Expropriation, Inequality, and Growth: The Economic Impacts of Eminent Domain

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Abstract

Government takings of private property rights is the typical remedy to coordination breakdowns between numerous property right owners, but little is empirically known about the consequences of government takings. We embed a prominent set of theories in a model whose reduced form predictions isolate the channel through which government takings have their effects: insecure property rights, moral hazard, or public use. We then show how data collection from appellate and district courts, combined with the effective random assignment of U.S. federal judges, allows estimating two separate parameters of policy interest, one where the counterfactual is the opposite precedent and one where the counterfactual is no precedent. We use data on U.S. judicial biographies to implement a sparse model for estimating treatment effects with high dimensional instruments. Consistent with minority landowners being disproportionately condemned and undercompensated, 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. Decisions allowing takings of physical property rights result in 22% less federal compensation of condemnations and 26% greater non-residential relocation costs per state per year and increase growth by 0.2% while reducing minority home ownership and employment by 0.5% and 0.3% respectively. Decisions allowing uncompensated regulations, many of which involve environmental protection that limit property rights, have no effect on condemnations, initially decrease property prices, but increase economic growth in the medium-run by 0.7%, amounting to $2.2 billion per year. (*JEL* codes: R32, K11, R42, R52; *Keywords*: Property Rights, Takings, Regulation, Environment, Inequality)

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1 Introduction

The ability for governments to take property rights from individuals is hotly debated around the world. In India and China, fatal riots have followed government acquisitions of private land on behalf of commercial developers,[1] while in the former Soviet bloc, legislation allowing governments to take land for the establishment of privately-owned industrial parks is pending. In the U.S., the *Charles River Bridge* case of 1837 represents a watershed moment, when the state of Massachusetts abrogated exclusive rights to bridge traffic between Cambridge and Boston by building a free bridge nearby (Lamoreaux 2011). Variously referred to as eminent domain, compulsory purchase, compulsory acquisition, or expropriation by different legal systems,[2] a common question arises: can state taking of private property rights be justified? Coordination breakdowns between numerous property right owners frequently stymie socially optimal outcomes (Buchanan and Yoon 2000). Proponents argue that government exercise of eminent domain spurs economic growth through public goods provision, blight removal, and commercial development (*Kelo v. City of New London*, 545 U.S. 469 (2005)). Skeptics, however, worry that public choice incentives lead revenue-seeking governments to collude with private developers (Byrne 2005) at the expense of disadvantaged groups (see Justices O'Connor's and Thomas's dissents arguing that the *Kelo* decision allowing the city to take land from the poor on behalf of Pfizer was "Reverse Robin Hood") (Carpenter and Ross 2009; Frieden and Sagalyn 1989) and emphasize that government compensantion for land acquisition is less than fair market value (Chang 2010; Munch 1976). In recent years, contention surrounding government takings has risen to a new level: if governments simply regulate and restrict certain property rights, such as environmental protections that restrict the ability to develop land, the regulation can be considered a taking.[3] Liberty issues aside, little is empirically known about the consequences of government takings, despite a plethora of economic and legal theories regarding their potential consequences (Epstein 1985, 2008; Kaplow 1986; Blume, Rubinfeld and Shapiro 1984).

[1] 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/2011/02/05/nyregion/05metjournal.html, http://www.nytimes.com/2011/02/23/world/asia/23india.html, http://www.cga.ct.gov/2005/rpt/2005-r-0321.htm, 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.

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

[3] *Pennsylvania Coal Co. v. Mahon* (1922) established the doctrine of regulatory takings (see Appendix A). Examples of regulatory takings include zoning restrictions for the location of hotels (*Dexter 345 Inc. v. Cuomo*, 2011 WL 6015780) and regulations shortening the fishing year *(Vandevere v. Lloyd,* 644 F.3d 957).

Empirical studies to date have primarily focused on descriptive statistics. Data limitations have made it practically impossible to study the causal effects of eminent domain. Eminent domain is rarely exercised in a random fashion and few centralized sources of data document the condemnation of property rights across jurisdictions. Part of the difficulty is because various levels of government (e.g., local, state, and federal in the U.S.) are able to invoke the power of eminent domain. We sidestep this issue by focusing on court-made laws that make it harder or easier for subsequent government actors to take. The focus on the U.S. results in some loss of generality, but its common law system, random assignment of judges, and system of appellate courts with regional jurisdiction setting legal precedent for millions of people, allows us to isolate causal effects. 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 (what if *Kelo* had been decided in the opposite direction), of policy interest to a judge, and one where the counterfactual is no precedent (what if *Kelo* did not exist), of policy interest to an advocate or historian. 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. 2011), exploiting variation stemming from judges interpreting the facts and the law differently and in a manner correlated with their demographic characteristics.

Methodologically, we are motivated by the fact that at present, when judges face hard cases with no strong legal precedent, they typically rely on policy arguments but not formal models or empirical evidence of the effects of their decisions. We cannot ask judges to conduct prospective evaluations by randomizing decisions in the interest in legal science, so we explore how closely our empirical design tracks a randomized control trial (Duflo et al. 2007; Lee 2008) while also addressing common concerns about the use of randomization in the field (Deaton 2010). We investigate whether the empirical framework provides causal estimates of court precedent holding all else equal including unobserved factors, overcoming the basic issues of omitted variables and reverse causality. We also consider whether the exclusion restriction is likely to hold, the LATE interpretation of IV estimates is policy relevant, the general equilibrium or displacement effects are those that we would want to include, and the impulse response function is well-identified.

The sign of any effect of eminent domain on subsequent economic outcomes is ex ante theoretically ambiguous. We embed prominent theories of eminent domain in a simple model of takings. After documenting that our model is consistent with the predictions of models in the literature, we show how estimating the impact of eminent domain permits inference as to the predominant underlying mechanism, i.e., moral hazard, insecure property rights, or public use. In doing so, our study falls in the competing models class of studies that use random assignment in the field (Card et al. 2011). In classic law and economics models, because of the just compensation clause, property owners are over-insured: they do not pay the insurance premium on the insurance they receive in the event of a taking, a moral hazard problem that leads to over-investment (Blume, Rubinfeld and Shapiro 1984; Miceli and Segerson 1994; Innes 1997; Kaplow 1986; Cooter 1985). We show, however, that these results rely on the probability of takings being fixed and use the second-best as benchmark. Our empirical framework exploits exogenous variation in the probability of takings, so we use the first-best as benchmark. In law and development models, if the government does not compensate or uses the most conservative appraiser to calculate fair market value, the landowner receives less return on investment; and insecure property rights leads to under-investment (Besley 1995; Banerjee et al. 2002; Field 2005; Hornbeck 2010; Riddiough 1997). In recent macroeconomy models, the expropriability of capital and extractive capacity of the state can lead to faster economic growth (Aguiar and Amador 2011; Mayshar et al. 2011). While these models are developed for other contexts, we show, ignoring the public use benefits of takings, that if there is just compensation, increasing the likelihood of takings has no effect on investment, prices, GDP, or employment; any differences in outcomes would then be due solely to the public use projects. On the other hand, if there is over- or under-compensation, greater risk of takings leads to over- or under-investment and higher or lower property prices, but invariably leads to lower economic growth because of the distortion in investment incentives, unless the public use benefits counteract.

Consistent with minority landowners being disproportionately condemned and under-compensated, minority Democratic appointees, who may have had experience defending or identifying with these kinds of clients, are 20% more likely to strike down a physical taking while Republican prior U.S. Attorneys, who previously advocated on behalf of the government, are 18% more likely to uphold a physical taking. With this variation, we show that following pro-government physical takings precedent, local governments reduce compensation and are more likely to displace commercial tenants that are more costly to move: takings for federal highway projects result in 22% less compensation and 26% greater non-residential relocation costs per state per year.

We then present an analysis of zip-code level property prices, state GDP, home ownership, and individual labor market outcomes. Our baseline estimates indicate that rulings allowing takings of physical property rights spur economic growth and property prices by

0.2% points, but reduce minority home ownership and employment by 0.5% and 0.3% points, respectively, suggesting that, while public use projects may spur growth, these projects do not necessarily employ minorities whose homes or businesses were displaced by eminent domain. In contrast, rulings allowing uncompensated regulations, many of which involve environmental protection that limit property rights, have no effect on condemnations, initially decrease property prices, but increase economic growth in the medium run by 0.7% points, amounting to $2.2 billion per year. Our results on the effects of precedent are robust to controlling for the local effect of the taking (we code all of our cases to the zip code(s) where the alleged taking took place). We find, moreover, that accounting for the effect of the presence of a case along with its precedential effect (in other words, the counterfactual of no precedent rather than the opposite precedent) leads to much smaller estimates and, for physical takings, becomes negative. These negative effects could help explain popular unrest in response to high profile takings, but the vast majority of unlitigated takings occuring in the shadow of takings law may have a positive impact on economic growth.

Perhaps the closest studies preceding ours are Guidry and Do (1998), which finds that property transferred under eminent domain correspond to higher property prices in a cross-sectional analysis of a small sample of properties, and Collins and Shester (2011), which finds that the use of eminent domain following the Housing Act of 1949 corresponded to substantially larger increases in city-level income, property values, and employment rates than previously thought. While no empirical study of regulatory takings exist as far as we are aware, Quigley and Rosenthal (2005) conclude that as much as 54% of land value can be linked to land use regulations; other studies, however, have linked land-use regulations to higher or lower property values (Katz and Rosen 1987; Jaeger 2006; Beaton 1991; Nickerson and Lynch 2001), suggesting perhaps the need for better identication. Several studies use the random assignment of judges to evaluate the effects of case outcomes on litigants (Kling 2006; Chang and Schoar 2007), whereas in this paper and a series of related papers (Chen and Sethi 2011; Chen and Yeh 2011; Chen, Levonyan and Yeh 2011), we use the random assignment of judges to evaluate the effects of legal precedent on social and economic outcomes. Our findings in this paper indicate that judicial decisions, like taxes and expenditures, can have large impacts on the economy, a proposition that is inconsistent with the conventional view that legal decisions merely reflect rather than affect societal trends (Rosenberg 1993; Klarman 2004).

2 Model

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.

2.1 Background

As conceptual framework, we model a government actor, as opposed to a court 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 exogenous probability that the court allows a physical taking to occur and π_r is the probability that the court allows the regulation to occur. 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 ($C = 0$). While this choice to litigate or regulate (Shleifer 2010) may seem somewhat counter-intuitive, major doctrinal developments (Appendix A) and actual cases in our database (Appendix Tables 1.1 and 1.2) suggest that the boundary 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. With physical condemnation, the government must bring an *in rem* action, so court fees accompany every physical taking. A regulation places the burden on the property owner to seek redress. We allow some takings cost TC as additional wedge between the two decisions, for example, to build the public use project. Relative to physical takings, regulations impose little additional costs to the government. FC is the fixed cost of planning.

For physical takings, if the court finds for the plaintiff, no benefits or additional costs exist. For regulations, if courts find for the plaintiff and hold the regulation a taking, compensation is required.[4] We assume that C for regulatory takings is very small relative to B_r, since only a small fraction of property owners would seek redress and only a handful of land parcels

[4]For various reasons, courts are reluctant to simply reverse a regulation. Among them, 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).

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 if there is "just compensation," but the vast majority of decisions in our data focus on whether there is enough public use to justify the takings and do not address just compensation. This means in our welfare analysis, we use the first-best as benchmark, which varies the probability of takings, rather than the second-best, which varies the level of compensation. Note that we do not model the rich set of dynamics (e.g. externalities, loss of tax revenue, etc.) as a consequence of government takings, but simply collapse these dynamics into a single measure of benefit from government projects.

2.2 Landowner Investment

To validate our model, we first replicate results of the previous literature before proceeding to our estimation strategy. We evaluate how takings law affects the landowner's investment incentives and investigate when investment differs from the first best (what the social planner would choose in the absence of regulation) and second best (what the social planner would choose taking regulation as given) benchmarks. We assume risk neutrality. In this discussion, 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 invariably lead to lower growth unless compensation policy is exactly right.

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 $C = C(G, I)$. [5] Compensation 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:

[5]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 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.

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

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. [6]

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

Equation 5 indicates that the landowner always over-invests compared to the second best optimal investment, which replicates the result that any positive compensation increasing with investment will act as insurance toward takings risk, leading to over-investment (Blume et al. 1984; Cooter 1985; Kaplow 1986). The only way to eliminate over-investment in these

[6] 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.

models is to set $C_I(G, I) = 0$, which is completely contrary to the doctrine of "just compensation." Classical law and economics models, however, *take the probability of a taking as fixed.* We have exogenous variation in takings risk so our empirical framework 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.

2.3 Estimation

Neither investment nor its marginal returns, $C_I(G, I)$, are observable, but investment affects property prices and the aggregate investment of all landowners can affect local growth and employment, which are all observable. Overinvestment decreases growth, at least in the medium or long-run (Green 2003; Tsiddon 1992; Driffill 2003). The landowner perceives the probability of government action, π, 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, 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 $\frac{dI}{d\pi} = \frac{dI}{d\pi_p} = 0$. Notably, investment, property prices, GDP, and employment *should all be independent of the probability of a taking* and any differences in outcomes would be due solely to 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) leads to more under-investment only if there is under-compensation. Comparing to the first-best benchmark, insecure property rights lead to under-investment, consistent with predictions of law and development models (Besley 1995; Banerjee et al. 2002; Field 2005; Hornbeck 2010; Riddiough 1997). In the U.S. context, under-compensation is the presumption in the literature (Radin 1982; Fennell 2004) and especially among minority landowners, who tend to be disproportionately expropriated and receive less compensation (Carpenter and Ross 2009; Frieden and Sagalyn 1989). As the dissent in *Kelo* articulated, "extending the concept of public purpose to encompass any economically beneficial goal guarantees that these losses will fall disproportionately on poor communities. Those communities are not only systematically less likely to put their lands to the highest and best social use, but are also the least politically powerful." Over-investment relative to first best occurs only if there is over-compensation (i.e., $C_I(G, I) > 1$).

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. Making it easier for the government to exercise eminent domain would stimulate growth only if the social benefits, which may differ for regulatory and physical takings, exceed the possible distortions from the increased probability of taking. Furthermore, unequal distribution of social benefits may directly affect inequality.

3 Design of Study

3.1 Identification Strategy

Generally speaking, court decisions are endogenous and takings decisions are no exception; for example, 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 or that the compensation is just. Estimates would be spurious were we to only examine the correlation between appellate decisions and future property prices.

A foundational understanding of the U.S. judicial system 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." A district court sits in each locality, serves as the general trial court for its prescribed geographic area, where a jury is drawn to decide issues of facts. A judicial "circuit" is the larger geographic subdivision and encompasses between 5 and 13 judicial districts. Figure 1 displays district court boundaries in dotted lines and circuit court boundaries in solid lines. There are a total of 12 circuits in the United States and in each sits a single appellate court, which reviews decisions of the district courts in that circuit. A tiny fraction of cases, about 2%, get appealed again to the Supreme Court (which decides less than a hundred cases per year, while circuit courts decide many tens of thousands of cases per year), so these circuit courts are quite powerful, determining the vast majority of decisions each year that set precedent.

These circuit courts decide *issues of law* (rather than facts), determining whether the district court was in error and providing new interpretations or distinctions of pre-existing precedents or statutes. According to one view, appellate courts are continually finding new distinctions with which to expand or contract the space under which an actor would be found liable (Gennaioli and Shleifer 2007). (For an eminent domain example, in *Martino v. Santa Clara Valley Water Dist.*, 703 F.2d 1141, the Ninth Circuit held that it was possible for an ordinance requiring that the landowner obtain permits and establish dedications for a flood control project before the landowner could develop his land to constitute a taking. In *Moore v. Costa Mesa*, 886 F. 2d 260, which distinguishes *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. If there is a circuit split or highly unusual case, the Supreme Court of the United States can review the decision of the circuit court, but again, this is typically with some delay.

For each case, the circuit court *randomly assigns three judges* to sit as a panel (a formal test of random assignment is provided in the web appendix) 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. Some judges take a reduced caseload, but all are randomly assigned by a computer algorithm and are not revealed to the litigating parties until after they file their briefs, sometimes only a few days before the hearing, if there is a hearing. The random assignment of judges creates idiosyncratic year-to-year variation in the composition of judges sitting on any particular set of cases.

A large literature has now documented that judges exercise *judicial discretion* in interpreting the facts and the law in a manner often correlated with biographical characteristics. For example, black judges have been found to vote differently from white judges on issues where minorities are disproportionately affected, such as affirmative action, race harassment, unions, and search and seizure cases (Kastellec 2011; Chew and Kelley 2008; Scherer 2004).

These five aspects of the U.S. judicial system, the *geographic* nature of the *binding precedent* and the *random assignment of judges* exercising *judicial discretion* in deciding *issues of law,* allow us to construct a setting akin to a randomized experiment in the establishment of precedent across different regions of the United States.

Information Transmission We should expect to see an effect of appellate judicial decisions if judges follow precedent and appellate decisions on the margin make it easier for subsequent plaintiffs to bring and win suit against the government.[7] We might then expect property buyers, sellers, and government actors to respond to appellate decisions (Berliner 2003; Nader and Hirsch 2004), whether through newspaper publicity (Pastor 2007; Eagle 2007; Sandefur 2004), advocates, lawyers, or information consultants highlighting the risk of suit after major appellate decisions. Landowners increase their perception of takings risk after major eminent domain decisions (Nadler and Diamond 2008; Nadler et al. 2008). Government actors self-report adjusting their acquisitions or land-use regulations to avoid exposure to costly litigation (Administration 2002; Pollak 2001).[8]

3.2 Data

Our empirical analysis draws on several sources of data. Data on eminent domain cases come from established datasets as well as our own data collection. All cases were double-coded. We collected physical takings cases in both appellate and district courts from 1975-2008 and regulatory takings cases from 1979-2004. The sample includes only cases that had substantive decisions about takings, rather than cases that were decided only on procedural grounds. We focus on decisions in federal appellate courts and not state courts, where judges are not randomly assigned. Takings compensation claims are typically litigated in state courts before

[7] For example, *Penn Central Transportation Co. v. New York City* (1978) asserting that regulations that do not cause a landowner to discontinue use of their property to their benefit do not constitute a regulatory taking would make it easier for subsequent government actors to enact regulations without fear of suit.

[8] For example, the City of Sacramento refrained from exacting an easement for a bicycle path specifically because of the planner's attention to regulatory takings appellate precedent.

proceeding to federal courts (*Williamson County Regional Planning Commission v. Hamilton Bank* (473 U.S. 172)). As a matter of practice, state courts follow the precedent set in federal courts, which decide on larger doctrinal issues. State attorneys general 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 (Drees 1997). In results available on request, we find that our 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 state statutes outside the circuit. Some state supreme courts found the need to distinguish pro-plaintiff federal appellate precedent many years after the initial decision, suggesting that federal appellate precedent was presumed to be influential among state courts after the decision.

Our outcome variables include condemnation statistics by state governments for highway projects, zip-code level house price indices, state GDP, employment outcomes, and homeownership status. Our judicial biographical database for both appellate and district court judges updates and expands previous work. Data collection efforts, datasets, and variable construction are described in further detail in the web appendix. Our final sample includes 220 regulatory takings cases and 134 physical takings cases. The complete list of cases and summary statistics are also included in the web appendix.

