694 F.2d 476	Barbian v. Panagis	7	1982	0
684 F.2d 1301	In re Aircrash in Bali	9	1982	1
669 F.2d 105	In re Ashe	3	1982	0
671 F.2d 432	Nasser v. Homewood	11	1982	0
686 F.2d 1327	PVM Redwood Co. v. USA	9	1982	0
718 F.2d 789	Amen v. Dearborn	6	1983	1
710 F.2d 1097	Frazier v. Lownes County, Miss. Bd. Of Ed.	5	1983	0
707 F.2d 524	Kizas v. Webster	12	1983	0
703 F.2d 1141	Martino v. Santa Clara Valley Water Dist.	9	1983	1
706 F.2d 1130	Memorial Hospital v. Heckler	11	1983	0
707 F.2d 103	Ocean Acres Ltd. Partnership v. Dare Cty Bd. Of Health	4	1983	0
724 F.2d 1247	Peick v. Pension Ben. Guaranty Corp.	7	1983	0
711 F.2d 582	Price v. Junction	5	1983	0
718 F.2d 628	Rep. Indus. V. Teamster Joint Council No. 83	4	1983	0
749 F.2d 1396	Board of Trustees v. Thompson Bldg. Materials, Inc.	9	1984	0
734 F.2d 175	Coastland Corp. v. County of Currituck	4	1984	0
728 F.2d 876	Devines v. Maier	7	1984	0
739 F.2d 1562	Dirt, Inc. v. Mobile County Com.	11	1984	0
725 F.2d 695	Family Div. Trial Lawyers of Superior Ct - DC v. Moultrie	12	1984	1
729 F.2d 402	Hamilton Bank of Johnson City v. Williamson Cty Reg. Planning	6	1984	1
762 F.2d 1124	Keith Fulton & Sons v. NE Teamster & Trucking	1	1984	0
749 F.2d 541	MacLeod v. County of Santa Clara	9	1984	0
740 F.2d 792	Mountain States Legal Found. v. Clark	10	1984	1
700 F.2d 37	Park Ave. Tower Associates v. NY	2	1984	0
732 F.2d 312	Sadowsky v. NY	2	1984	0
736 F.2d 1207	Scott v. Sioux City	8	1984	0
765 F.2d 756	Sederquist v. Tiburon	9	1984	1
727 F.2d 1121	Silverman v. Barry	12	1984	1
739 F.2d 118	Terson Co. v. Bakery Drivers & Salesman Local 194	3	1984	0
727 F.2d 287	Troy Ltd. v. Renna	3	1984	0
749 F.2d 549	Trustees for Alaska v. US EPA	9	1984	0
771 F.2d 707	Keystone Bituminous Coal Ass'n v. Duncan	3	1985	0
764 F.2d 796	Rymer v. Douglas County	11	1985	0
780 F.2d 1448	Furey v. Sacramento	9	1986	0
833 F.2d 1270	Hall v. Santa Barbara	9	1986	1
799 F.2d 317	In re Chicago, M., S.P. & P. R. Co.	7	1986	0
828 F.2d 23	Citizen's Asso. Of Portland v. Internat'l Raceways, Inc.	9	1987	0
819 F.2d 1002	Cone v. The State Bar of Florida	11	1987	0
816 F.2d 907	Empire Kosher Poultry v. Hallowell	3	1987	0
809 F.2d 508	Gorrie v. Bowen	8	1987	0
834 F.2d 1488	Herrington v. County of Sonoma	9	1987	1
820 F.2d 982	In re Consolidated US Atmosheric Testing Litig.	9	1987	0