3.3 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. Outcomes include: change in log quarterly property prices at the zip code level, change in log yearly state GDP, and housing and employment status, such as whether an individual lives in public housing, whether an individual lives below the poverty line, individual's hours worked last week, and log real weekly earnings.[9] Law_{ct} is the proportion of appellate cases with a pro-plaintiff (landowner) outcome when there is a case

[9]Changes rather than levels is preferred when outcomes are highly persistent and to avoid relying on cointegration assumptions (Bond et al. (2010)). The web appendix illustrates how when we can calculate first-differences, first-differences and level regression results are qualitatively similar.

but 0 when there are no cases.[10] We control for $\mathbf{1}[M_{ct} > 0]$, the presence of an appeal (M_{ct} is the number of eminent domain cases). We show that our estimates are invariant to the inclusion or exclusion of: circuit fixed effects, C_c, time fixed effects, T_t, circuit-specific time trends, $C_c * Time$, a vector of observable unit characteristics, X_{ict}, depending on the unit being observed (for example, at the individual level: age, gender, educational attainment, and race, which each enter as dummies with the exception of age, and at the state level: state fixed effects); and time-varying circuit-level controls, W_{ct}, the characteristics of the pool of judges available to be assigned.

Laws might not be immediately capitalized in prices (DellaVigna and Pollet 2007) and agents may need time to adjust to judicial decisions; alternatively, the effects of a law may fade as expectations or statutory regimes adjust or circuit splits are resolved. We therefore use a distributed lag specification that includes four years (16 quarters) of lags and one year (4 quarters) lead (n=-1 to 4) and test robustness to the number of lags and leads. The use of leads serves as a randomization check. Controlling for lags also addresses the fact that treatment and control occur repeatedly over time within a circuit. Individual lag coefficients help distinguish between level and growth effects: in first differences, a level effect would be inferred from a reversal in the sign of the coefficients and cumulative net effect of coefficients to be 0 (Dell et al. 2012 forthcoming). The average and joint significance of the lag coefficients are displayed in the bottom of our tables.

In principle, we have 408 (1,632) experiments for physical takings (34 years x 12 circuits (x 4 quarters)). We cluster standard errors at the circuit level to address serial correlation of ε_{ict}.[11] In robustness checks, we also execute a wild bootstrap (Cameron, Miller and Gelbach 2008) and a Monte Carlo simulation, where we randomly assign the legal variation to another circuit, to address the small number of clusters.

3.4 Moment Condition for Causal Inference

With judge-made law, there is so much cross-fertilization across different areas of legal doctrine so if different, but related, doctrinal areas have independent effects on economic outcomes,

[10] In robustness checks, we account for the number of decisions, not the proportion, by using the number of cases as weights in this regression. We did not consider quadratic or non-monotonic functions of the number of pro-plaintiff decisions, however, because there is less than one eminent domain decision per circuit per year.

[11] Bester, Conley and Hansen (2011 forthcoming) suggest scaling up the cluster t-statistic with relatively few clusters (e.g., 12 circuits), but the scaling is very close to one with large N. Barrios et al. (2010) indicates that the use of clustered standard errors, along with the random assignment of treatment, also addresses possible spatial correlation in the errors.

social changes may be misattributed to one legal rule when many legal rules are changing simultaneously. This phenomenon, along with other social trends that may drive both the decision to appeal and the appellate decision itself, means that it is important to seek variation in Law_{ct} and $\mathbf{1}[M_{ct} > 0]$ that is uncorrelated with social trends or legal developments. The figures in the web appendix depict the intuition for our identification of Law_{ct}; we exploit the variation that arises from the random deviation in the composition of judges assigned to eminent domain cases. While the composition of judges in the circuit pool varies smoothly over time, in circuits and years when eminent domain cases receive an unexpectedly high proportion of judges who tend to be pro-plaintiff in takings cases, we see "treatment" in the corresponding eminent domain precedent for that circuit and year. We also use the random assignment of judges to identify $\mathbf{1}[M_{ct} > 0]$; one district court judge is randomly assigned per case (Bird 1975) and the correlation between district judge demographic characteristics and their reversal rates has been previously documented (Haire, Songer and Lindquist 2003; Sen 2011; Barondes 2011; Steinbuch 2009). If some district judges are less likely to be reversed, the lower reversal rate could discourage litigating parties from pursuing an appeal.

Were we only interested in the contemporaneous effect of Law_{ct}, the moment condition for causal inference is $\mathbf{E}[(N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct}))\varepsilon_{ict}] = 0$. The greater the excess proportion of judges who tend to be pro-plaintiff in takings cases, N_{ct}/M_{ct}, the more pro-plaintiff is the takings precedent in that circuit-year. $\mathbf{E}(N_{ct}/M_{ct})$ is the expected proportion of judges who tend to be pro-plaintiff in takings cases. We attribute the degree to which outcomes change to this excess proportion.[12] We are interested in the distributed lag effect, however, and circuit-years without cases would dramatically reduce sample size. To remedy this, we seek to construct an instrumental variable whose moment conditions imply the original moment condition. Consider an instrument, $p_{ct} - \mathbf{E}(p_{ct})$. The moment condition for this instrumental variable is: $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$, where p_{ct} is the proportion of judges who tend to be pro-plaintiff in takings cases and p_{ct} is defined as 0 when there are no cases:

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

When $\mathbf{1}[M_{ct} > 0] = 1$, $p_{ct} = N_{ct}/M_{ct}$ returns the original moment condition. When $\mathbf{1}[M_{ct} > 0] = 0$, then $p_{ct} = 0$ and $\mathbf{E}(p_{ct}) = 0$, so $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$. However, these two conditional

[12] Variance in the excess proportion varies with M_{ct}, $\mathbf{E}(N_{ct}/M_{ct})$, and the number of judges available to be assigned (the smallest circuit has 8 judges and the largest has 40). This heteroskedasticity only affects the standard errors, not the point estimates (since the moment condition remains satisfied), and robust clustered standard errors address heteroskedasticity.

moment conditions do not imply $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$ unconditionally. The presence of appellate cases, $\mathbf{1}[M_{ct} > 0]$, may be a function of ε_{ict}, so it needs to be controlled. After controlling for it, then $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$ unconditionally.

We can further simplify our instrument by observing that $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}[\mathbf{E}(p_{ct})\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}(p_{ct})\mathbf{E}(\varepsilon_{ict}) = \mathbf{E}[p_{ct}\varepsilon_{ict}]$, so we can ignore $\mathbf{E}(p_{ct})$. We have now constructed our instrumental variable, p_{ct}.

The inclusion of $\mathbf{1}[M_{ct} > 0]$, however, threatens the moment condition in a distributed lag specification. Whether there are any cases in a given year may respond to previous years' realization of the instrument. Once we identify both $\mathbf{1}[M_{ct} > 0]$ and Law_{ct}, however, then the impulse response function is well-identified. Additional lags and leads of our fitted $\mathbf{1}[M_{ct} > 0]$ and Law_{ct} would be orthgonal to other years' fitted $\mathbf{1}[M_{ct} > 0]$ and Law_{ct}. Of course, additional lags that are important predictors of the outcome would improve statistical precision of the other lags, though this comes at the cost of fitting additional parameters, the coarsening of the first stage because all lags and leads of all instruments are used for every endogenous variable, and the mechanical dropping of data to allow for additional lags and leads.

3.5 Distinguishing Local Effects from Precedential Effects

We distinguish between the local effect of a taking from the precedential effect of making it easier for subsequent governments to take to understand growth under the shadow of (rather than actual) expropriation. Appellate eminent domain decisions affirm or overturn a local taking of private property rights potentially affect a large portion of the circuit as some regulations are at the state level. We thus code the corresponding zip codes for the regulation or condemnation for each case in our database. The web appendix shows a map for the location of original takings decisions and displays estimates of the specification $Y_{ict} = \beta_0 + \beta_1 Law_{ct} + \beta_2 LocalLaw_{ict} + \varepsilon_{ict}$, 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 only apply this specification to property price data since our other datasets are not available at the zip code level.

3.6 Heterogeneous Treatment Effects and Inequality

Race is the leading source of heterogeneity and there are few datasets on eminent domain to suggest alternative predictors of takings risk except to document that low-valued land is undercompensated (Chang 2010; Munch 1976), so we restrict our attention of heterogeneous treatment effects to interactions between takings decisions and race. We considered using quantile regressions in price and income but individual level unobservables are likely not rank-invariant to potential treatment status. If low-valued land is disproportionately targeted by government actors for eminent domain and eminent domain raises property prices, then the same parcels that have relatively higher property prices without getting treatment (of court decisions that make it easier for government actors to take) may not be the ones who would have relatively higher property prices with the treatment.

3.7 Interpretation

The exclusion restriction assumption for causal inference of the 2SLS estimates is likely to hold. The identity of judges sitting on eminent domain panels is not likely directly to affect economy-wide outcomes that are of interest except through the appellate precedent alone. Even if individuals are aware of the identity of judges, subsequent courts are supposed to take the decisions as given rather than discount them according to the identity of the judges.

Our estimates are internally valid conditional on everything that happened before the judges got assigned to the case. Under monotonicity assumptions, we have a LATE (Local Average Treatment Effect) interpretion of the instrumental variables estimates.[13] That is, only cases where there is enough controversy to allow judicial biographical characteristics to matter are affected by our instrumental variable. These cases may very well be the difficult decisions that set new precedent, and the sorts of cases in which judges interested in the empirical consequences of decisions seek guidance (Posner 1998; Breyer 2004). Our empirical strategy thus estimates two parameters that are of policy-interest. Law_{ct} is of policy interest to a judge interested in the causal effect of a decision on a case already in front of her. The counterfactual is a decision made in the opposite direction. This effect would be particularly informative when facing a difficult case without strong legal precedent; these are perhaps the very decisions where personal policy preferences play a role. This parameter directly relates to the theoretical model's π_p and π_r.

The second parameter is of policy interest to a historian or advocate interested in the causal effect of an appeal, $\mathbf{1}[M_{ct} > 0]$. The counterfactual is the absence of an appeal. Conceptually, the historian may want to know what would it be like if *Kelo* did not exist, rather than if *Kelo* was decided in the opposite direction, and an advocate may want to know

[13] We focus on LATE estimates rather than MTE (Marginal Treatment Effect) because we lack a policy reason to distinguish cases that are more likely to be pro-taking to see if they have bigger or smaller impacts on economic growth.

the effect of pushing forward for an appeal. In terms of our theoretical model, the presence of a decision, however, does not typically affect π_p and π_r, though it may affect π if there is an expressive effect from knowing the existence of an appeal. The web appendix provides additional mathematical calculations for the policy parameters and shows how to use them to assess the magnitude of the impact of legal decisions on social change. The results of these policy calculations are displayed in the lower half of our tables.

An additional advantage of identifying $\mathbf{1}[M_{ct} > 0]$ is that we address potential displacement effects: government actors may defer public use projects until a favorable legal regime. Roads only need to be built once and regulations are only issued once. The absence of a case, however, serves as a "supercontrol" (Crepon et al. 2011). The difference between pro-taking (treatment) and pro-plaintiff (control) decisions captures both the treatment effect and a displacement effect of building a road during a treatment year rather than a control year. We observe what would have happened regardless of the judge's decision; if we still see one road being built, then we know the original $treatment - control$ estimate was pure displacement. To subtract the displacement effect, we would want to subtract from$treatment - control$ the effect of "no case" (i.e., *adding* the effect of the presence of a case). The result gives us the pure treatment effect minus displacement.

Factor mobility[14] or a tendency for circuits to follow each other, albeit with some delay, would lead to underestimates as treatment and control groups become more similar.[15] In general, allowing factor mobility to reduce the size of the estimates is appropriate as such an allowance would incorporate the general equilibrium effects of the law. We leave for future work a spatial lag specification that estimates the effect of other circuits' precedent as such an estimation would likely need structural assumptions to reduce the number of parameters that need to be estimated.

We considered using subsequent citation counts as a measure of case impact but citation counts can be endogenous to the economic response to the decision. We also considered an event-study approach, but the high number of cases means multiple events occur serially or even in the same circuit-year. Finally, we considered assessing the proportion of social change that is due to court-made law, but this calculation assumes that other societal factors do not have effects on social change in the opposite direction.

[14]For person-level outcomes, labor mobility across circuits could cause outcomes in employment or housing to converge.

[15]To see this, consider the following numerical example. We measure 3 pro-plaintiff decisions in treatment and 3 pro-takings decisions in control, but in reality, the precedent including peer effects has the strength of 2 pro-plaintiff decisions and 2 pro-takings decisions. We underestimate the true effect since we measure 10 units of outcome change in response to 6 units of law change rather than the actual 4 units of law change.

3.8 First Stage

The number of possible combinations of judges or demographic characteristics on a judicial panel is very large, because judicial demographics are heterogeneous within each circuit and a circuit may have as many as forty judges in the pool of judges available to be assigned. With this very large number of possible panel compositions, our strategy benefits from a surfeit of experimental variation. Choosing among a large number of instruments, however, is a challenging statistical issue involving a trade-off between increasing the power of the first stage regression (Angrist and Imbens 1995) and avoiding the weak instruments problem with additional instruments (Stock and Yogo 2005).

Race of judges has been found to correlate with decision-making in affirmative action, race harassment, unions, and search and seizure cases in a manner consistent with the interests of their racial category (Chew and Kelley 2008; Kastellec 2011; Scherer 2004; Brudney et al. 1999). Since eminent domain is coventionally viewed as having disparate racial impacts, we may expect race of judges to predict decision-making. Those with previous experience litigating on behalf of the government may also view government takings in a different light than those without such experience. The web appendix discusses the first stage in more detail, showing, for example, 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. In extended tables, we show how judge-level differences in voting patterns aggregate to the case-level and then to the circuit-year level. The joint F-statistics at the level of our analyses range from 26 to 42. Nonparametric local polynomial estimates further show that the first stage relationships are not driven by outliers. The web appendix also shows a falsification test of the instrument, examining one or two years before and after the true instrument to see if judicial decision-making is related with judicial assignment in the off years. We also use LASSO to address the issue of instrument selection (Belloni et al. 2011) and to verify the robustness of our main IV results to alternative instruments. An extended explanation is provided in the web appendix. Several tests of randomization are also discussed in the web appendix.

We considered individual judge fixed effects from prior takings decisions and uni-dimensional measures of judicial attitudes (e.g. liberal or conservative), but individual judge fixed effects from prior takings decisions would be imprecisely estimated since each judge hears only a handful of eminent domain cases and uni-dimensional measures of judicial attitudes lack statistical power.

4 The Impact of Eminent Domain

4.1 Takings in the Shadow of Expropriation

We begin our analysis by examining the local government response to appellate takings decisions. If courts are more likely to uphold a taking, government actors may either take more property rights, provide less compensation per taking because of increased bargaining power, or take different types of property consistent with increased state capacity to take property rights. Using a dataset of land appropriations for transportation projects that is uniformly collected across the U.S., we find that local governments are more likely to displace commercial landowners, increase the average cost of relocating them, and reduce compensation in the years after pro-government physical takings decisions, while regulatory takings decisions have a weak or non-existent effect that is not robust.

On average, each year for the five years after a pro-government physical takings precedent when there is one takings decision results in 22% less federal compensation (Table 1 Column 2), 2% more non-residences being displaced (Column 4), and 26% greater non-residential relocation costs per state per year (Column 6). The average federal compensation is \$20 million, number of displaced non-residences is 56, and non-residential relocation costs is \$800 thousand per state per year, so these effects are economically important. Similar results are obtained with reestablishment and replacement expenses as with relocation costs. Greater relocation costs of non-residents suggest that government actors proceed to displace larger commercial property owners after expansion of state capacity.[16]

4.2 House Prices

Pro-government physical takings precedent cause an increase in property prices. On average, each year for the five years after a pro-government physical takings precedent when there is one takings decision results in 1.2 percentage points more price growth (Table 2 Column 2). To help put this magnitude in perspective, 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) and a \$100 per capita difference in Housing Act of 1949 grant funding was associated with a 7.7% difference in 1980 median property value and five extra years of legislation enabling government acquisitions was associated with approximately 4% higher median property values (Collins and Shester 2011). The impact of physical takings

[16] After 2003, not all states consistently report data. When we treat non-reporting as missing and include all available data in an unbalanced panel, the results are similar.

precedent on property prices is robust across IV specifications (Table 3), data aggregation levels (Table 3), controls (Table 4), and lag structure (Table 5). Four years of leads show no association and controlling for local effects do not affect the main estimates (Table 5).[17] The lags do not sum to 0, suggesting that the effects of physical takings precedent persist in the medium run, i.e., they look more like growth effects rather than level effects. Full tables of robustness checks are organized in the web appendix.

4.3 Inequality in Housing

Our model suggests that those landowners who are disproportionately affected by takings and who are undercompensated would be adversely affected by decisions making it easier for the government to take. Even if developers create jobs, it is not clear how the jobs would be distributed, especially if development projects favor businesses and sectors with fewer minorities. We therefore investigate whether eminent domain has a disparate impact on the housing and employment outcomes of minority groups, as feared by many legal observers and as suggested by the voting patterns correlated with a judge's demographic background.

We being with the observation that 52% of minorities (72% of whites) own a home (Table 6). The effect of pro-government physical takings precedent on minority home ownership relative to non-minority home ownership is negative and jointly significant, as shown in the lagged version of the interaction coefficients (Table 6). Pro-government takings decisions when there is one takings decision reduce the probability of minority home ownership by 2.5% relative to non-minorities. In absolute terms, pro-takings decisions reduce minority home ownership by 2.1% points (Column 2). Results on the probability of living in public housing and living below the poverty line indicate similar adverse effects for minorities and are shown in the web appendix.

4.4 Inequality in Employment

Turning to employment outcomes, employment status increases among whites by about 1.2% per year following a pro-government physical takings decision when there is one decision (Table 6 Column 4). However, these decisions disproportionately hurt the labor market outcomes of minorities both relative to whites and in absolute terms. The employment probability of

[17]Different instrumental variables at the appellate level and instrumenting with district judge assignment (Table 3), data aggregation at the individual unit or circuit-year level (Table 3), controls such as circuit-specific time trends, time-varying judicial pool characteristics, individual unit characteristics (Table 4), different numbers of lags and up two four years of leads (Table 5), and instrumenting and controlling for the local impacts of decisions (Table 5) do not affect the inferences.

minorities relative to whites decreases by 2.1% on average per year in the four years after a pro-takings decision when there is one decision. Results on hours worked and earnings are included in the web appendix. In unreported analyses, we find that these latter effects are mostly capturing the extensive margin. Restricting the analysis to labor force participants, the results become much smaller on every margin.

Our model suggests that reductions in housing and employment for minorities may result from insecure property rights, but we cannot rule out the possibility that the public use benefits themselves are unequally distributed. The adverse effects among minorities contrast with the overall employment increase of 0.007 for the entire population,[18] suggesting that the economic benefits of takings may dominate on average, but at the expense of some minority groups.

4.5 Economic Growth

Decisions making it easier for the government to take also have positive effects on economic growth. On average, each year for the five years after a pro-government physical takings decision when there is one takings decision results in 1.1% points faster growth (Table 2 Column 4). The positive effect on economic growth is consistent with the general increase in employment. This finding is indicative of the public use channel spurring growth and overcoming distortions that may arise from either over- or under-investment. Among some groups, however, the evidence is more consistent with the under-investment channel, such as among minorities who tend to be undercompensated and consequently experience worse housing and employment outcomes, even when the public use benefits are included.

4.6 The Economic Effects of Regulatory Takings Precedent: Uncompensated Takings

In contrast to physical condemnations, which require compensation, pro-government regulatory takings decisions make it easier to regulate without having to compensate landowners. With this undercompensation, theory predicts underinvestment to occur, observed through negative price and economic growth effects unless the public benefits of a regulatory action outweigh these other effects. Overall, pro-government regulatory takings decisions spur growth in house prices. On average, each year for the five years after a pro-government regulatory

[18]To calculate, multiply the non-interacted average lag effect 0.012 by 0.78, the proportion of the population that is white, and add the absolute decline of 0.009 multiplied by 0.22, the proportion of the population that is non-white.

takings decision when there is one takings decision results in 0.5% points faster growth in property prices (Table 7 Column 2) and 1.6% points faster economic growth (Column 4). We observe, however, a pattern of a negative response in initial years followed by net positive growth by years 3 and 4 when we use log price index (levels rather than first-differences) as the outcome (web appendix). In contrast to physical takings, the effects of regulatory takings precedents on inequality are generally smaller, less statistically significant, and not robust (web appendix).

4.7 Interpretation

Our calculations (displayed at the bottom of the tables) indicate that compared to conditional effects (where the counterfactual is the opposite precedent), the unconditional effects of takings decisions (where the counterfactual is no precedent) are far smaller.[19] For example, the typical conditional effect of pro-government physical takings decisions taking into account the typical number of cases in a given circuit-year amounts to a 2% point increase in property price and economic growth (Table 2 Columns 2 and 4), but the typical unconditional effect, which incorporates the effect of the presence of cases, is a 0.5% point increase.

Another interesting finding, however, is that the unconditional effects of all takings decisions tend to be negative (0.5%-1.4% point decrease in Tables 2 and 7 Columns 2 and 4). One interpretation may be that takings cases that reach the appellate courts involve exceptionally large or wasteful government projects, while the vast majority of unlitigated takings, however, may actually have a positive impact on growth. This perspective could also help explain popular unrest in response to high profile takings.

There is suggestive evidence that the effects of eminent domain are primarily experienced by densely populated areas. Our main results are robust to conducting analyses separately for metropolitan areas only as well as restricting analyses to only those zip codes with zip-code specific price indices (results not displayed). The story shifts slightly with population weights (web appendix). The impact of physical takings precedent is similar whether

[19]We considered and reject several reasons other than displacement effects for the smaller unconditional effects. First, opposing decisions, pro-government or pro-plaintiff, may have effects of opposite signs, in expectation. A second potential mechanism is the reluctance of local business owners to invest in the surrounding area given uncertainty before the resolution of the pending appellate decision. We control for the actual decision, however. Third, some district judges may write strong opinions that are more likely to be appealed but are also more likely to influence precedent in a manner that threatens local developers, who are more likely to appeal eminent domain decisions (in the Auburn Courts of Appeals database, of the appellate court decisions since 1975 involving property, the government is appealing the decision only 20% of the time, and of the 855 appellate decisions since 1975 involving regulation, a government is appealing the decision only 22% of the time). District judges' opinions generally have no binding precedential effect, however.

these weights are used or not, but the impact of regulatory takings precedent switches sign when population weights are used. This switching of signs indicates that pro-government regulatory takings precedent, making it easier for the government to regulate without compensation (typically regulations that protect the environment), has negative effects on house prices and growth in densely populated, urban areas whereas the positive effects are experienced primarily in more rural areas.

5 Conclusion

A growing body of work documents the long-run consequences of state institutions on growth and inequality (Acemoglu et al. 2001; Dell 2010). Our paper focuses on state seizure of private property rights as one channel through which institutions can have long-lasting consequences. We build a simple model of takings embedding the theoretical predictions of prior work and derive conditions under which different empirical outcomes may be observed. Assembling a dataset on judicial biographies and takings decisions in U.S. Circuit and District Courts and using an identification strategy exploiting random variation in appellate precedent through the judicial assignment process, our study estimates the effect of two dimensions of eminent domain law on economic outcomes in the United States, that of physical takings precedent, which is often related to urban renewal and development strategies, and of regulatory takings precedent, which is often related to acts of environmental protection.

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 show that over-investment results only when property rights holders are over-compensated and that the results of eminent domain models indicating that any compensation leads to over-investment rely on a constant probability of takings. We find that rulings making it easier to take physical property rights lead to a higher incidence of takings, lower compensation, and the taking of property that is more expensive to relocate. These rulings also increase economic growth and property price growth by 0.2% points and reduce minority home ownership and employment by 0.5% and 0.3% points respectively. Rulings in favor of the government in regulatory takings cases, which make it easier to regulate without having to compensate landowners, increase growth in property prices by about 0.2% points and increase economic growth by 0.7% points in a typical circuit-year. The net effect on economic growth taking into account the presence of the precedent is negative, which may reflect selection of undesirable government exercise of eminent domain into litigation at the appellate level (Roback 1982; Malani 2007). Consistent with minority landowners being disproportionately condemned and undercompensated, physical (but not regulatory) takings precedent strongly and robustly negative impacts minority labor market outcomes both relative to non-minorities and in absolute terms.

We leave for future research the investigation of specific government projects that stem from increased state capacity, the exploration of geographic heterogeneity rather than racial heterogeneity, and the understanding to what degree our estimates may be underestimated due to growth effects that come from, for example, highways connecting different circuits, which can stimulate trade and growth in both circuits. Another interesting dimension of future research is whether raising or changing the bargaining procedure surrounding compensation for low-valued or minority-owned land would ameliorate eminent domain's disparate impact. A final dimension of future research is to study whether judges make decisions based on a dynamic optimization problem vis-à-vis state actors balancing strategic public choice considerations around an optimal policy control function as well as their impact on the economy (Benabou and Tirole 2010; Buera et al. 2011).

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A 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."

A.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.

A.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.

B Data

Our empirical analysis draws on several sources of data on eminent domain cases—established datasets as well as our own data collection. Sunstein et al. (2006) collect data on all appellate regulatory takings published decisions from 1979-2004. We apply a similar methodology to collect appellate physical takings decisions from 1975-2008. We also collect all district court cases involving regulatory and physical takings. Judicial biographies come from a number of sources. Our outcome variables are condemnation statistics for state acquisitions to build highways, property values (house price indices) at the zip code level from the Fiserv Case-Shiller Weiss data, state GDP from the Bureau of Economic Analysis, and housing and employment outcomes from the Current Population Survey.

B.1 Legal Cases

We obtained data on all appellate regulatory takings published decisions from 1979-2004 from the authors of Sunstein et al. (2006). The cases were identified by shepardizing (tracking the citations of) the following landmark Supreme Court decisions; most takings cases would cite one or more of these cases: *Lucas v. South Carolina Coastal Council*, 505 U.S. 1003 (1992); *Nollan v. California Coastal 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).[20] 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.

We apply a similar methodology to collect appellate physical takings decisions from 1975-2008. We shephardized *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);[21] *Kelo v. City of New London*, 545 U.S. 469 (2005); *Yee v. City of Escondido*, 503 U.S. 519 (1992). This data includes a range of decisions regarding the use of eminent domain for development, a government-built dam that flooded land, sewer construction that deprived landowners of well water, and the government diversion of a river. Our physical takings data collection actually goes back to the 1950s, but our outcomes dataset begins in 1975. This extension does allow us to estimate models with more distributed lags without loss of observations. Note that some cases are hard to define as physical or regulatory takings

[20] We exclude decisions of the U.S. Court of Federal Claims.

[21] We restricted the cases citing *Loretto* to those that discussed whether the government had physically invaded or was present on the property.

even for courts, and will invariably appear on both lists. Appendix Tables 1.1 and 1.2 provide the list and coding of the cases.

Following Sunstein et al. (2006), we code a vote as pro-plaintiff (landowner) if the judge voted to grant the party alleging a violation of the Takings Clause any relief. The sample includes only cases that had substantive decisions about takings, rather than cases that were decided only on procedural grounds. Appendix Figure 1 plots the quantity of eminent domain cases that were decided pro-plaintiff or pro-defendant (government) during this time period. On average there are 0.71 regulatory takings panels per circuit-year for a total of 220 cases and 0.33 physical takings panels per circuit-year for a total of 134 cases; a sizeable portion of circuit-years, 54%, had no regulatory takings panel; 74% of circuit-years had no physical takings panel (Appendix Table 1.3).

In addition to appellate takings cases, our third and fourth datasets comprise all district court cases involving regulatory and physical takings. On Westlaw, we shepardize the same Supreme Court cases that we used to construct the appellate databases. This resulted in 498 regulatory takings cases from 1979-2004 and 635 physical takings cases from 1975-2008.[22]

Our final data collection effort involves mapping each of the appellate decisions to the zip code or zip codes where the alleged taking took place (alleged regulatory takings can affect multiple zip codes). The result of these efforts are displayed in Appendix Figure 2.

B.2 Judicial Biographies

We collect information on judge characteristics from several sources. We begin with the Appeals Court Attribute Data and District Court Attribute Data compiled by Zuk, Barrow, and Gryski.[23] These data cover nearly all judges appointed to federal appeals courts and federal district courts from 1789 to 2000 and provide partial information through 2004. The dataset includes information on vital statistics, geographic history, education, occupational history and governmental positions, military service, religion, race, gender, political affiliations, and other variables. For appellate judges with missing information, we first filled in their entries with data from the Auburn District Court database, provided that the judge had occupied a district court seat prior to 2000. Because these data do not cover many George W. Bush appointees, we used the Federal Judicial Center website[24] for information on a judge's birth, geographic origin, gender, education, occupational history and political appointments. We

[22]We use only cases decided by district court judges and exclude recommendations by magistrate judges because litigants cannot directly appeal a magistrate judge's recommendation (28 U.S.C. § 636(c)(1)).

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

[24]http://www.fjc.gov/history/home.nsf.

obtained religion data on Reagan- and post-Reagan judicial appointees from Goldman and from Sisk[25] and searched transcripts of Congressional confirmation hearings and other official or news publications on Lexis to fill in the rest of the missing religion variables. We coded the judges whose religions remained missing or unknown as having no publicly known religious affiliation. The average circuit-year has 17.81 judges available for assignment to panels. Appellate-level judges typically sit on three-judge appellate panels, though some judges in the appellate-level pool come 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 black judges per seat (or 0.18 black judges on a 3-judge panel), 0.06 minority Democratic appointees per seat, and 0.07 prior U.S. Attorney Republican appointees per seat (Appendix Table 1.3). We calculate the expectations based on the frequency the typical senior judge sits on cases and weigh senior judges accordingly.

B.3 Condemnation Data

We use annual state-level statistics on real property acquisitions, condemnations, compensation, and displacement expenses for 1991-2009 from the Federal Highway Administration.[26] 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 (49 CFR Part 24). The statistics relate to the basic procedures that states must follow under the Uniform Act. To acquire property, the governments must offer just compensation to the owners and give the owners reasonable time to consider the offers. Condemnation proceedings occur only after the parties are unable to reach an agreement through negotiations required by the statute, including the option to pursue administrative settlements. Around 80% of the property acquisitions are settled before the government pursues a condemnation proceeding. In a property acquisition, displaced residents are eligible to receive reimbursements for relocation expenses and the added costs of obtaining replacement housing, and displaced businesses are eligible for moving and reestablishment expenses, up to specific ceilings.[27]

We focus on available proxies for aspects of condemnation intensity. The total number of parcels acquired and parcels acquired by condemnation can be first measures for the governments' activities in the wake of takings precedent. Compensation amounts can reflect both

[25] 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

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

[27] http://www.fhwa.dot.gov/realestate/cndmst.htm

the government's bargaining power as well as general economic trends in the types of properties the government is seeking. We analyze the aggregate amounts of compensation paid or otherwise made available to the property owner on all parcels acquired, whether through open market purchase, condemnation, or administrative settlement. We also examine relocation costs, which may be better measures of the intensity of government actions rather than parcels condemned (or acquired) since the latter does not account for the size of the parcel or the number of people and businesses affected. The literature occasionally uses condemnation rate, the ratio of parcels condemned to parcels acquired. That measure, however, is less likely to be informative since state officials may feel empowered to acquire more parcels after a pro-government court decision, and if the number of protestors remains the same, condemnation rates may fall.[28]

B.4 Property Prices Data

Our main outcome variables are property values (house price indices) at the zip code level from the Fiserv Case-Shiller Weiss data, which cover the entire United States. We use these data to construct a panel of about 40,000 zip codes followed quarterly from 1975 to 2008. The Fiserv Case-Shiller Weiss indices are based on repeat sales data on single-family homes. In geographic areas that do not have a valid Case-Shiller price index, Fiserv splices in the corresponding Federal Housing Finance Agency (FHFA) index. The FHFA series is a quarterly, weighted, repeat-sales single family house price index based on repeat mortgage transactions handled by Fannie Mae or Freddie Mac. Because the Fiserv index requires a significant number of transactions in an area, the zip code-specific price indices are concentrated in metropolitan areas.[29] 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. As a check of our data, if the mean dependent variable of 1.2% (Table 2 Columns 1-2) is annualized, the average yearly change is about 5%, which is close to average local GDP growth (Table 2 Columns 3-4) and to annualized growth in other studies using the same price data (Mian and Sufi 2009). In robustness checks, we weigh the zip codes using zip code specific population estimates calculated for 2005 from the U.S. Census.[30] Other datasets through which we would have liked to study land development, population growth, gentrification, construction,

[28] Compensation by type of acquisition is not broken down into separate categories. Also, all states in certain years report only the compensation amounts paid from federal funds. This include expenses incidental to transfer of title and exclude appraisal costs, negotiator fees and other administrative expenses.

[29] Data based on transactions, however, may not appropriately capture land value if the type of land being sold changes in the shadow of eminent domain. We leave this question for future research.

[30] The Census data documentation is located at: http://www.census.gov/prod/cen2000/doc/sf1.pdf.

and investment, as a consequence of eminent domain, are only available decade by decade or are proprietary, as far as we are aware.[31]

B.5 Labor, Housing, and GDP Data

We use the March Current Population Survey (CPS) for housing outcomes, such as whether an individual owns or rents, whether an individual lives in public housing, and whether an individual lives below the poverty line. We use the Merged Outgoing Rotation Groups (MORG) CPS for employment outcomes, such as weekly earnings, amount of time worked, and employment status. 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, which lets us match the state of residence to the circuit having legal jurisdiction. We restrict our sample to individuals between the ages of 18 and 65 and weight our analysis with March CPS person weights for individual housing outcomes and MORG earnings weights for employment outcomes. 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. In robustness checks, we drop individuals not employed or not in the labor force. Data on GDP by state were obtained from the Bureau of Economic Analysis; estimates are aggregated across all industries by year.[32]

C Calculation of Policy Parameters

We are able to causally identify the two separate parameters because we collect data at both the appellate and district court level and employ random assignment of judges at both levels. In describing them here, we dub the two parameters, perhaps unconventionally, as the *conditional* and *unconditional* effect of Law_{ct}. The *conditional* effect of Law_{ct} conditions on the presence of a case, $\mathbf{1}[M_{ct} > 0]$, already in front of the judge. The *unconditional* effect adds the effect of Law_{ct} and $\mathbf{1}[M_{ct} > 0]$. For example, to calculate the effect of 1 pro-government decision when there is only 1 decision in that circuit-year, we would need to add the effect of $\mathbf{1}[M_{ct} > 0]$ with the effect of Law_{ct} to obtain the *unconditional* estimates of going from 0 to 1 pro-government decision. To calculate the effect of n pro-government decisions when

[31] An alternative dataset is from Zillow, but it is only available starting in the late 1990s. In any event, between 2000 and 2006, property price growth is correlated at around 0.95 between the two datasets (Guerrieri et al. 2010).

[32] http://www.bea.gov/regional/gsp/default.cfm#download.

there are m decisions, we would need to add $\mathbf{1}[M_{ct} > 0]{+}n/m{*}Law_{ct}$. In our tables, we show the distributed lag coefficients for *conditional* effects of Law_{ct}. We also show the average lag coefficients of Law_{ct} and of $\mathbf{1}[M_{ct} > 0]$, their respective joint tests of significance, and joint tests of significance for the distributed lags of $Law_{ct}{+}\mathbf{1}[M_{ct} > 0]$, which is dubbed the *unconditional* effect. To obtain cumulative effects when the outcome is in first-differences, all of these calculations could be multipled by four (the number of lags that are represented in the average lag) or the individual lags could be summed.

In interpreting the magnitudes, we make a separate distinction for the *typical* effect. This refers to the causal effect of the typical number of pro-government takings appellate decisions in a circuit-year. For example, to get the *typical* conditional effect, we multiply the conditional effect of 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 $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of circuit-years with an appellate takings case. Both $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$ and $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$ are displayed in Appendix Table 1.3.

For unconditional effects, we calculate the *typical* effect of pro-government decisions, pro-plaintiff decisions, and all decisions. These are, in turn: $\mathbf{1}[M_{ct} > 0]{*}\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]{+}Law_{ct}{*}\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]$, $\mathbf{1}[M_{ct} > 0]{*}\mathbf{E}[\mathbf{1}[Proplaintiff_{ct} > 0]]$, and $\mathbf{1}[M_{ct} > 0]{*}\mathbf{E}[\mathbf{1}[M_{ct} > 0]]{+}Law_{ct}{*}\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]{*}\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$. The first two formulas account for pro-government decisions and pro-plaintiff decisions occuring in the absence of the opposite decision, which is likely with less than one case per circuit-year. If these decisions occur in the same circuit-year frequently, only the third formula has a meaningful interpretation. The results of these calculations are displayed in the last four rows of the results tables.

For the reader interested in calculating the effect of 1 pro-government decision in a typical circuit-year, the formula is: $\mathbf{1}[M_{ct} > 0]{+}Law_{ct}/\mathbf{E}[M_{ct}|\mathbf{1}[M_{ct} > 0]]$. $\mathbf{E}[M_{ct}|\mathbf{1}[M_{ct} > 0]]$ is obtained from dividing $\mathbf{E}[M_{ct}]$, the typical number of appellate takings panels, by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of circuit-years with an appellate takings case. Both $\mathbf{E}[M_{ct}]$ and $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$ are also displayed in Appendix Table 1.3.

D First Stage

Do different outcomes result in eminent domain cases from being assigned judges with different background characteristics? The only prior study of this question documents that political affiliation alone does not predict decisions in eminent domain cases Sunstein et al. (2006). This lack of correlation with political affilation may be due to the fact that the Republican party platform has historically been more pro-growth (commercial development) and pro-individual property rights (libertarian on economic issues) and these tendencies cut in opposite directions. Republican appointees who are prior U.S. Attorneys, however, would have advocated on behalf of the government and be more likely to see things from the government perspective. In contrast, minority Democratic appointees may have prior experience serving on behalf of the poor and minorities, whose properties are disproportionately condemned (Carpenter and Ross 2009; Frieden and Sagalyn 1989). Of course, it need not be the case that judicial background characteristics is the only reason for the different decisions: litigants may tailor their oral arguments depending on the judge that they are assigned. Appendix Figure 3 roughly depicts the intuition for our 2SLS identification strategy, in which we exploit the random variation that arises from using the random deviation in the actual number of black judges per seat in regulatory takings cases.