818 F.2d 1449	Kinzli v. Santa Cruz	9	1987	0
841 F.2d 872	Lake Nacimiento Ranch Co. v. County of San Luis Obispo	9	1987	0
861 F.2d 727	A.A. Profiles, Inc. v. Ft. Lauderdale	11	1988	1
854 F.2d 732	Adolph v. Fed. Emergency Mngment Agency	5	1988	0
840 F.2d 678	Austin v. Honululu	9	1988	0
847 F.2d 304	Calvert Invest., Inc. v. Louisville & Jefferson Cty Metro.	6	1988	0
837 F.2d 546	Carlin Communications, Inc. v. FCC	2	1988	0
841 F.2d 301	Lai v. Honolulu	9	1988	0
844 F.2d 172	Naegele Outdoor Advertising v. Durham	4	1988	1
851 F.2d 1501	Nat. Wildlife Fed. v. ICC	12	1988	1
842 F.2d 598	Pineman v. Fallon	2	1988	0
862 F.2d 184	Pinkham v. Lewiston Orchards Irrigation Dist.	9	1988	0
853 F.2d 145	Presault v. Interstate Commerce Comm.	2	1988	0
841 F.2d 107	SDJ, Inc. v. Houston	5	1988	0
873 F.2d 1407	Baytree of Inverrary Realty Partners v. Lauderhill	11	1989	0
865 F.2d 1395	Bennett v. White	3	1989	1
879 F.2d 316	Glosemeyer v. Missouri K.T. Railroad	8	1989	0
870 F.2d 529	Hoehne v. County of San Benito	9	1989	1
868 F.2d 335	In re Southeast Co.	9	1989	0
874 F.2d 1070	Jackson Ct Condos, Inc. v. New Orleans	5	1989	0
886 F.2d 260	Moore v. Costa Mesa	9	1989	0
876 F.2d 1013	Tenoco Oil Co. v. Dep't of Cons. Affairs	1	1989	0
902 F.2d 905	Centel Cable Television Co. v. Thos. J. White Dev. Corp.	11	1990	0
919 F.2d 1385	Conti v. Fremont	9	1990	0
920 F.2d 1496	Del Monte Dunes v. City of Monterey	9	1990	1
898 F.2d 573	Estate of Himelstein v. Ft. Wayne	7	1990	0
900 F.2d 783	GA Outdoor Advertising, Inc. v. Waynesville	4	1990	1
909 F.2d 608	Hoffman v. Warwick	1	1990	0
913 F.2d 573	Kaiser Dev. Co. v. Honolulu	9	1990	0
917 F.2d 1150	Lockary v. Kayfetz	9	1990	0
905 F.2d 595	Mehta v. Surles	2	1990	0
898 F.2d 347	Pinewood Estates of MI v. Barnegat Twp Lev Bd.	3	1990	1
914 F.2d 348	Rector, Wardens & Members of Vestry of St. Bart's Church	2	1990	0
907 F.2d 239	Smithfield Concerned Ctzns. for Fair Zng. v.Smithfield	1	1990	0
922 F.2d 498	Southern Pac. Transp. Co. v. L.A.	9	1990	0
911 F.2d 1331	Tahoe-Sierra Preservation Council, Inc. v. Tahoe Reg'l Planning	9	1990	0
895 F.2d 780	Western Fuels-Utah, Inc. v. Lujan	12	1990	0
948 F.2d 575	Azul Pacifico, Inc. v. L.A.	9	1991	0
941 F.2d 872	Commercial Builders of Northern CA v. Sacramento	9	1991	0
939 F.2d 165	Esposito v. SC Coastal Council	4	1991	0
922 F.2d 1536	Executive 100 v. Martin County	11	1991	0