Appendix Table 1.4 shows that minority Democratic appointees are 20% less likely to vote in favor of the government in physical takings cases while Republican appointees who are prior U.S. Attorneys are 18% more likely to vote in favor of the government (Panel A). All analyses in this section cluster standard errors at the circuit level. Similar patterns hold at the case level; an additional minority Democratic appointee per seat on a three-judge panel decreases the chances of a pro-takings decision by 57% while an additional Republican prior U.S. Attorney per seat increases the chances of a pro-takings decision by 68% (Panel B). At the circuit-year level, an additional minority Democratic appointee reduces the proportion of pro-takings decisions by 62%, while an additional Republican prior U.S. Attorney per seat increases the proportion of pro-takings decisions by 93% (Panel C). Both of these effects are large and economically significant and indicate the possibility that the mere presence of a judge with a particular decision-making tendency can influence her peers. The circuit-year level estimates differ slightly from the case level since cases are not evenly distributed across circuit-years (not every circuit-year has a case and cases can bunch up unevenly across circuit-years).[33] The estimates and statistical significance are robust regardless of whether the circuit-years with no cases are dropped or are dummied and the proportion of pro-takings decisions and judge type per seat are set to 0 for those circuit-years with no cases. The R-square increases

[33]For an example of how a coefficient can differ between circuit-year and case level, suppose there are 4 cases, one case each with 0, 1, 2, or 3 judges who are black, and suppose that the panel makes a pro-government decision when there are 3 Republican prior U.S. attorneys. If 1 circuit-year has the case with 0 Republican prior U.S. attorneys and the other circuit-year has the remaining 3 cases, the coefficient at the circuit-year level is 0.5 (0.5 = difference in percent pro-takings/difference in Republican prior U.S. attorneys assigned per seat) but when the 1 circuit-year with the case has the case with 1 Republican prior U.S. attorney judge, the coefficient at the circuit-level is 1.5.

significantly since we now replace missing values with 0s for both the instrument and the endogenous variable. The F-statistic is 9 and increases with the inclusion of controls up to 19. The first stage analysis is similar for the circuit-quarter level and the F-statistic ranges from 12 to 13. At the level of our analysis, merged with price data, the joint F statistic on the two instruments is 43 (Panel E). The estimates are slighly different at the analysis-level because of the differing numbers of zip codes per circuit. F-statistics are above the conventional threshold for weak instruments (Stock and Yogo 2005).

To check whether our linear specifications miss important aspects of the data, Appendix 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 the 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-U.S. Attorney judges and pro-government decisions while it is decreasing, though less sharply so, for minority Democratic appointee judges and pro-government decisions. Neither relationship is driven by outliers. These figures also show the tremendous variation across circuits and years, which will be useful in estimation.

We conduct an identical analysis for physical takings. Alleged regulatory takings disproportionately affect business entities, which constitute the largest share of regulatory takings plaintiffs (Stein 1995); black judges may be less likely to favor the plaintiff in regulatory takings challenges relative to white judges, as regulatory takings plaintiffs are likely to be non-poor and non-minority. Appendix Table 1.5 shows that black judges are 11% more likely to vote in favor of the government (pro-takings) in regulatory takings cases (Panel A). At the case level, an additional actual black judge per seat on a three-judge panel increases the chances of a pro-takings decision by 33% (Panel B). At both the judge level and case level, point estimates and statistical significance increase with controls for circuit and year fixed effects and the expected judge type per seat. At the circuit-year level, an additional black judge per seat increases the proportion of pro-government regulatory takings decisions by 40% (Panel C). The F-statistic is 6.9 and increases up to 26.6 with the inclusion of controls such as fixed effects for circuit and year, expected black judges per seat, and circuit-specific time trends. The R-square does not change much with the inclusion of these controls. The first stage analysis is similar for the circuit-quarter level (Panel D). At the level of our analysis, the F statistics again increase with the inclusion of fixed effects and additional circuit-year controls up to 17 (Panel E). A falsification of the instrumental variables shows that this kind of legal variation is not related to the instrument in the one or two years before and after the true instrument (Appendix Table 1.6). Non-parametric estimates of the first stage are displayed in Appendix Figure 4. Similar results obtain with minority judges as instrumental variable instead of black judges.

E LASSO

Some econometricians recommend larger first stage F-statistics to ensure that the first stage is sufficiently strong, such as F stat=25 or 50 to allow for heteroskedasticity and serial autocorrelation (Olea and Pflueger 2010). LASSO presents a way to optimally extract information from the combinatorially many instrumental variables available for use. Using LASSO (least absolute shrinkage and selection operator) in the first stage presents several advantages relative to using OLS. While OLS has low bias, it also has two disadvantages. 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: changing the data a bit results in different subsets of covariates deemed important. LASSO is a sparse model, which solves both of these problems. Formally, LASSO modifies OLS by minimizing the sum of squares subject to the sum of the absolute value of the coefficients being less than a constant. The nature of this constraint tends to set some coefficients to exactly 0 and hence reduces model complexity. Intuitively, LASSO gives interpretable models by imposing a data penalty for having too many covariates. In addition, LASSO ensures stability in instrument selection, making it an effective tool in selecting optimal instrumentals from a large number of valid instruments. Because it selects optimal instruments, LASSO enhances statistical precision when using the random assignment of judges. Belloni et al. (2011) show that the increased uncertainty due to selecting among many instruments does not show up to first order.

To construct our potential LASSO instruments, we use 30 biographical characteristics[34] and their interactions at the judge level and panel level (for example, for the combination of

[34] 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.

"black and Democrat," we examine the number of black Democratic appointees per seat for the *judge* level interactions and examine the number of Democrat appointees per seat multiplied by the number of black judges per seat for *panel* level interactions) yielding a total of 900 possible instruments. The instruments chosen by the LASSO procedure are listed in Appendix Table 1.7. For example, at the circuit-year level, the LASSO procedure selected Democrat prior assistant U.S. Attorneys for regulatory takings. Note that the number of observations per circuit-year or the number of years of data varies across outcomes. The F statistic with LASSO instruments is 38, representing a 100% improvement over the non-LASSO first stage F-statistics displayed in Column 5 of Panel C in Appendix Tables 1.4 and 1.5.

On a separate note, the use of the LASSO instruments provides a check of over-identification. 2SLS estimates derived from different judicial characteristics should be similar assuming that judicial panel composition affect economic outcomes in the same manner regardless of the type of judicial panel. We also consider a similar set of biographical characteristics and instrumental variables for the district judges and use LASSO to identify an exogenous component of the existence of an appeal.

F Randomization

Our empirical strategy involves using the proportion of judges with significantly different decision-making tendencies to approximate a true experiment when being assigned to eminent domain cases. This requires that appellate judges be randomly assigned to takings appeals. While random assignment is the standard procedure according to court administrators, some scholars argue that certain circuits have not always followed this (Hall 2010). Even if judges are randomly assigned, the decision to publish an opinion may introduce non-randomness. For example, the decision not to publish may be a compromise among judges who disagree about the correct outcome (Law 2005; Wald 1999). If minority Democratic appointees publish and Republican prior U.S. attorneys choose not to publish physical takings decisions, then a correlation may arise between the egregiousness of the eminent domain case and the judicial panel composition, which could reintroduce possible endogeneity between social trends related to the egregiousness of eminent domain cases and the panel composition of published cases. If panel composition significantly affects the decision to publish, we might expect panel composition of published cases to be serially correlated.[35]

[35]The same potential bias occurs with judges granting motions for summary judgment, in which case the opinion might not include a citation to the important case that categorizes the opinion as an eminent domain opinion.

Another reason to check for randomization is that the editorial decision to be included in Westlaw or Lexis is left to the discretion of the individual companies for unpublished cases (many cases designated by judges to be unpublished are actually published and their impact on precedent is debateable) and this editorial discretion may be endogenous to social trends. A further reason to check for randomization is because if judges strategically cite important precedent, then legal data collected through shephardizing cases may reintroduce bias. Finally, the possibility of differential rates of settlement upon the announcement of the judicial panel could reintroduce bias. Appellate judges are revealed to parties very late in the process, usually after briefs are filed. Parties are unlikely to settle, however, after filing briefs because the relatively short interval between learning panel members' identities and announcement of the judges' decision imposes small additional costs relative to the cost of litigation prior to learning the judges' identities.

All of these concerns that threaten identification of the effect of judicial decisions are reduced, if not eliminated, if we observe that the appellate instrument is as good as randomly assigned at the circuit-year level, conditional on having an appeal. We check the randomization assumption in two ways. First, surveys of appellate courts indicate that the assignment of judges to panels is random (Chen and Sethi 2011). In some courts, two to three weeks before the oral argument, a computer program is used to randomly assign available judges, including any visiting judges, to panels that will hear cases. In other courts, random assignment of panels occurs before the random assignment of cases. Panels of judges are set up to hear cases on a yearly basis, randomly assigned together by computer program and given dates for hearings. There are "holes" left in some of the panels by the program, where visiting judges are plugged in. Occasionally, if a panel of judges has previously looked at a case, it will be sent back to them (for example, if it was remanded to resolve one issue, etc.). If a judge must recuse himself, the case is taken off of the calendar and placed back in the pool for reassignment. Chen and Sethi (2011) also shows that case characteristics as determined by the lower court are orthogonal to the appellate instrument.

As a second randomization check, we examine whether the sequence of proportions of judges with significantly different decision-making tendencies is like a random process. Appendix Figure 3 suggests visually that panel composition is not serially correlated. Formally, the general approach to assessing randomness is analogous to a Fisher exact test, except that we use simulations. The methodology we follow is:

1. Propose a statistic that can be computed from the sequence of numbers of black judges 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 black judges 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. Some sources suggest that courts do batch cases dealing with similar issues to one panel in order to dispose of cases more quickly and without duplication of effort (Wallace 2005).

While this process generates a collection of p-values, it is not intuitively obvious what the rejection criteria should be. Since p-values from a truly random process with a sufficient number of possible states is uniformly distributed, even with just 10 units and 3 statistics, the probability of not having even one p-value less than .025 or greater than .975 is only about 21%. With a truly random process, 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.) Of course, since there are only 12 units, we would not expect a kernel density estimate to "look" uniform. We use Kolmogorov-Smirnov Test to test whether the empirical distribution of p-values approaches the CDF of a uniform distribution using the one-sided critical value with n = 12.[36] The intuition is to simply add up the space between the 45-degree line representing a uniform distribution and the p values displayed in Appendx Figure 5. We plot the empirical distribution for our 3 test statistics and one set of instruments each for regulatory takings and physical takings in Appendix Figure 5. Appendix Table 1.8 confirms the visual intuition that our p-values are uniformly distributed for all 6 tests.

G Web Appendix

Organization of the figures and tables is as follows:

Figures

Section I – Research Design

[36] http://www.ciphersbyritter.com/JAVASCRP/NORMCHIK.HTM#KolSmir.

Section II – Physical Takings

2.1 - House Prices

A. Robustness Check Across IV Specifications and Aggregation Levels

B. Robustness Check Across Controls, Clustering, and Weights

C. Robustness of IV Distributed Lag Estimates Across Controls, Clustering, Weights, Lag Structure, Leads, and Local Effects

2.2 - Growth

A. Robustness Check Across IV Specifications and Aggregation Levels

B. Robustness Check Across Controls, Clustering, and Weights

C. Robustness of IV Distributed Lag Estimates Across Controls, Clustering, Weights, Lag Structure, Leads, and Local Effects

2.3 - Inequality

A. Housing Inequality

B. Employment Inequality

2.4 - Condemnations

Section III – Regulatory Takings

3.1 - House Prices

A. Robustness Check Across IV Specifications and Aggregation Levels

B. Robustness Check Across Controls, Clustering, and Weights

C. Robustness of IV Distributed Lag Estimates Across Controls, Clustering, Weights, Lag Structure, Leads, and Local Effects

D. Robustness of IV Estimates in Levels

3.2 - Growth

A. Robustness Check Across IV Specifications and Aggregation Levels

B. Robustness Check Across Controls, Clustering, and Weights

C. Robustness of IV Distributed Lag Estimates Across Controls, Clustering, Weights, Lag Structure, Leads, and Local Effects

3.3 - Inequality

A. Housing Inequality

B. Employment Inequality

H Threats to Validity

In this section, we describe the results of a number of robustness checks. We investigate all outcomes discussed in the paper, but due to space constraints, we focus more attention below on house prices.

H.1 Leads and Lag Structure

Our main specifications include a one-year lead of takings precedent, but we also vary the number of lags and leads and use up to four leads. We examine to what extent economic outcomes predict the random assignment of judges (in the IV specification, where the law changes should be truly exogenous). The one-year lead in the OLS regression for regulatory takings indicates that pro-government regulatory takings decisions have a statistically significant and positive relationship with the previous year's house price growth (Appendix Table 3.1A Column 1). This correlation suggests that when property prices improve, judicial panels are more likely to rule that a regulation is allowed. With IV, however, the lead coefficients are generally not statistically significant and only a fraction of the magnitude of the lag coefficients.

The point estimates of 4 years of leads are small in magnitude and neither statistically significant (last four columns of the last line of Part G in Appendix Tables 2.1C, 2.2C, 3.1C, and 3.2C) individually or jointly. The standard errors are similar in magnitude to the lag effects; the lead coefficients are near 0 rather than being inprecisely estimated. In one specification, the two-year leads are jointly significant, but the average yearly lags remain quite strong.

The point estimates are quite robust to varying the lag structure (Part G in Appendix Tables 2.1C, 2.2C, 3.1C, and 3.2C). We display results using only 1 lag, only 2 lags, 2 leads and 4 lags, or 1 lead and 5 lags.

H.2 Covariate Controls and Outlier Circuits

Our main specification includes circuit and year (quarter-of-year, when feasible) fixed effects. Our robustness checks in Appendix Tables 2.1B, 2.2B, 3.1B, 3.2B, 2.1C, 2.2C, 3.1C, and 3.2C add circuit-specific time trends (row A), remove circuit and year fixed effects (row B), and add time-varying characteristics of the circuit pool of judges available for assignment (row D). In unreported results, the results change little when we control for lagged dependant variables.

Together, these sensitivity checks establish that the average conditional lag effect of pro-physical takings decisions on house price growth ranges from 0.006 to 0.017; the lags are always jointly significant at the 1% level. The individual point estimates are positive, significant, and similar in magnitude to the main price results. Pro-government regulatory takings precedent has a negative initial effect that is eventually overcome by a net positive response.

We also exclude one circuit at a time. The physical takings results are quite stable; the regulatory takings results are fairly stable, but the joint significance of the lag effects varies.

H.3 Instrumenting For Non-Random Presence of Takings Appeals and LASSO Instruments

Results using district IV with the main IV cases are shown in the main appendix tables for house prices, employment and housing outcomes, and GDP growth (Appendix Tables 2.1A, 2.2A, 3.1A, 3.2A, 2.3A, 2.3B, 3.3A, and 3.3B). Estimates using the district IV may be less precise than the estimates without district IV because the LASSO-selected district IV have, at worse, F-statistics of around 8, just below the conventional threshold for strong instruments. Estimates may be further weakened because we greatly increase the number of endogenous variables to 12 and the number of instrumental variables to 36. The off-year (e.g., contemporaneous appellate instrument on lag pro-takings precedent) and off-level (e.g., district instrument on pro-takings precedent) instruments are statistically insignificant, but the cumulation of off-year/off-level coefficients may be an issue.

The estimates for physical takings decisions are robust. The point estimates of the lagged price growth effects of pro-government physical takings decisions are comparable to those in the main IV specification for the first two years. Averaged across four years, the lag effect on prices is slightly smaller in magnitude at 0.007 and is jointly significant. Estimates of the impacts of physical takings decisions on minority housing and employment outcomes as well as GDP are similarly robust in the joint effects, though the point estimates for GDP are less precise. Regulatory takings regressions with district IV are a bit more sensitive: the average lag effects on house prices are comparable in magnitude and the individual coefficients are similar in magnitude, but their joint significance weakens. Occasionally the lead coefficient is statistically significant when there is no district IV, but the lead is no longer significant when district IV is used.

H.4 LASSO Instruments and Aggregation Level

We verify that the IV estimates are robust when using LASSO-selected instruments and when we aggregate the data to the circuit-year level (i.e. collapse the outcomes data by using population-weighted averages within the circuit-year). Compared to the main IV results for house prices, the LASSO IV results remain positive but are smaller in magnitude. The price estimates are robustly small and positive when collapsed to the circuit-year level. Generally speaking, the results with high-granularity data, such as zip-code level house prices and individual-level housing and employment outcomes, are more precise and robust, while low-granularity data, such as state-level GDP, are less precise and robust.

H.5 Standard Errors: Bootstrap Simulations and Clustering

Standard errors in the main specification are clustered by circuit and our results are robust the clustering at the state-level (Row C of Appendix Tables 2.1B, 2.2B, 3.1B, 3.2B, 2.1C, 2.2C, 3.1C, and 3.2C). We also employ wild bootstrap and Monte Carlo simulations that randomly assign the laws and appellate panel assignments to different circuits. For house price growth, we find that the point estimates within a year of the pro-government physical takings precedent and 3-4 years later each differ from the null of 0 at the 10% level (results not shown in tables); point estimates for the average interaction lags are at the 90th percentile in Monte Carlo simulations.

H.6 Different Ways of Measuring Outcomes and Measuring Law

Level vs. First Differences

Qualitatively, we find that regressions in levels and first-differences line up (Appendix Table 3.1C and 3.1D). Level regressions indicate a 4% increase in property price levels occurs in the 4th year, whereas the estimated cumulative effect of price growth is a 2.5% increase in price levels.

Circuit Quarter Laws

When we exploit variation at the circuit-quarter level, the effects are slightly larger in magnitude and quite a bit more jointly statistically significant (Appendix Table 2.1B and 3.1B Row I). Moreover, the individual coefficients are within standard error bounds of the original estimates (Appendix Table 2.1C and 3.1C Row H). The leads in the physical takings cases are jointly statistically significant, however, but these leads are not robust to using the date of the case with the publication date of the district court decision.[37]

Number vs. Percent of Pro-Takings Decisions

When we weight by the average number of cases in the current year and previous 4 years, the estimates become more statistically significant and precisely estimated across the specifications (results not displayed). Whether using weights to treat Law_{ct} as an average of M_{ct} number of decisions or using weights to treat Law_{ct} as appearing with M_{ct} frequency show dramatic improvements in precision for our estimates. To apply weights in the distributed lag specifications, we weigh each observation by the sum of lagged M_{ct}, or alternatively, the geometric mean of lagged $(1+M_{ct})$.

H.7 Local vs. Precedential Effects of Takings

The impact of physical takings is robust to controlling for the local direct effects; the local effects are imprecisely measured. In unreported results, the lead coefficients of the precedential effects are negligible while the lead coefficients of the local direct effects are sizeable and larger in magnitude than the lag coefficients of the local direct effects, which is consistent with the

[37]Some physical takings cases take very long to resolve. The media frequently discusses the cases before the actual decision is published, and the time between oral argument, which is public, and publication can be many years in the extreme. Since the oral argument date is not reliably observable for most cases, we substitute the publication date of the district court decision as the date of appellate decision to verify that economic outcomes do not move in advance of appellate decisions.

local taking having occurred quite a few years before the appellate decision. Local effects are sometimes negative, which can be due to both the effect of the local takings as well as the possibility that landowners may hear about a local taking and reduce their subjective belief in the probability of government action.

Table 1 - Impact of Physical Takings Precedent on Condemnations

Dependent Variable	**Log Federal Compensation**		**Log Non-Residential Displacements**		**Log Non-Residential Relocation Costs**	
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Pro-Taking	-0.188	-0.480*	-0.274+	0.0188	-0.0546	0.291
Appellate Decisions$_{t+1}$	(0.138)	(0.245)	(0.126)	(0.165)	(0.211)	(0.527)
Proportion Pro-Taking	-0.114	-0.328+	-0.0796	-0.208	0.113	-0.343
Appellate Decisions$_{t}$	(0.137)	(0.194)	(0.146)	(0.300)	(0.181)	(0.551)
Proportion Pro-Taking	-0.544*	-0.518	-0.00196	0.00893	0.171	0.479+
Appellate Decisions$_{t-1}$	(0.177)	(0.443)	(0.140)	(0.176)	(0.190)	(0.284)
Proportion Pro-Taking	0.0390	0.209	0.151	0.161	0.303+	0.436
Appellate Decisions$_{t-2}$	(0.172)	(0.194)	(0.0980)	(0.157)	(0.140)	(0.286)
Proportion Pro-Taking	-0.455**	-0.885*	0.204	0.316+	-0.304	-0.554+
Appellate Decisions$_{t-3}$	(0.119)	(0.364)	(0.115)	(0.173)	(0.281)	(0.305)
Proportion Pro-Taking	-0.424+	-0.631**	-0.118	-0.151	0.214	0.674
Appellate Decisions$_{t-4}$	(0.194)	(0.216)	(0.0850)	(0.189)	(0.157)	(0.482)
Appellate IV	N	Y	N	Y	N	Y
District IV	N	N	N	N	N	N
Aggregation Level	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year
N	612	612	663	663	663	663
R-sq	0.616	0.614	0.310	0.308	0.395	0.391
Mean dependent variable	1986.309	1986.309	55.722	55.722	80.123	80.123
Mean log dependent variable	15.503	15.503	3.139	3.139	12.117	12.117
Average lag effect	-0.300	-0.431	0.031	0.025	0.099	0.138
Joint P-value of lags	0.013	0.000	0.027	0.000	0.088	0.012
P-value of leads	0.202	0.050	0.053	0.909	0.800	0.581

Notes: Significant at +10%, *5%, **1%. Data come from FHWA. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. All values are in logs of the underlying value plus one. Means of the underlying values are displayed as mean dependent variable. All data is from 1991-2003 except compensation, which is from 1995-2003.

Federal Compensation: Total of the amounts paid, deposited in court, or otherwise made available to a property owner from federal funds pursuant to applicable law. This includes all parcels acquired during the report year where title or possession was vested in the Agency during the reporting period, whether through purchase in the open market, condemnation, or administrative settlement. Includes expenses incidental to transfer of title. Excludes appraisal costs, negotiator fees and other administrative expenses.

Number of Non-Residential Displacements: Number of businesses, nonprofit organizations, and farms who were permanently displaced during the fiscal year by project or program activities and moved to their replacement location. This includes businesses, nonprofit organizations, and farms, that upon displacement, discontinued operations.

Non-Residential Relocation Costs: Total amount paid for nonresidential moving expenses (actual expense and fixed payment) and for reestablishment expenses. Excludes agency administrative costs.

Table 2 - Impact of Physical Takings Precedent on House Prices and GDP

Dependent Variable	ΔLog Price Index		ΔLog GDP	
	(1)	(2)	(3)	(4)
Proportion Pro-Taking	0.00402	0.00285	0.000911	0.00233
Appellate Decisions$_{t+1}$	(0.00230)	(0.00428)	(0.00641)	(0.00969)
Proportion Pro-Taking	0.00499*	0.00955+	0.00410	0.00472
Appellate Decisions$_{t}$	(0.00193)	(0.00557)	(0.00411)	(0.00931)
Proportion Pro-Taking	0.00296*	0.0136**	0.00287	0.0192*
Appellate Decisions$_{t-1}$	(0.00133)	(0.00396)	(0.00299)	(0.00849)
Proportion Pro-Taking	0.00330*	0.0190**	0.00297	0.00994**
Appellate Decisions$_{t-2}$	(0.00133)	(0.00326)	(0.00377)	(0.00378)
Proportion Pro-Taking	0.00159	0.0124**	0.000282	0.0138*
Appellate Decisions$_{t-3}$	(0.00166)	(0.00410)	(0.00337)	(0.00626)
Proportion Pro-Taking	-0.000393	0.00552**	-0.00288	0.00528
Appellate Decisions$_{t-4}$	(0.00129)	(0.00165)	(0.00342)	(0.00956)
Appellate IV	N	Y	N	Y
District IV	N	N	N	N
Aggregation Level	Zip-Year	Zip-Year	State-Year	State-Year
N	3989626	3989626	1671	1671
R-sq	0.112	0.080	0.426	0.410
Mean dependent variable	0.012	0.012	0.066	0.066
Average lag effect	0.002	0.012	0.001	0.011
Joint P-value of lags	0.032	0.000	0.254	0.000
P-value of leads	0.108	0.505	0.890	0.810
Average lag of no appeal	0.003	0.010	0.002	0.009
Joint P-value of no appeal lags	0.094	0.000	0.040	0.000
P-value of unconditional ($Law_{ct} + 1[M_{ct} > 0]$) lags	0.000	0.000	0.040	0.025
Typical				
Conditional effect	0.0004	0.0021	0.0002	0.0020
Unconditional effect - pro	-0.0001	0.0005	-0.0001	0.0005
Unconditional effect - anti	-0.0002	-0.0008	-0.0002	-0.0007
Unconditional effect - all	-0.0005	-0.0006	-0.0004	-0.0005

Notes: Significant at +10%, *5%, **1%. 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, a dummy for whether there were no cases in that circuit-year. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic Non-White Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year.

Table 3 - Impact of Physical Takings Precedent on House Prices -- Robustness Across IV Specifications and Aggregation Level

Dependent Variable	**ΔLog Price Index**			
	(1)	(2)	(3)	(4)
Proportion Pro-Taking	0.00647	0.000831	0.00616	0.00379
Appellate Decisions$_{t+1}$	(0.00492)	(0.00437)	(0.00387)	(0.00482)
Proportion Pro-Taking	0.00860	0.0106+	0.0140**	0.0100*
Appellate Decisions$_{t}$	(0.00583)	(0.00549)	(0.00447)	(0.00436)
Proportion Pro-Taking	0.0124*	0.0118**	0.0141**	0.00869*
Appellate Decisions$_{t-1}$	(0.00506)	(0.00399)	(0.00513)	(0.00428)
Proportion Pro-Taking	0.0211**	0.0105	0.00985**	0.00567
Appellate Decisions$_{t-2}$	(0.00427)	(0.00681)	(0.00363)	(0.00539)
Proportion Pro-Taking	0.0196**	0.00906	0.00367	0.00256
Appellate Decisions$_{t-3}$	(0.00617)	(0.00719)	(0.00444)	(0.00594)
Proportion Pro-Taking	0.00478	-0.00633	-0.001000	-0.00302
Appellate Decisions$_{t-4}$	(0.00420)	(0.00596)	(0.00280)	(0.00431)
Appellate IV	Y	Y	Lasso IV	Lasso IV
District IV	N	Lasso IV	N	Lasso IV
Aggregation Level	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
N	398	398	398	398
R-sq	0.429	0.525	0.538	0.566
Mean dependent variable	0.013	0.013	0.013	0.013
Average lag effect	0.013	0.007	0.008	0.005
Joint P-value of lags	0.000	0.000	0.002	0.000
P-value of leads	0.189	0.849	0.112	0.432
Average lag of no appeal	0.010	0.005	0.006	0.003
Joint P-value of no appeal lags	0.000	0.000	0.208	0.532
P-value of unconditional				
(Law_{ct} + $1[M_{ct} > 0]$) lags	0.000	0.000	0.000	0.029
Typical				
Conditional effect	0.0023	0.0012	0.0014	0.0009
Unconditional effect - pro	0.0007	0.0004	0.0005	0.0004
Unconditional effect - anti	-0.0008	-0.0004	-0.0005	-0.0002
Unconditional effect - all	-0.0004	-0.0001	-0.0002	0.0001

Notes: Significant at +10%, *5%, **1%. 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, a dummy for whether there were no cases in that circuit-year. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic Non-White Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year.

Table 4 -- Impact of Physical Takings Precedent on House Prices -- Robustness of IV Estimates Across Controls

	The Effect of Appellate Physical Takings Precedent on ΔLog Price Index		
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
A. Add Circuit-Specific Trends	0.012	0.000	0.643
B. No Fixed Effects	0.006	0.002	0.209
C. State Cluster	0.012	0.000	0.408
D. Control for Expectation	0.017	0.000	0.350
E. Use Population Weights	0.015	0.000	0.521
F. Add 2-year Lead	0.012	0.000	0.557
G. Drop 1 Circuit			
Circuit 1	0.012	0.000	0.693
Circuit 2	0.010	0.000	0.456
Circuit 3	0.013	0.000	0.491
Circuit 4	0.012	0.000	0.578
Circuit 5	0.013	0.000	0.300
Circuit 6	0.011	0.000	0.571
Circuit 7	0.014	0.000	0.568
Circuit 8	0.012	0.000	0.342
Circuit 9	0.010	0.000	0.217
Circuit 10	0.012	0.000	0.347
Circuit 11	0.013	0.000	0.326
Circuit 12	0.012	0.000	0.510
H. Circuit-quarter laws	0.010	0.000	0.004

Notes: Significant at +10%, *5%, **1%. 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 5, Panel B. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior U.S. Attorneys per seat assigned to physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Table 5 -- Impact of Physical Takings Precedent on House Prices
Robustness of IV Distributed Lag Estimates Across Controls, Lag Structure, Leads, and Local Effects

	The Effect of Appellate Physical Takings Precedent on ΔLog Price Index					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. Add Circuit-Specific Trends	0.010+	0.013**	0.019**	0.014**	0.006**	
	(0.006)	(0.004)	(0.004)	(0.005)	(0.002)	
B. No Fixed Effects	-0.000	-0.003	0.015+	0.018+	0.001	
	(0.007)	(0.004)	(0.009)	(0.010)	(0.006)	
C. State Cluster	0.010+	0.014**	0.019**	0.012**	0.006*	
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	
D. Control for Expectation	0.016+	0.021**	0.023**	0.015**	0.010**	
	(0.009)	(0.006)	(0.003)	(0.004)	(0.003)	
E. Use Population Weights	0.014+	0.019**	0.023**	0.014**	0.005**	
	(0.007)	(0.006)	(0.005)	(0.004)	(0.002)	
F. Drop 1 Circuit						
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)	
G. Lag Structure						
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)
4 Leads, 1 Lag	0.004	0.005+	0.001	-0.004	-0.005	-0.004
(t0, t1, f4, f3, f2, f1)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
	(q0)	(q4)	(q8)	(q12)	(q16)	Mean
H. 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)	
I. 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)	

Notes: Significant at +10%, *5%, **1%. 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 5, Panel B. Coefficients on the lags are shown here. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior U.S. Attorneys per seat assigned to physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Table 6 - Impact of Physical Takings Precedent on Inequality

Dependent Variable	Dummy Indicator for Home Ownership		Dummy Indicator for Employed	
	(1)	(2)	(5)	(6)
Proportion Pro-Taking	0.00131	-0.00879*	0.00438	0.00538
Appellate Decisions$_{t+1}$	(0.00338)	(0.00377)	(0.00268)	(0.00583)
Proportion Pro-Taking	0.0106**	0.0128+	0.00476*	0.0111+
Appellate Decisions$_{t}$	(0.00310)	(0.00773)	(0.00165)	(0.00652)
Proportion Pro-Taking	0.0131**	0.0121	0.00433*	0.00369
Appellate Decisions$_{t-1}$	(0.00350)	(0.00957)	(0.00180)	(0.00434)
Proportion Pro-Taking	0.00435	-0.00353	0.00577*	0.00872*
Appellate Decisions$_{t-2}$	(0.00400)	(0.0101)	(0.00202)	(0.00395)
Proportion Pro-Taking	0.000375	-0.00598	0.00545*	0.0170+
Appellate Decisions$_{t-3}$	(0.00353)	(0.0137)	(0.00245)	(0.00951)
Proportion Pro-Taking	0.00397	0.00223	0.00440	0.0104
Appellate Decisions$_{t-4}$	(0.00354)	(0.00950)	(0.00338)	(0.00868)
Proportion Pro-Takings	-0.0131	-0.0186	-0.0187*	-0.0299+
Appellate Decisions$_{t+1}$ * Non-White	(0.0119)	(0.0204)	(0.00610)	(0.0163)
Proportion Pro-Takings	-0.0386**	-0.0586**	-0.0218**	-0.0406*
Appellate Decisions$_{t}$ * Non-White	(0.0112)	(0.0107)	(0.00605)	(0.0192)
Proportion Pro-Takings	-0.0289+	-0.00407	-0.0113	-0.0114
Appellate Decisions$_{t-1}$ * Non-White	(0.0132)	(0.0270)	(0.00660)	(0.00907)
Proportion Pro-Takings	-0.0210	0.0113	-0.0134*	-0.00274
Appellate Decisions$_{t-2}$ * Non-White	(0.0143)	(0.0255)	(0.00568)	(0.00776)
Proportion Pro-Takings	-0.0328+	-0.0443	-0.0184*	-0.0262
Appellate Decisions$_{t-3}$ * Non-White	(0.0168)	(0.0591)	(0.00717)	(0.0230)
Proportion Pro-Takings	-0.0314+	-0.0292	-0.0102	-0.0230
Appellate Decisions$_{t-4}$ * Non-White	(0.0162)	(0.0436)	(0.00632)	(0.0202)
Appellate IV	N	Y	N	Y
District IV	N	N	N	N
Aggregation Level	Individual	Individual	Individual	Individual
N	4098609	4098609	6720948	6720948
R-sq	0.062	0.062	0.095	0.095
Mean dependent variable (Non-White)	0.522	0.522	0.655	0.655
Mean dependent variable (White)	0.721	0.721	0.742	0.742
Average interaction lag	-0.031	-0.025	-0.015	-0.021
Average level effect lag	0.004	0.004	0.005	0.012
P value of chi-sq of interaction lags	0.063	0.000	0.016	0.011
P value of chi-sq of level effect lags	0.020	0.111	0.158	0.000
Average no appeal interaction lag	-0.038	-0.038	-0.012	-0.017
Average no appeal level effect lag	0.007	0.005	0.004	0.008
P value of no appeal interaction lags	0.000	0.000	0.009	0.002
P value no appeal level effect lags	0.014	0.229	0.489	0.000
Typical				
Conditional interaction effect	-0.0055	-0.0045	-0.0027	-0.0037
Conditional level effect	0.0007	0.0007	0.0009	0.0021
Unconditional interaction effect - pro	0.0006	0.0016	-0.0008	-0.0010
Unconditional interaction effect - anti	0.0030	0.0030	0.0010	0.0014
Unconditional interaction effect - all	0.0047	0.0058	0.0006	0.0008
Unconditional level effect - pro	-0.0004	-0.0001	0.0003	0.0009
Unconditional level effect - anti	-0.0006	-0.0004	-0.0003	-0.0006
Unconditional level effect - all	-0.0012	-0.0006	-0.0002	0.0000

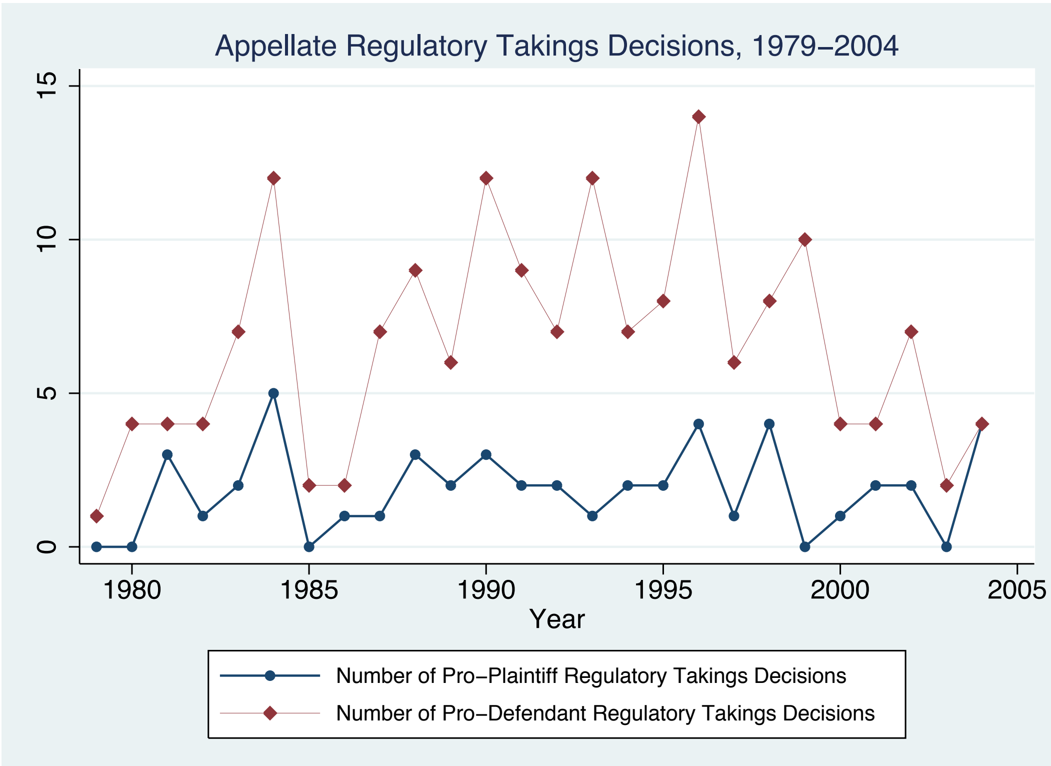
Notes: Regressions of housing outcomes use March CPS and regressions of employment outcomes use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. Regressions include individual controls (age, race dummies, educational attainment dummies, 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. Instruments for regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for physical takings are Democratic Nonwhite Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO Instruments are displayed in Appendix Table A3. Significant at +10%, *5%, **1%

Table 7 - Impact of Regulatory Takings Precedent on House Prices and GDP

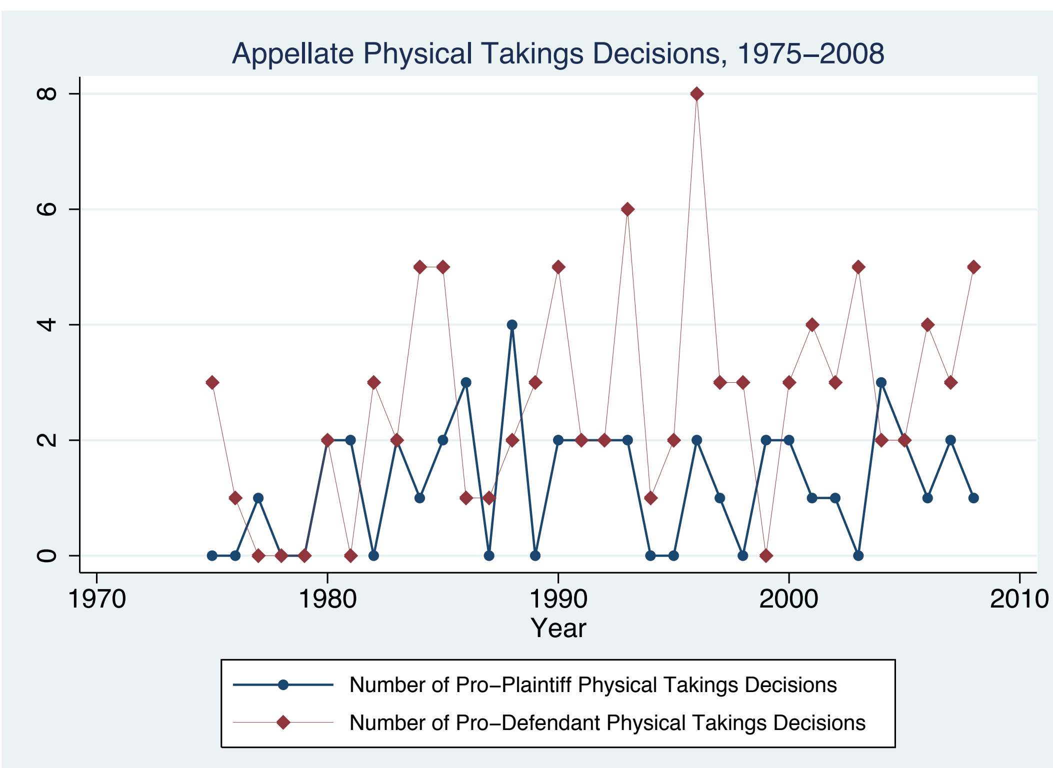
Dependent Variable	ΔLog Price Index		ΔLog GDP	
	(1)	(2)	(3)	(4)
Proportion Pro-Taking	-0.00349**	-0.00192	-0.000509	-0.00644
Appellate Decisions$_{t+1}$	(0.000985)	(0.00932)	(0.00386)	(0.0153)
Proportion Pro-Taking	0.00156	-0.0108	-0.000851	-0.00985
Appellate Decisions$_{t}$	(0.00232)	(0.0116)	(0.00486)	(0.0218)
Proportion Pro-Taking	0.00201	0.00419	0.00341	0.00200
Appellate Decisions$_{t-1}$	(0.00135)	(0.0133)	(0.00211)	(0.0137)
Proportion Pro-Taking	0.000963	0.0111	0.00833	0.0398*
Appellate Decisions$_{t-2}$	(0.00102)	(0.00966)	(0.00560)	(0.0197)
Proportion Pro-Taking	0.00273	0.0166	0.0105*	0.00587
Appellate Decisions$_{t-3}$	(0.00158)	(0.0159)	(0.00344)	(0.0208)
Proportion Pro-Taking	0.00257+	0.00474	0.00319	0.0421
Appellate Decisions$_{t-4}$	(0.00121)	(0.00867)	(0.00555)	(0.0257)
Appellate IV	N	Y	N	Y
District IV	N	N	N	N
Aggregation Level	Zip-Year	Zip-Year	State-Year	State-Year
N	2486744	2486744	1065	1065
R-sq	0.082	.	0.243	0.124
Mean dependent variable	0.011	0.011	0.056	0.056
Average lag effect	0.002	0.005	0.005	0.016
Joint P-value of lags	0.086	0.001	0.024	0.066
P-value of leads	0.005	0.837	0.897	0.673
Average lag of no appeal	0.003	0.005	0.007	0.015
Joint P-value of no appeal lags	0.208	0.029	0.004	0.061
P-value of unconditional				
($Law_{ct} + 1[M_{ct} > 0]$) lags	0.532	0.000	0.124	0.434
Typical				
Conditional effect	0.0008	0.0021	0.0021	0.0067
Unconditional effect - pro	-0.0003	0.0002	-0.0006	0.0009
Unconditional effect - anti	-0.0004	-0.0007	-0.0010	-0.0021
Unconditional effect - all	-0.0008	-0.0006	-0.0017	-0.0014

Notes: Significant at +10%, *5%, **1%. 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, a dummy for whether there were no cases in that circuit-year. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic Non-White Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year.