935 F.2d 691	Federal Sav. & Loan Ins. Corp. v. Griffin	5	1991	0
939 F.2d 696	Leroy Land Dev. v. Tahoe Regional Planning Agency	9	1991	0
942 F.2d 668	McDougal v. County of Imperial	9	1991	1
945 F.2d 667	Midnight Sessions, Ltd. v. Philadelphia	3	1991	0
947 F.2d 1158	Nat. Advert. Co. v. Raleigh	4	1991	0
940 F.2d 925	Sam1d v. Dallas	5	1991	0
938 F.2d 951	Sierra Lake Reserve v. Rocklin	9	1991	1
973 F.2d 704	Azul Pacifico, Inc. v. L.A.	9	1992	0
953 F.2d 600	Cable Holdings of G. v. McNeil Real Estate Fund VI	11	1992	1
967 F.2d 648	Colorado Springs Prod. Credit Ass'n v. Farm Credit Admin.	12	1992	0
969 F.2d 664	Get Away Club, Inc. v. Coleman	8	1992	0
959 F.2d 395	Kraebel v. NYC Dep't of Housing Preservation & Dev.	2	1992	0
978 F.2d 1269	Nixon v. US	12	1992	1
968 F.2d 1131	Reahard v. Lee County	11	1992	0
959 F.2d 1268	Rogers v. Bucks Cty Dom Rel Section	3	1992	0

980 F.2d 84	Southview Assoc., Ltd. v. Bongartz	2	1992	0
2 F.3d 276	Armour & Co. v. Inver Grove Heights	8	1993	0
995 F.2d 161	Christenson v. Yolo County Bd. Of Supervisors	9	1993	0
5 F.3d 285	Gamble v. Eau Claire County	7	1993	0
987 F.2d 913	Garelick v. Sullivan	2	1993	0
1 F.3d 121	Hertz Corp. v. City of NY	2	1993	0
998 F.2d 680	Levald, Inc. v. City of Palm Desert	9	1993	0
989 F.2d 13	McAndrews v. Fleet Bank of MA	1	1993	0
985 F.2d 36	McMurray v. Commissioner	1	1993	0
985 F.2d 1488	New Port Largo v. Monroe County	11	1993	1
997 F.2d 604	Outdoor Sys., Inc. v. City of Mesa	9	1993	0
998 F.2d 1073	Tri-State Rubbish, Inc. v. Waste Management, Inc.	1	1993	0
995 F.2d 1179	United Wire, Metal & Mach. Health & Welfare Fund v. Morristown	3	1993	0
993 F.2d 962	Washington Legal Found. v. MA Bar Found.	1	1993	0
42 F.3d 1185	Barber v. Hawaii	9	1994	0
24 F.3d 1441	Bell Atl. Tel. Cos. v. FCC	12	1994	1
37 F.3d 468	Carson Harbor Village Ltd. v. City of Carson	9	1994	0
43 F.3d 1476	Christopher Lake Dev. Co. v. St. Louis Cty.	8	1994	1
14 F.3d 44	Lovell v. Peoples Heritage Sav. Bank	1	1994	0
19 F.3d 215	Matagorda County v. Russell Law	5	1994	0
21 F.3d 1214	Orange Lake Assocs. V. Kirkpatrick	2	1994	0
13 F.3d 1192	Parkridge Investors Ltd. Partnership by Mortimer v. Farmers Home	8	1994	0
18 F.3d 111	Res. Trust Corp. v. Diamond	2	1994	0
47 F.3d 832	Barrick Gold Exploration v. Hudson	6	1995	0
70 F.3d 1566	Clajon Prod. Corp. v. Petera	10	1995	0
59 F.3d 852	Dodd v. Hood River County	9	1995	1
57 F.3d 781	Hoeck v. City of Portland	9	1995	0
49 F.3d 1263	LB Credit Corp. v. Resolution Trust Corp.	7	1995	0
53 F.3d 478	LTV Steel Co. v. Shalala	2	1995	0
62 F.3d 449	Meriden Trust & Safe Deposit Co. v. FDIC	2	1995	0
65 F.3d 1113	Multi-Channel TV Cable Co. v. Charlottesville Quality Cable Corp.	4	1995	0
57 F.3d 505	Pro-Eco v. Board of Comm'rs	7	1995	0
67 F.3d 194	Youppee v. Babbitt	9	1995	1
101 F.3d 320	287 Corp Center Assoc. v. The Twp of Bridgewater	3	1996	0
89 F.3d 704	Bateman v. City of W. Bountiful	10	1996	0
89 F.3d 1481	Bickerstaff Clay Prods. Co. v. Harris County	11	1996	1
79 F.3d 516	Blue Diamond Coal Co. v. Sec of HHS	6	1996	0
85 F.3d 422	Broad v. Sealaska	9	1996	0
95 F.3d 1066	Corn v. City of Lauderdale Lakes	11	1996	0
75 F.3d 1114	Davon, Inc. v. Shalala	7	1996	0
95 F.3d 1422	Del Monte Dunes v. City of Monterey	9	1996	1
83 F.3d 45	Fed. Home Loan Mortg. Corp. v. NY State Div. Of Hous. & Comm. Renewal	2	1996	0
90 F.3d 306	Goss v. City of Little Rock	8	1996	1
74 F.3d 694	Kruse v. Village of Chargrin Falls	6	1996	1
90 F.3d 688	Lindsey Coal Mining Co. v. Chater	3	1996	0
83 F.3d 1531	NJ v. USA	3	1996	0
103 F.3d 690	Outdoor Graphics v. City of Burlington	8	1996	0
93 F.3d 301	Porter v. DiBlasio	7	1996	0
96 F.3d 401	Sinclair Oil Corp. v. County of Santa Barbara	9	1996	0
101 F.3d 1095	TX Manufactured Hous. Ass'n v. City of Nederland	5	1996	0
90 F.3d 790	United States v. 30.54 Acres of Land	3	1996	0
121 F.3d 695	Cape Ann Citizens Ass'n v. City of Gloucester	1	1997	0
110 F.3d 150	Eastern Enters. v. Chater	1	1997	0
126 F.3d 1125	Macri v. King County	9	1997	0
112 F.3d 313	McKenzie v. City of White Hall	8	1997	0
124 F.3d 1150	Richardson v. City & County of Honolulu	9	1997	0

121 F.3d 610	Villas of Lake Jackson v. Leon County	11	1997	0
130 F.3d 731	Waste Mgmt. v. Metropolitan Gov't	6	1997	1
136 F.3d 1219	Dodd v. Hood River County	9	1998	0
135 F.3d 275	Front Royal & Warren Cty Indus. Pk. Corp. v. Town of Front Royal	4	1998	0
147 F.3d 802	Garneau v. City of Seattle	9	1998	0
151 F.3d 861	Goss v. City of Little Rock	8	1998	1
138 F.3d 1036	Hidden Oaks v. City of Austin	5	1998	0
153 F.3d 356	Int'l College of Surgeons v. City of Chicago	7	1998	0
159 F.3d 670	Philip Morris v. Harshbarger	1	1998	1