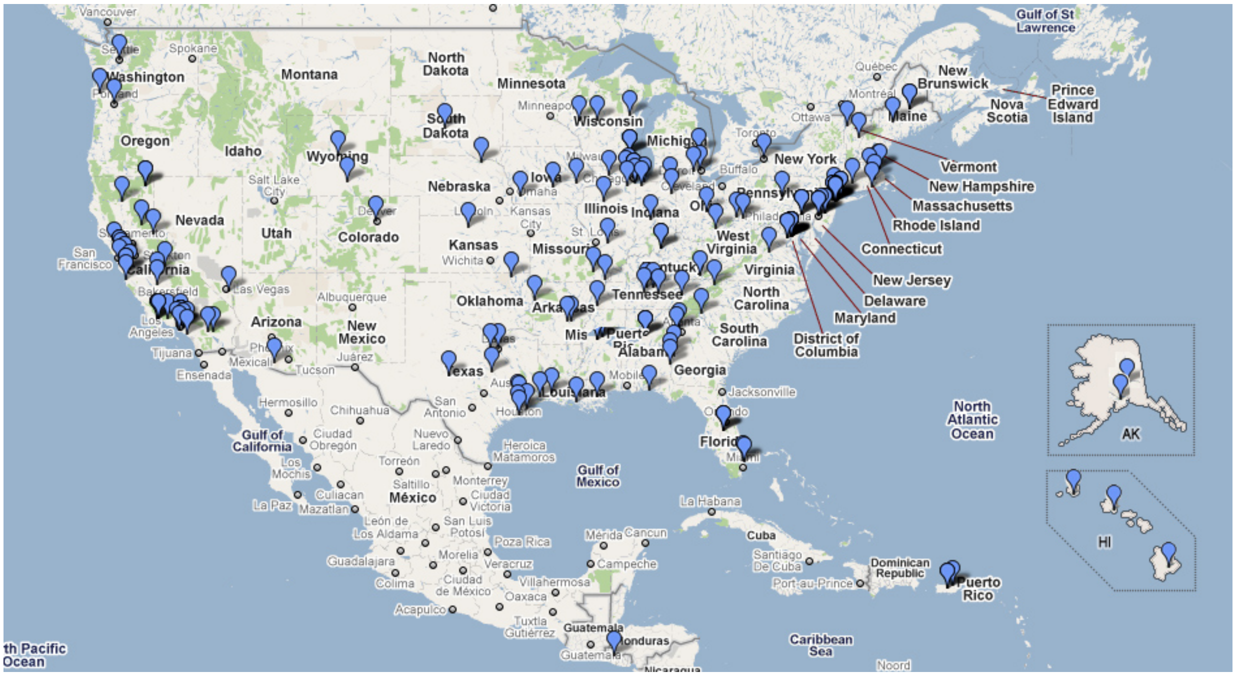
Appendix Figure 1A



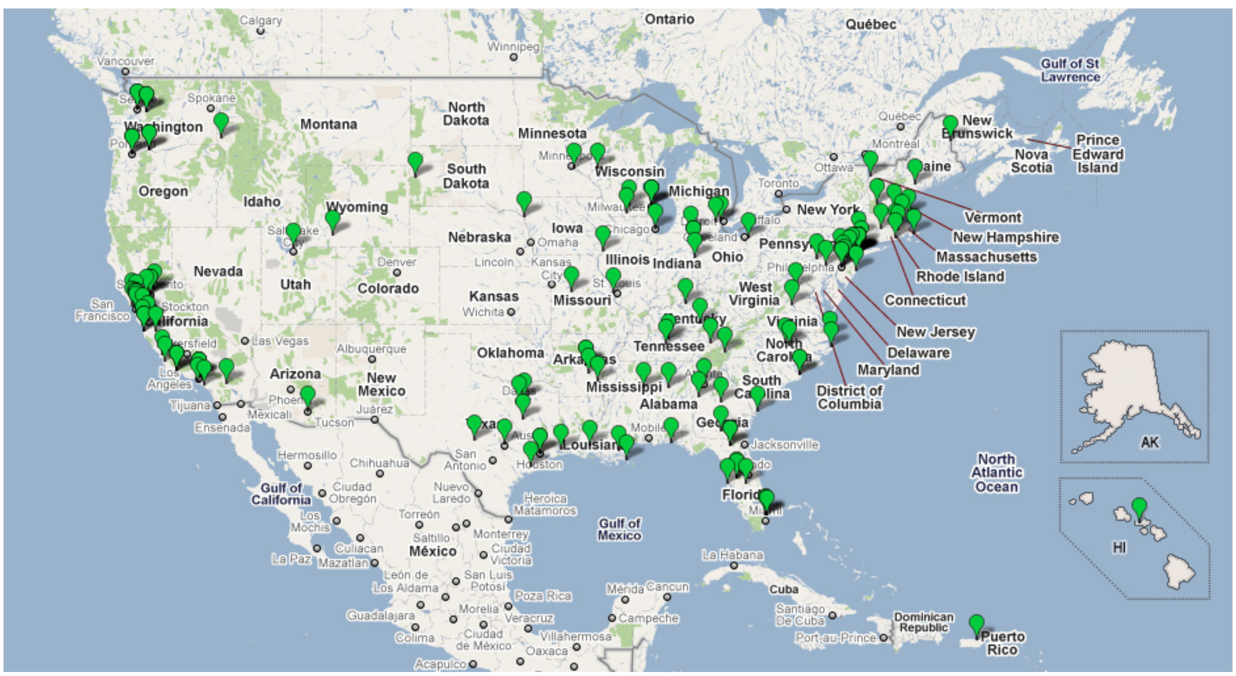
Appendix Figure 1B



Appendix Figure 2 -- Map of Local Takings

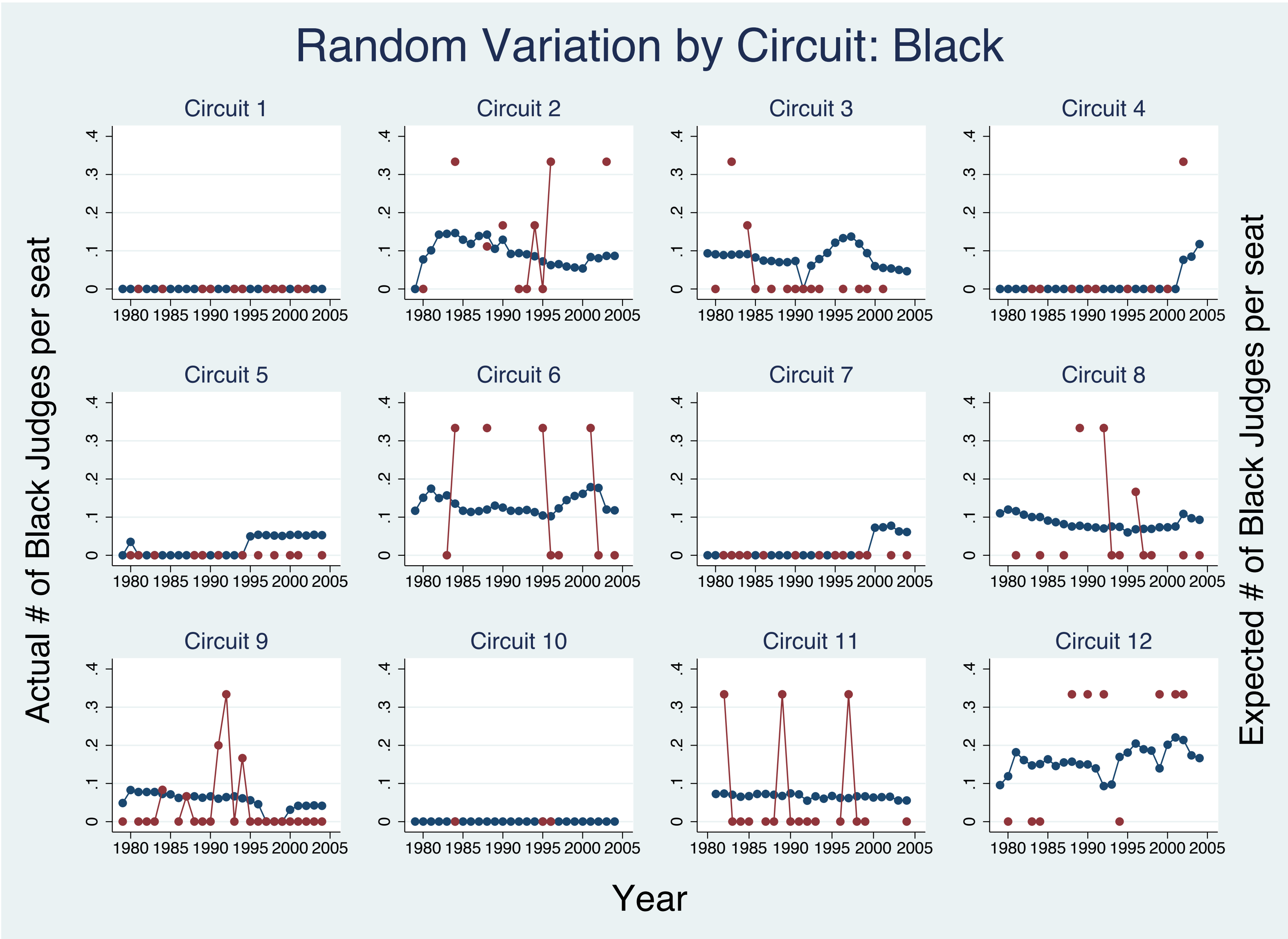


Regulatory Takings

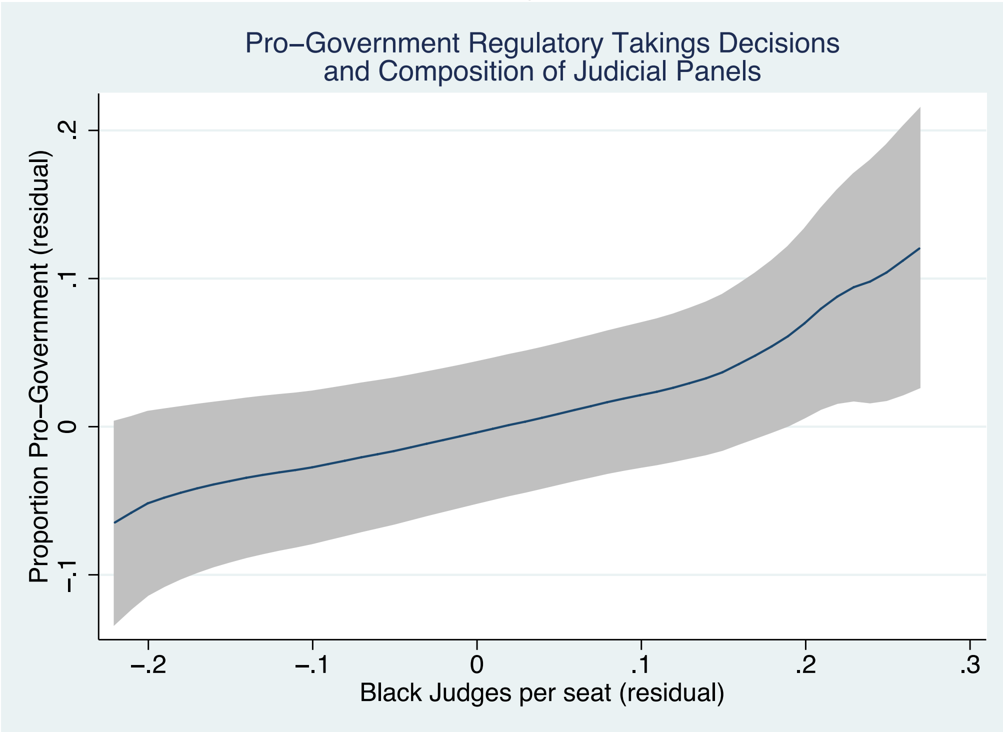


Physical Takings

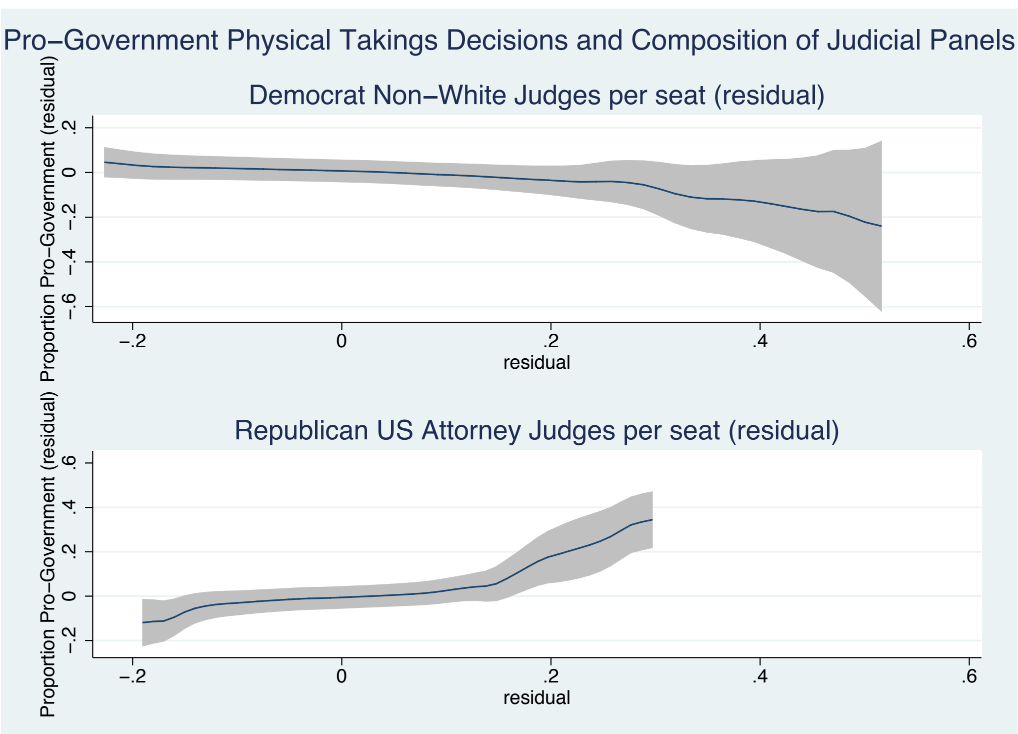
Appendix Figure 3



Appendix Figure 4A



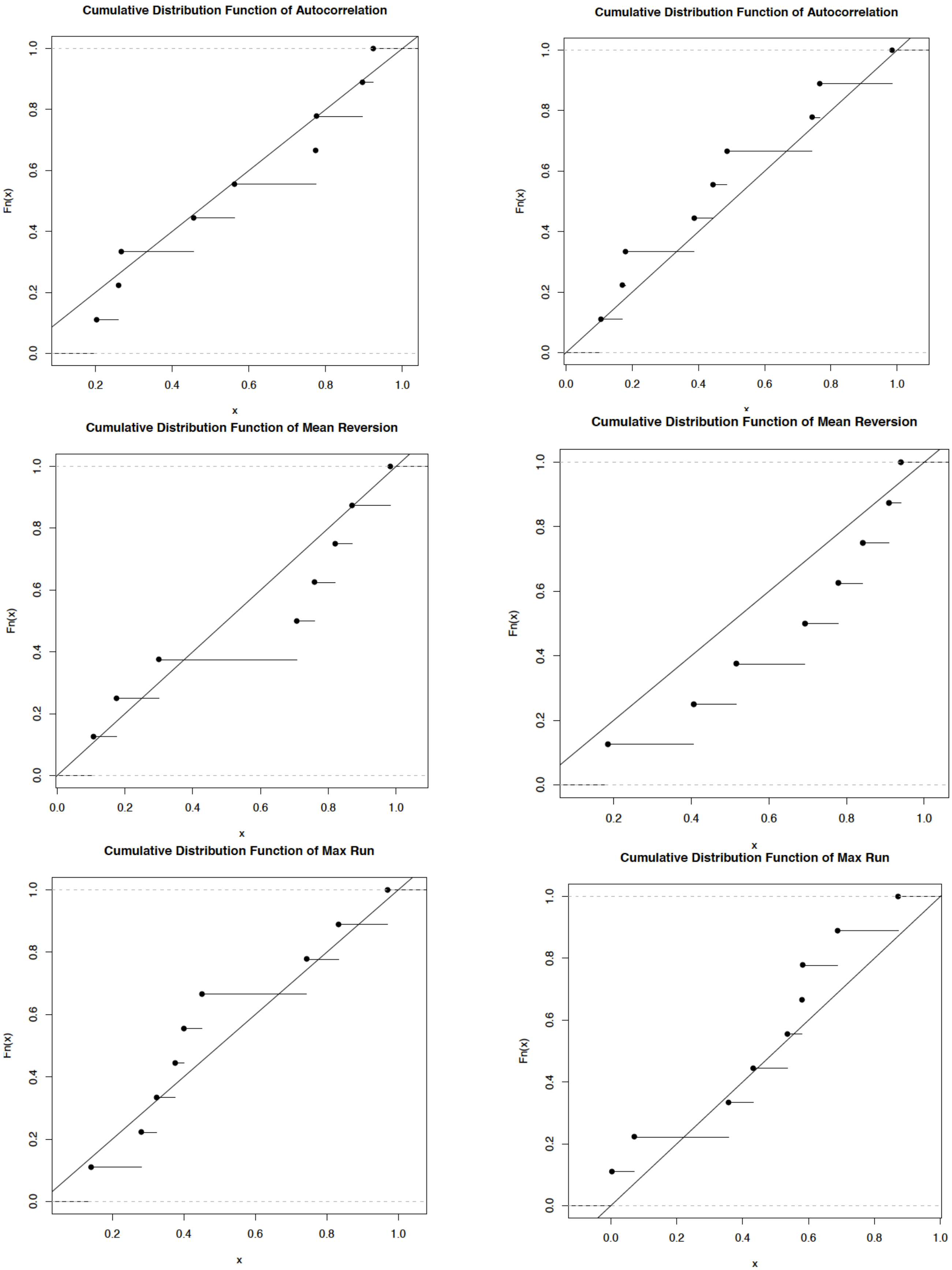
Appendix Figure 4B



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.