145 F.3d 1095	San Remo Hotel v. City & Cty of San Francisco	9	1998	0
151 F.3d 1194	Schneider v. Cal Dep't of Corrections	9	1998	1
160 F.3d 834	South County Sand & Gravel Co. v. Town of S. Kingstown	1	1998	0
158 F.3d 729	Stern v. Halligan	3	1998	0
141 F.3d 1427	Vesta Fire Ins. Co. v. Florida	11	1998	1
195 F.3d 1225	Agripost, Inc. v. Miami-Dade County	11	1999	0
191 F.3d 1127	Buckles v. King County	9	1999	0
198 F.3d 642	Central States, SE and SW Areas Pension Fund v. Midwest	7	1999	0
198 F.3d 874	District Intown Props. Ltd. Pshp. v. D.C.	12	1999	0
175 F.3d 178	Houlton Citizens' Coalition v. Town of Houlton	1	1999	0
172 F.3d 22	Nat. Educ. Ass'n-Rhode Island v. Retirement Bd.	1	1999	0
172 F.3d 906	National Mining Ass'n v. Babbitt	12	1999	0
164 F.3d 677	Patriot Portfolio, LLC v. Weinstein	1	1999	0
170 F.3d 961	Quarty v. USA	9	1999	0
178 F.3d 649	Unity Real Estate v. Hudson	3	1999	0
224 F.3d 1030	Chevron USA, Inc. v. Cayetano	9	2000	0
214 F.3d 573	John Corp. v. City of Houston	5	2000	0
228 F.3d 998	Tahoe-Sierra Preservation Council, Inc. v. Tahoe Reg'l Planning	9	2000	0
227 F.3d 170	Traficanti v. USA	4	2000	0
226 F.3d 412	US Fid. & Guar. Co. v. McKeithen	5	2000	1
266 F.3d 487	Anderson v. Charter Twp. Of Ypsilanti	6	2001	0
254 F.3d 89	Bldg. Owners & Managers Ass'n Int'l v. FCC	12	2001	0
263 F.3d 286	Cowell v. Palmer Twp.	3	2001	0
267 F.3d 45	Philip Morris, Inc. v. Reilly	1	2001	0
271 F.3d 835	Wash. Legal Found. v. Legal Found. Of Wash.	9	2001	1
270 F.3d 180	Wash. Legal Found. v. Tex. Equal Access to Justice Found.	5	2001	1
306 F.3d 113	Barefoot v. City of Wilmington	4	2002	0
288 F.3d 375	Daniel v. County of Santa Barbara	9	2002	0
306 F.3d 445	Daniels v. Area Plan Comm'n	7	2002	1
285 F.3d 142	Deniz v. Municpality of Guaynabo	1	2002	0
307 F.3d 978	Esplanade Props. V. City of Seatthle	9	2002	0
312 F.3d 24	Philip Morris, Inc. v. Reilly	1	2002	1
289 F.3d 417	Prater v. City of Burnside	6	2002	0
284 F.3d 148	Sinclair Broadcast Group v. FCC	12	2002	0
276 F.3d 1014	United States v. Kornwolf	8	2002	0
342 F.3d 118	Santini v. Conn. Hazardous Waste Mgmt. Serv.	2	2003	0
345 F.3d 1083	Vance v. Barrett	9	2003	0
374 F.3d 887	Cashman v. City of Cotati	9	2004	1
363 F.3d 846	Chevron USA, Inc. v. Bronster	9	2004	1
365 F.3d 435	Coalition for Gov't Procurement v. Fed. Prison Indus.	6	2004	0
362 F.3d 512	Dakota, Minn. & R.R. Corp. v. S.D.	8	2004	1
366 F.3d 1186	Garvie v. City of Fort Walton Beach	11	2004	0
361 F.3d 934	Greenfield Mills, Inc. v. Macklin	7	2004	0
375 F.3d 936	Squaw Valley Dev. Co. v. Goldberg	9	2004	0
369 F.3d 882	Vulcan Materials Co. v. City of Tehuacana	5	2004	1

Appendix Table 5 - LASSO Instruments for Regulatory Takings Panels, 1979-2004

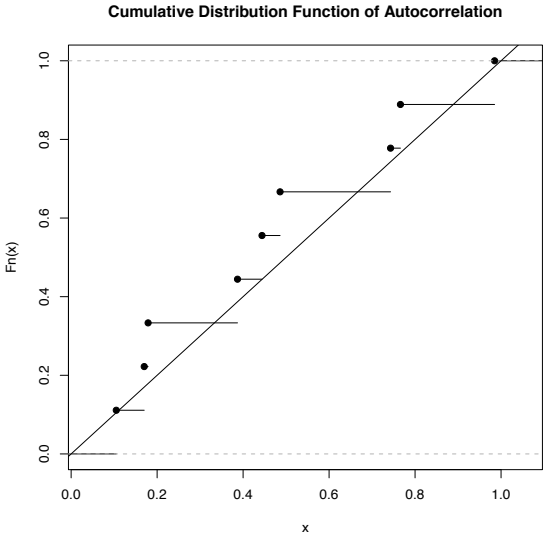
Panel A	Appellate-level LASSO Instruments
	Outcome: Pro-Takings Precedent
Fiserv (Zip-Year)	Secular * ABA score of well-qualified or better
GDP (State-Year)	Secular * ABA score of well-qualified or better
CPS (Individual-Year)	Secular * ABA score of well-qualified or better
FHWA (State-Year)	Secular and had prior government experience
Panel B	District-level LASSO Instruments
	Outcome: Presence of Appellate Case
Fiserv (Zip-Year)	Born in 1940s * Born in 1940s, Protestant Judge whose BA is from appointment state
GDP (State-Year)	Born in 1920s * Mainline Protestant who attended baccalaureate in state of appointment
CPS (Individual-Year)	Born in 1910s * Catholic, BA from appointment state * BA from appointment state
FHWA (State-Year)	Born in 1940s * Born in 1940s

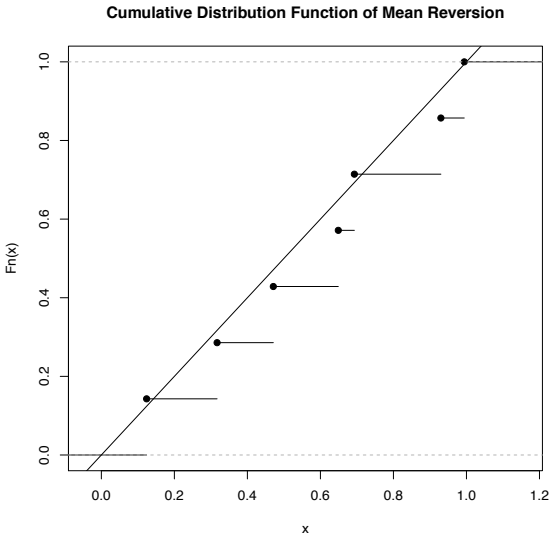
Notes: LASSO selected optimal instruments from the following judge characteristics and their interactions at the judge and circuit-year level for a total of 900 possible instruments: Democrat, male, male Democrat, female Republican, minority, Black, Jewish, Catholic, Secular, Mainline Protestant, Evangelical, baccalaureate (BA) from appointment state, public baccalaureate, JD from a public institution, having an LLM or SJD, elevated from district court, decade of birth (1910s, 1920s, 1930s, 1940s, or 1950s), appointed when the President and Congress majority were from the same party, ABA score of well-qualified or better, above median wealth, appointed by president from an opposing party, prior federal judiciary experience, prior law professor, prior government experience, prior assistant U.S. attorney, and prior U.S. attorney. The symbol ".*" indicates a circuit-year level interaction.

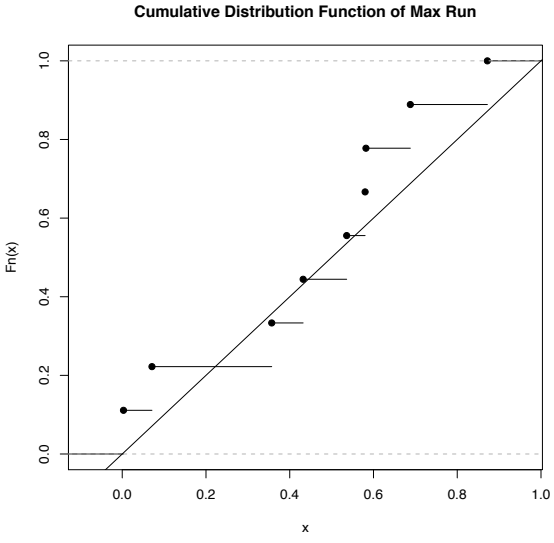
Appendix Figure 1: Randomization Check

Empirical Distribution of P-Values of Strings of Judge Characteristics

Minority Democrat







ABA scores of well-qualified or better