Appendix Figure 5: Randomization Check

P-Values of Black judge strings | P-Values of Minority Democrat strings



Appendix Table 1.1: List of Physical Takings Appellate Precedent

Citation	Case Name	Circuit	Year	Pro-plaintiff
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
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

Citation	Case Name	Circuit	Year	Pro-plaintiff
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
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

Appendix Table 1.2: List of Regulatory Takings Appellate Precedent

Citation	Case Name	Circuit	Year	Pro-plaintiff
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 1.3 - Summary Statistics

Circuit-Year Level	Mean [Standard Deviation]
Physical Takings Cases (1975-2008)	
Number of Judges	17.662 [7.719]
Number of Physical Takings Panels	0.333 [0.630]
Proportion of Circuit-Years with No Physical Takings Panels	73%
Proportion of Pro-Government Physical Takings Decisions when Circuit-Year has Panels	66%
Expected # of Minority Judges per Seat when Circuit-Year has Panels	0.086 [0.066]
Expected # of Democratic Appointees per Seat when Circuit-Year has Panels	0.418 [0.129]
Expected # of Prior US Attorneys per Seat when Circuit-Year has Panels	0.071 [0.081]
Expected # of Democratic Minority Appointees per when Circuit-Year has PanelsSeat	0.064 [0.059]
Expected # Republican Prior US Attorneys per Seat when Circuit-Year has Panels	0.038 [0.062]
N (circuit-years)	402
Regulatory Takings Cases (1979-2004)	
Number of Judges	17.813 [7.457]
Number of Regulatory Takings Panels	0.71 [0.988]
Proportion of Circuit-Years with No Regulatory Takings Panels	54%
Proportion of Pro-Government Regulatory Takings Decisions when Circuit-Year has Panels	78%
Expected # of Black Judges per Seat when Circuit-Year has Panels	0.06 [0.056]
N (circuit-years)	310

Appendix Table 1.4 - First Stage: Relationship Between Pro-Government Physical Takings Appellate Decisions and Composition of Physical Takings Panels, 1975-2008

Panel A: Judge Level	Outcome: Pro-Takings Vote					
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic, Minority	-0.203*		-0.258*	-0.154+	-0.198	-0.112+
	(0.0686)		(0.113)	(0.0755)	(0.125)	(0.0552)
Republican, Prior US Attorney		0.176*	0.153+	0.0859	0.134+	0.0647
		(0.0741)	(0.0748)	(0.0827)	(0.0654)	(0.0902)
Circuit-year controls	N	N	N	Fixed Effects	Expectations	Both
F-statistic	8.800	5.638	4.367	5.010	4.092	4.260
N	394	307	307	307	307	307
R-sq	0.017	0.008	0.031	0.359	0.054	0.398

Panel B: Case Level	Outcome: Pro-Takings Decision			
	(1)	(2)	(3)	(4)
Democratic, Minority Appointees per Seat	-0.570*	-0.573**	-0.551*	-0.426*
	(0.186)	(0.182)	(0.249)	(0.188)
Republican, Prior US Attorneys per Seat	0.677*	0.610	0.598+	0.502
	(0.235)	(0.515)	(0.272)	(0.545)
Circuit-year controls	N	Fixed Effects	Expectations	Both
F-statistic of instruments	12.540	14.429	9.978	4.239
N	134	134	134	134
R-sq	0.076	0.388	0.079	0.410

Panel C: Circuit-Year Level	Outcome: % Pro-Takings Decisions				
	(1)	(2)	(3)	(4)	(5)
Democratic, Minority Appointees per Seat	-0.615**	-0.615**	-0.655**	-0.666**	-0.651**
	(0.193)	(0.191)	(0.172)	(0.177)	(0.160)
Republican, Prior US Attorneys per Seat	0.929**	0.929**	0.969**	0.963**	1.032**
	(0.272)	(0.270)	(0.233)	(0.231)	(0.212)
Circuit-years with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	9.010	9.114	15.178	15.220	19.239
N	107	402	402	402	402
R-sq	0.108	0.651	0.692	0.693	0.705

Panel D: Circuit-Quarter Level	Outcome: % Pro-Takings Decisions				
	(1)	(2)	(3)	(4)	(5)
Democratic, Minority Appointees per Seat	-0.547*	-0.547*	-0.561**	-0.563**	-0.561**
	(0.184)	(0.182)	(0.175)	(0.176)	(0.171)
Republican, Prior US Attorneys per Seat	0.707*	0.707*	0.703*	0.702*	0.717**
	(0.232)	(0.230)	(0.232)	(0.232)	(0.229)
Circuit-quarters with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-quarter controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	12.357	12.530	12.991	13.039	13.349
N	129	1608	1608	1608	1608
R-sq	0.075	0.680	0.690	0.690	0.693

Panel E: Circuit-Quarter Level	Outcome: % Pro-Takings Decisions				
(Merged with Zip Code Price Data)	(8)	(9)	(10)	(11)	(12)
Democratic, Minority Appointees per Seat	-0.518*	-0.518*	-0.534*	-0.534*	-0.533**
	(0.184)	(0.184)	(0.175)	(0.174)	(0.168)
Republican, Prior US Attorneys per Seat	0.553*	0.553*	0.542*	0.540*	0.555*
	(0.215)	(0.215)	(0.215)	(0.216)	(0.211)
Circuit-quarters with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-quarter controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	34.975	34.976	40.946	42.747	41.925
N	357691	4054704	4054704	4054704	4054704
R-sq	0.062	0.676	0.686	0.686	0.689

Notes: Heteroskedasticity-robust standard errors are in parentheses and clustered at the circuit level. Fixed effects are dummy indicators for circuit, year, and quarter. Expectations are the expected proportions that are minority Democratic appointees or prior US attorney Republican appointees on a given panel. Trends are circuit-specific time trends. Proportions during circuit-years with no cases are defind to be 0. Panel D uses variation in judicial decisions at the circuit-quarter level. Panel E sample includes zip-code level prices 1975q1-2008q4. Significant at +10%; *5%; **1%.

Appendix Table 1.5 - First Stage: Relationship Between Pro-Government Regulatory Takings Appellate Decisions and Composition of Regulatory Takings Panels, 1979-2004

Panel A: Judge Level	Outcome: Pro-Takings Vote			
	(1)	(2)	(3)	(4)
Judge is Black	0.108*	0.153**	0.151**	0.158**
	(0.0378)	(0.0366)	(0.0338)	(0.0383)
Circuit-year controls	N	Fixed Effects	Expectations	Both
F-statistic	8.162	17.599	19.947	17.101
N	651	651	651	651
R-sq	0.004	0.123	0.014	0.125

Panel B: Case Level	Outcome: Pro-Takings Decision			
	(1)	(2)	(3)	(4)
Black Judges per Seat	0.326*	0.550**	0.508**	0.563**
	(0.119)	(0.158)	(0.124)	(0.161)
Circuit-year controls	N	Fixed Effects	Expectations	Both
F-statistic of instruments	7.572	12.076	16.772	12.167
N	220	220	220	220
R-sq	0.011	0.151	0.029	0.152

Panel C: Circuit-Year Level	Outcome: % Pro-Takings Decisions				
	(1)	(2)	(3)	(4)	(5)
Black Judges per Seat	0.395*	0.395*	0.519**	0.527**	0.495**
	(0.150)	(0.150)	(0.101)	(0.105)	(0.113)
Circuit-years with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-year controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	6.913	6.917	26.594	25.020	19.202
N	143	310	310	310	310
R-sq	0.016	0.708	0.735	0.736	0.747

Panel D: Circuit-Quarter Level	Outcome: % Pro-Takings Decisions				
	(1)	(2)	(3)	(4)	(5)
Black Judges per Seat	0.400**	0.400**	0.414**	0.413**	0.414**
	(0.109)	(0.109)	(0.0993)	(0.101)	(0.102)
Circuit-quarters with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-quarter controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	13.419	13.469	17.338	16.871	16.609
N	187	1195	1195	1195	1195
R-sq	0.020	0.796	0.801	0.801	0.803

Panel E: Circuit-Quarter Level	Outcome: % Pro-Takings Decisions				
(Merged with Zip Code Price Data)	(1)	(2)	(3)	(4)	(5)
Black Judges per Seat	0.515**	0.515**	0.511**	0.514**	0.516**
	(0.129)	(0.129)	(0.123)	(0.124)	(0.125)
Circuit-quarters with no cases	Dropped	Dummied	Dummied	Dummied	Dummied
Circuit-quarter controls	N	N	Fixed Effects	FE, Expect	FE, Trends
F-statistic of instruments	15.856	15.856	17.198	17.288	17.078
N	501391	2981400	2981400	2981400	2981400
R-sq	0.026	0.785	0.792	0.792	0.795

Notes: Heteroskedasticity-robust standard errors are in parentheses and clustered at the circuit level. Fixed effects are dummy indicators for circuit, year, and quarter. Expectations are the expected proportions of black judges on a given panel. Trends are circuit-specific time trends. Proportions during circuit-years with no cases are defind to be 0. Panel D uses variation in judicial decisions at the circuit-quarter level. Panel E sample includes zip-code level prices 1979q4-2004q3. Significant at +10%; *5%; **1%.

Appendix Table 1.6 -- Falsification Test of Instrument: Relationship Between Pro-Government Regulatory Takings Decisions and Composition of Regulatory Takings Panels in Other Years, 1979-2004

Circuit-Year Level	Outcome: Proportion of Pro-Takings Decisions, *t*			
	(1)	(2)	(3)	(4)
Black Judges per Seat, *t*	0.382*	0.418**	0.362*	0.340*
	(0.160)	(0.129)	(0.139)	(0.140)
Black Judges per Seat, *t-1*	0.156	0.162		
	(0.107)	(0.104)		
Black Judges per Seat, *t-2*		-0.154		
		(0.234)		
Black Judges per Seat, *t+1*			-0.244	-0.240
			(0.191)	(0.189)
Black Judges per Seat, *t+2*				0.0836
				(0.107)
N	298	286	298	286
R-sq	0.178	0.185	0.174	0.174

Notes: Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. Proportions of pro-takings decisions and judicial type per seat during circuit-years with no cases are defind to be 0 and dummied out. + Significant at 10%; * Significant at 5%; ** Significant at 1%

Appendix Table 1.7: LASSO instruments

Data	Aggregation	Law	Appellate		District	
Fiserv	Zip-Year	Physical	perseatxprotestantllm_sjd_212		t0xba_publicroaring	t0x_evangel.*t0x_war
		Regulatory	perseatx_noreligion_12.*perseatx_aba_12		t0x_war.*t0x_war	t0xprotestantinstate_ba
	Circuit-Year	Physical	perseatxprotestantllm_sjd_212	perseatxrepublicanprior_usa_212	t0x_evangel.*t0x_war	t0xba_publicroaring
		Regulatory	perseatxdemprior_ausa_12		t0x_noreligion.*t0x_instate_ba	t0x_instate_ba.*t0x_instate_ba
GDP	State-Year	Physical	perseatxdemprior_usa_212		t0xba_publicroaring	t0xroaringrich
		Regulatory	perseatx_noreligion_12.*perseatx_aba_12		t0x_mainline.*t0x_roaring	
	Circuit-Year	Physical	perseatxabaunity_212	perseatxroaringprior_govt_212	t0x_evangel.*t0x_war	t0xba_publicroaring
		Regulatory	perseatxdemprior_ausa_12		t0x_noreligion.*t0x_instate_ba	t0x_instate_ba.*t0x_instate_ba
CPS	Individual-Year	Physical	perseatx_llm_sjd_212.*perseatx_elev_212		t0xblackprior_lawp	t0xba_publicroaring
		Regulatory	perseatx_noreligion_12.*perseatx_aba_12		t0x_catholic.*t0x_early	t0x_instate_ba.*t0x_instate_ba

Notes: The symbol ".*" indicates a panel level interaction, otherwise it's a judge level interaction. We apply LASSO to select the optimal instruments from the following judge characteristics, interacted for a total of 900 possible instruments: 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.

Appendix Table 1.8 - Randomization Check: P-values

	Regulatory Takings (Black judges)				
	distance	size	90%	95%	99%
Autocorrelation	0.21844444	9	0.3392	0.3874	0.4795
Mean Reversion	0.332	8	0.3583	0.4097	0.5068
Longest Run	0.21566667	9	0.3392	0.3874	0.4795
	Physical Takings (Minority Democrat Appointees)				
	distance	size	90%	95%	99%
Autocorrelation	0.18066667	9	0.3392	0.3874	0.4795
Mean Reversion	0.318	8	0.3583	0.4097	0.5068
Longest Run	0.20088889	9	0.3392	0.3874	0.4795

Appendix Table 2.1A - Impact of Physical Takings Precedent on House Prices

Dependent Variable	ΔLog Price Index								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proportion Pro-Taking	0.00402	0.00285	0.00166	-0.00212	-0.00258	0.00647	0.000831	0.00616	0.00379
Appellate Decisions_{t+1}	(0.00230)	(0.00428)	(0.00408)	(0.00703)	(0.0100)	(0.00492)	(0.00437)	(0.00387)	(0.00482)
Proportion Pro-Taking	0.00499*	0.00955+	0.0121**	0.0139*	-0.00000577	0.00860	0.0106+	0.0140**	0.0100*
Appellate Decisions_{t}	(0.00193)	(0.00557)	(0.00445)	(0.00647)	(0.00552)	(0.00583)	(0.00549)	(0.00447)	(0.00436)
Proportion Pro-Taking	0.00296*	0.0136**	0.0112**	0.00147	0.00353	0.0124*	0.0118**	0.0141**	0.00869*
Appellate Decisions_{t-1}	(0.00133)	(0.00396)	(0.00364)	(0.00679)	(0.00490)	(0.00506)	(0.00399)	(0.00513)	(0.00428)
Proportion Pro-Taking	0.00330*	0.0190**	0.00872	0.00478	0.00507	0.0211**	0.0105	0.00985**	0.00567
Appellate Decisions_{t-2}	(0.00133)	(0.00326)	(0.00566)	(0.00390)	(0.00804)	(0.00427)	(0.00681)	(0.00363)	(0.00539)
Proportion Pro-Taking	0.00159	0.0124**	0.00652	-0.00393	-0.000501	0.0196**	0.00906	0.00367	0.00256
Appellate Decisions_{t-3}	(0.00166)	(0.00410)	(0.00547)	(0.00749)	(0.00401)	(0.00617)	(0.00719)	(0.00444)	(0.00594)
Proportion Pro-Taking	-0.000393	0.00552**	-0.00342	0.00573	0.00291	0.00478	-0.00633	-0.001000	-0.00302
Appellate Decisions_{t-4}	(0.00129)	(0.00165)	(0.00443)	(0.0107)	(0.00903)	(0.00420)	(0.00596)	(0.00280)	(0.00431)
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Y	Y	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	N	Lasso IV	N	Lasso IV
Aggregation Level	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
N	3989626	3989626	3989626	3989626	3989626	398	398	398	398
R-sq	0.112	0.080	0.099	0.103	0.087	0.429	0.525	0.538	0.566
Mean dependent variable	0.012	0.012	0.012	0.012	0.012	0.013	0.013	0.013	0.013
Average lag effect	0.002	0.012	0.007	0.004	0.002	0.013	0.007	0.008	0.005
P-value of lags	0.032	0.000	0.001	0.101	0.883	0.000	0.000	0.002	0.000
P-value of leads	0.108	0.505	0.684	0.763	0.797	0.189	0.849	0.112	0.432
Average lag of no appeal	0.003	0.010	0.006	0.004	0.003	0.010	0.005	0.006	0.003
P-value of no appeal lags	0.094	0.000	0.153	0.286	0.002	0.000	0.000	0.208	0.532
P-value of unconditional (Law_{ct} + $1[M_{ct} > 0]$) lags	0.000	0.000	0.000	0.015	0.060	0.000	0.000	0.000	0.029
Typical									
Conditional effect	0.0004	0.0021	0.0012	0.0007	0.0004	0.0023	0.0012	0.0014	0.0009
Unconditional effect - pro	-0.0001	0.0005	0.0003	0.0001	-0.0001	0.0007	0.0004	0.0005	0.0004
Unconditional effect - anti	-0.0002	-0.0008	-0.0005	-0.0003	-0.0002	-0.0008	-0.0004	-0.0005	-0.0002
Unconditional effect - all	-0.0005	-0.0006	-0.0004	-0.0004	-0.0005	-0.0004	-0.0001	-0.0002	0.0001

Notes: Significant at +10%, *5%, **1%. 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, a dummy for whether there were no cases in that circuit-year. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic Non-White Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO instruments are displayed in Appendix Table A3.

Appendix Table 2.1B -- Impact of Physical Takings Precedent on House Prices -- Robustness of IV Estimates Across Controls

	The Effect of Appellate Physical Takings Precedent on ΔLog Price Index		
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
A. Add Circuit-Specific Trends	0.012	0.000	0.643
B. No Fixed Effects	0.006	0.002	0.209
C. State Cluster	0.012	0.000	0.408
D. Control for Expectation	0.017	0.000	0.350
E. Use Population Weights	0.015	0.000	0.521
F. Add 2-year Lead	0.012	0.000	0.557
G. Drop 1 Circuit			
Circuit 1	0.012	0.000	0.693
Circuit 2	0.010	0.000	0.456
Circuit 3	0.013	0.000	0.491
Circuit 4	0.012	0.000	0.578
Circuit 5	0.013	0.000	0.300
Circuit 6	0.011	0.000	0.571
Circuit 7	0.014	0.000	0.568
Circuit 8	0.012	0.000	0.342
Circuit 9	0.010	0.000	0.217
Circuit 10	0.012	0.000	0.347
Circuit 11	0.013	0.000	0.326
Circuit 12	0.012	0.000	0.510
H. Circuit-quarter laws	0.010	0.000	0.004

Notes: Significant at +10%, *5%, **1%. 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 5, Panel B. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior U.S. Attorneys per seat assigned to physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 2.1C -- Impact of Physical Takings Precedent on House Prices
Robustness of IV Distributed Lag Estimates Across Controls, Lag Structure, Leads, and Local Effects

	The Effect of Appellate Physical Takings Precedent on ΔLog Price Index					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. Add Circuit-Specific Trends	0.010+	0.013**	0.019**	0.014**	0.006**	
	(0.006)	(0.004)	(0.004)	(0.005)	(0.002)	
B. No Fixed Effects	-0.000	-0.003	0.015+	0.018+	0.001	
	(0.007)	(0.004)	(0.009)	(0.010)	(0.006)	
C. State Cluster	0.010+	0.014**	0.019**	0.012**	0.006*	
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)	
D. Control for Expectation	0.016+	0.021**	0.023**	0.015**	0.010**	
	(0.009)	(0.006)	(0.003)	(0.004)	(0.003)	
E. Use Population Weights	0.014+	0.019**	0.023**	0.014**	0.005**	
	(0.007)	(0.006)	(0.005)	(0.004)	(0.002)	
F. Drop 1 Circuit						
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)	
G. Lag Structure						
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)
4 Leads, 1 Lag	0.004	0.005+	0.001	-0.004	-0.005	-0.004
(t0, t1, f4, f3, f2, f1)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
	(q0)	(q4)	(q8)	(q12)	(q16)	Mean
H. 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)	
I. 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)	

Notes: Significant at +10%, *5%, **1%. 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 5, Panel B. Coefficients on the lags are shown here. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior U.S. Attorneys per seat assigned to physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 2.2A - Impact of Physical Takings Precedent on Economic Growth

Dependent Variable	ΔLog GDP								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proportion Pro-Taking	0.000911	0.00233	0.00506	0.0251	0.00760	0.00202	-0.00482	0.0897	-0.00486
Appellate Decisions$_{t+1}$	(0.00641)	(0.00969)	(0.00674)	(0.0187)	(0.0282)	(0.0115)	(0.00769)	(0.115)	(0.00631)
Proportion Pro-Taking	0.00410	0.00472	0.0114+	0.0241	0.0219	0.00274	0.00771	-0.0499	-0.000596
Appellate Decisions$_{t}$	(0.00411)	(0.00931)	(0.00648)	(0.0252)	(0.0247)	(0.0104)	(0.00593)	(0.0398)	(0.00987)
Proportion Pro-Taking	0.00287	0.0192*	0.0180*	0.0158	0.0134	0.0104	0.00429	0.0137	-0.00842
Appellate Decisions$_{t-1}$	(0.00299)	(0.00849)	(0.00905)	(0.0176)	(0.0149)	(0.00721)	(0.00748)	(0.0139)	(0.0143)
Proportion Pro-Taking	0.00297	0.00994**	0.00836	0.0110	-0.00451	0.0167	-0.000318	0.0735	0.00659
Appellate Decisions$_{t-2}$	(0.00377)	(0.00378)	(0.00659)	(0.0196)	(0.0377)	(0.0120)	(0.00740)	(0.0845)	(0.00900)
Proportion Pro-Taking	0.000282	0.0138*	0.0112	0.0157	0.00122	0.0194*	0.0128	-0.0468	0.0139
Appellate Decisions$_{t-3}$	(0.00337)	(0.00626)	(0.00775)	(0.0229)	(0.0365)	(0.00783)	(0.00813)	(0.0633)	(0.0107)
Proportion Pro-Taking	-0.00288	0.00528	0.00677	-0.0103	-0.0229	0.0114	0.00693	0.0443	-0.00736
Appellate Decisions$_{t-4}$	(0.00342)	(0.00956)	(0.00940)	(0.0207)	(0.0280)	(0.0106)	(0.00898)	(0.0476)	(0.0102)
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Y	Y	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	N	Lasso IV	N	Lasso IV
Aggregation Level	State-Year	State-Year	State-Year	State-Year	State-Year	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
N	1671	1671	1671	1671	1671	387	387	387	387
R-sq	0.426	0.410	0.410	0.386	0.350	0.627	0.653	.	0.638
Mean dependent variable	0.066	0.066	0.066	0.066	0.066	0.064	0.064	0.064	0.064
Average lag effect	0.001	0.011	0.011	0.011	0.002	0.012	0.006	0.007	0.001
P-value of lags	0.254	0.000	0.009	0.012	0.205	0.001	0.484	0.136	0.824
P-value of leads	0.890	0.810	0.453	0.181	0.788	0.860	0.531	0.434	0.441
Average lag of no appeal	0.002	0.009	0.010	0.011	0.011	0.009	0.004	0.007	0.003
P-value of no appeal lags	0.040	0.000	0.000	0.002	0.400	0.019	0.035	0.202	0.086
P-value of unconditional (Law_{ct} + 1[M_{ct} > 0]) lags	0.040	0.025	0.048	0.029	0.651	0.085	0.427	0.696	0.693
Typical									
Conditional effect	0.0002	0.0020	0.0020	0.0020	0.0004	0.0021	0.0011	0.0012	0.0002
Unconditional effect - pro	-0.0001	0.0005	0.0004	0.0002	-0.0014	0.0007	0.0004	0.0001	-0.0003
Unconditional effect - anti	-0.0002	-0.0007	-0.0008	-0.0009	-0.0009	-0.0007	-0.0003	-0.0006	-0.0002
Unconditional effect - all	-0.0004	-0.0005	-0.0007	-0.0010	-0.0026	-0.0003	0.0000	-0.0006	-0.0006

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. 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. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO instruments are displayed in Appendix Table A3.

Appendix Table 2.2B -- Impact of Physical Takings Precedent on Growth -- Robustness of IV Estimates Across Controls

	The Effect of Appellate Physical Takings Precedent on ΔLog GDP		
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
A. Add Circuit-Specific Trends	0.008	0.011	0.894
B. No Fixed Effects	0.001	0.031	0.903
C. State Cluster	0.011	0.002	0.763
D. Control for Expectation	0.013	0.002	0.556
E. Use Population Weights	0.009	0.000	0.683
G. Drop 1 Circuit			
Circuit 1	0.010	0.001	0.767
Circuit 2	0.008	0.000	0.465
Circuit 3	0.012	0.000	0.601
Circuit 4	0.010	0.000	0.932
Circuit 5	0.011	0.001	0.644
Circuit 6	0.009	0.000	0.566
Circuit 7	0.008	0.004	0.759
Circuit 8	0.010	0.000	0.812
Circuit 9	0.025	0.000	0.451
Circuit 10	0.012	0.000	0.422
Circuit 11	0.011	0.000	0.740
Circuit 12	0.010	0.000	0.824

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include circuit fixed effects, year and quarter fixed effects, 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 10, Panel B. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to appellate physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 2.2C -- Impact of Physical Takings Precedent on Growth
Robustness of IV Distributed Lag Estimates Across Controls, Lag Structure, and Leads

	The Effect of Appellate Physical Takings Precedent on ΔLog GDP					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. Add Circuit-Specific Trends	0.002	0.015*	0.008+	0.013+	0.003	
	(0.008)	(0.008)	(0.004)	(0.007)	(0.009)	
B. No Fixed Effects	-0.017	-0.005	0.019*	0.012	-0.004	
	(0.022)	(0.006)	(0.009)	(0.015)	(0.008)	
C. State Cluster	0.005	0.019*	0.010+	0.014*	0.005	
	(0.009)	(0.008)	(0.005)	(0.006)	(0.007)	
D. Control for Expectation	0.006	0.023*	0.013**	0.015*	0.008	
	(0.012)	(0.009)	(0.005)	(0.007)	(0.011)	
E. Use Population Weights	-0.001	0.014+	0.016+	0.015**	0.003	
	(0.008)	(0.008)	(0.009)	(0.004)	(0.007)	
F. Drop 1 Circuit						
Drop Circuit 1	0.001	0.019*	0.010**	0.012*	0.005	
	(0.010)	(0.009)	(0.004)	(0.006)	(0.010)	
Drop Circuit 2	-0.002	0.019+	0.011**	0.011**	0.003	
	(0.008)	(0.010)	(0.004)	(0.004)	(0.010)	
Drop Circuit 3	0.008	0.021*	0.010*	0.017*	0.004	
	(0.010)	(0.009)	(0.004)	(0.007)	(0.009)	
Drop Circuit 4	0.004	0.019*	0.009*	0.014*	0.005	
	(0.011)	(0.009)	(0.004)	(0.006)	(0.010)	
Drop Circuit 5	0.006	0.024**	0.008*	0.011+	0.007	
	(0.009)	(0.007)	(0.004)	(0.006)	(0.009)	
Drop Circuit 6	0.002	0.019*	0.013**	0.012*	0.002	
	(0.010)	(0.009)	(0.004)	(0.006)	(0.009)	
Drop Circuit 7	0.004	0.011*	0.008+	0.015*	0.003	
	(0.011)	(0.005)	(0.005)	(0.007)	(0.009)	
Drop Circuit 8	0.008	0.015+	0.008+	0.013+	0.004	
	(0.009)	(0.008)	(0.005)	(0.007)	(0.009)	
Drop Circuit 9	0.010	0.021	0.033*	0.031	0.030**	
	(0.016)	(0.015)	(0.015)	(0.023)	(0.009)	
Drop Circuit 10	0.006	0.020**	0.012**	0.014**	0.008	
	(0.008)	(0.007)	(0.004)	(0.005)	(0.008)	
Drop Circuit 11	0.006	0.021*	0.009*	0.014*	0.006	
	(0.010)	(0.009)	(0.004)	(0.007)	(0.010)	
Drop Circuit 12	0.003	0.020*	0.010**	0.014*	0.004	
	(0.010)	(0.009)	(0.004)	(0.006)	(0.009)	
G. Lag Structure						
1 Lag	-0.003	0.007				
	(0.009)	(0.007)				
2 Lags	-0.006	0.013*	0.009			
	(0.008)	(0.006)	(0.008)			
2 Leads, 4 Lags	0.005	0.018+	0.010*	0.014**	0.005	
	(0.010)	(0.010)	(0.005)	(0.005)	(0.009)	
1 Lead, 5 Lags	-0.000	0.020**	0.018**	0.017*	0.004	0.005
	(0.008)	(0.008)	(0.004)	(0.006)	(0.009)	(0.005)
4 Leads, 1 Lag	-0.000	0.002	0.004	-0.018*	0.006	0.004
(t0, t1, f4, f3, f2, f1)	(0.007)	(0.006)	(0.010)	(0.009)	(0.008)	(0.009)

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. 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 10, Panel B. Coefficients on the lags are shown here. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to appellate physical takings cases in a circuit-year. Expectation controls are the expected probability of being assigned a Democratic minority appointee per seat and a Republic prior U.S. Attorney per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 2.3A - Impact of Physical Takings Precedent on Housing Inequality

Dependent Variable	Home Ownership				Live in Public Housing				Living Below Poverty Line			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proportion Pro-Taking	0.00131	-0.00879*	-0.00915	0.00355	-0.00131	-0.00105	-0.00112	-0.00573	0.000634	0.00839*	0.00656	0.00602
Appellate Decisions$_{t+1}$	(0.00338)	(0.00377)	(0.0137)	(0.0150)	(0.00106)	(0.00214)	(0.00208)	(0.00527)	(0.00416)	(0.00409)	(0.00437)	(0.0112)
Proportion Pro-Taking	0.0106**	0.0128+	0.0204*	0.0257	-0.00147	-0.000193	-0.000831	-0.00145	-0.000716	0.00397	-0.00200	0.00530
Appellate Decisions$_{t}$	(0.00310)	(0.00773)	(0.00980)	(0.0157)	(0.00104)	(0.00182)	(0.00121)	(0.00582)	(0.00306)	(0.00648)	(0.00640)	(0.0137)
Proportion Pro-Taking	0.0131**	0.0121	0.0103*	0.00621	0.00123	0.00333	0.00242	0.00630+	-0.00111	-0.00331	-0.00521	0.00794
Appellate Decisions$_{t-1}$	(0.00350)	(0.00957)	(0.00485)	(0.0252)	(0.00157)	(0.00292)	(0.00360)	(0.00362)	(0.00231)	(0.00596)	(0.00692)	(0.0142)
Proportion Pro-Taking	0.00435	-0.00353	-0.00395	0.0211	-0.00104	-0.000512	0.000278	0.000541	0.000334	0.000240	0.00215	-0.0137
Appellate Decisions$_{t-2}$	(0.00400)	(0.0101)	(0.00984)	(0.0293)	(0.00103)	(0.00348)	(0.00202)	(0.00518)	(0.00204)	(0.00460)	(0.00238)	(0.0253)
Proportion Pro-Taking	0.000375	-0.00598	-0.000298	0.0125	-0.0000532	-0.000601	-0.00142	-0.00187	-0.00138	0.00947	0.00783	-0.00786
Appellate Decisions$_{t-3}$	(0.00353)	(0.0137)	(0.00322)	(0.0209)	(0.00109)	(0.00268)	(0.00249)	(0.00650)	(0.00434)	(0.00697)	(0.00646)	(0.0163)
Proportion Pro-Taking	0.00397	0.00223	0.00313	0.0409*	-0.000632	0.00205	0.000561	0.00856	-0.00524	0.00591	0.00127	-0.0280
Appellate Decisions$_{t-4}$	(0.00354)	(0.00950)	(0.00999)	(0.0184)	(0.00132)	(0.00239)	(0.00287)	(0.0108)	(0.00399)	(0.00822)	(0.00870)	(0.0314)
Proportion Pro-Takings	-0.0131	-0.0186	-0.00437	-0.0344	0.00230	0.00988	0.00575	0.00946	0.000825	0.000764	0.00460	0.0485
Appellate Decisions$_{t+1}$ * Non-White	(0.0119)	(0.0204)	(0.0257)	(0.0444)	(0.00503)	(0.00823)	(0.00811)	(0.0223)	(0.00971)	(0.0164)	(0.0140)	(0.0307)
Proportion Pro-Takings	-0.0386**	-0.0586**	-0.0540**	-0.0823**	0.00583	0.00894*	0.00254	0.00283	0.00459	0.0245+	0.0121	0.00872
Appellate Decisions$_{t}$ * Non-White	(0.0112)	(0.0107)	(0.0175)	(0.0302)	(0.00504)	(0.00396)	(0.00666)	(0.0271)	(0.00779)	(0.0146)	(0.0242)	(0.0711)
Proportion Pro-Takings	-0.0289+	-0.00407	-0.0250*	0.0168	0.00909	0.000209	0.00811	-0.0256	0.00158	0.00166	0.0144	-0.117+
Appellate Decisions$_{t-1}$ * Non-White	(0.0132)	(0.0270)	(0.0117)	(0.105)	(0.00819)	(0.00468)	(0.00964)	(0.0264)	(0.00930)	(0.0118)	(0.0110)	(0.0621)
Proportion Pro-Takings	-0.0210	0.0113	0.0111	-0.00498	0.00971	0.00405	0.0121**	0.0287	0.00569	-0.00293	0.00114	0.0504
Appellate Decisions$_{t-2}$ * Non-White	(0.0143)	(0.0255)	(0.0270)	(0.111)	(0.00549)	(0.00390)	(0.00367)	(0.0358)	(0.0132)	(0.0168)	(0.0149)	(0.135)
Proportion Pro-Takings	-0.0328+	-0.0443	-0.0315	-0.0949	0.0102*	0.0104	-0.000374	0.0465	0.0287*	0.00874	0.00600	0.125
Appellate Decisions$_{t-3}$ * Non-White	(0.0168)	(0.0591)	(0.0340)	(0.130)	(0.00371)	(0.0118)	(0.00580)	(0.0388)	(0.0130)	(0.0203)	(0.0150)	(0.0992)
Proportion Pro-Takings	-0.0314+	-0.0292	-0.0298	-0.125	0.00865*	0.00359	-0.00764	-0.0239	0.0259+	-0.00175	-0.00128	0.00978
Appellate Decisions$_{t-4}$ * Non-White	(0.0162)	(0.0436)	(0.0421)	(0.0788)	(0.00291)	(0.00820)	(0.0103)	(0.0428)	(0.0121)	(0.0257)	(0.0253)	(0.121)
Appellate IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV
District IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV
Aggregation Level	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual
N	4098609	4098609	4098609	4098609	4098609	4098609	4098609	4098609	4098609	4098609	4098609	4098609
R-sq	0.062	0.062	0.062	0.060	0.024	0.024	0.023	0.022	0.041	0.041	0.041	0.038
Mean dependent variable (Non-White)	0.522	0.522	0.522	0.522	0.079	0.079	0.079	0.079	0.266	0.266	0.266	0.266
Mean dependent variable (White)	0.721	0.721	0.721	0.721	0.017	0.017	0.017	0.017	0.117	0.117	0.117	0.117
Average interaction lag	-0.031	-0.025	-0.026	-0.058	0.009	0.005	0.003	0.006	0.013	0.006	0.006	0.015
Average level effect lag	0.004	0.004	0.006	0.023	-0.001	0.000	0.000	0.001	-0.001	0.005	0.001	-0.010
P value of chi-sq of interaction lags	0.063	0.000	0.000	0.002	0.016	0.000	0.000	0.382	0.000	0.003	0.328	0.024
P value of chi-sq of level effect lags	0.020	0.111	0.000	0.000	0.002	0.647	0.534	0.032	0.076	0.020	0.001	0.573
Average no appeal interaction lag	-0.038	-0.038	-0.042	-0.033	0.014	0.013	0.016	0.016	0.033	0.034	0.034	0.032
Average no appeal level effect lag	0.007	0.005	0.012	0.009	-0.001	-0.000	-0.002	-0.002	-0.005	-0.003	-0.007	-0.006
P value of no appeal interaction lags	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
P value no appeal level effect lags	0.014	0.229	0.005	0.002	0.006	0.101	0.346	0.004	0.003	0.000	0.000	0.381
Typical												
Conditional interaction effect	-0.0055	-0.0045	-0.0046	-0.0103	0.0016	0.0009	0.0005	0.0011	0.0023	0.0011	0.0011	0.0027
Conditional level effect	0.0007	0.0007	0.0011	0.0041	-0.0002	0.0000	0.0000	0.0002	-0.0002	0.0009	0.0002	-0.0018
Unconditional interaction effect - pro	0.0006	0.0016	0.0021	-0.0051	-0.0006	-0.0012	-0.0020	-0.0015	-0.0030	-0.0044	-0.0044	-0.0024
Unconditional interaction effect - anti	0.0030	0.0030	0.0034	0.0026	-0.0011	-0.0010	-0.0013	-0.0013	-0.0026	-0.0027	-0.0027	-0.0026
Unconditional interaction effect - all	0.0047	0.0058	0.0067	-0.0014	-0.0022	-0.0026	-0.0038	-0.0033	-0.0066	-0.0081	-0.0081	-0.0060

Notes: Regressions of housing outcomes use March CPS and regressions of employment outcomes use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. 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. Instruments for regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for physical takings are Democratic Nonwhite Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO Instruments are displayed in Appendix Table A3. Significant at +10%, *5%, **1%

Appendix Table 2.3B - Impact of Physical Takings Precedent on Employment Inequality

Dependent Variable	**Employment Status**				**Hours Worked**				**Log Real Earnings**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Proportion Pro-Taking	0.00438	0.00538	0.00236	-0.0148	0.155	0.176	0.0942	-0.743	0.0254	0.0324	0.0183	-0.0771
Appellate Decisions$_{t+1}$	(0.00268)	(0.00583)	(0.00479)	(0.0112)	(0.151)	(0.198)	(0.177)	(0.538)	(0.0219)	(0.0440)	(0.0360)	(0.0800)
Proportion Pro-Taking	0.00476*	0.0111+	0.0128**	-0.00533	0.135	0.443	0.536**	-0.398	0.0285*	0.0725+	0.0888**	-0.0188
Appellate Decisions$_{t}$	(0.00165)	(0.00652)	(0.00386)	(0.0203)	(0.0936)	(0.336)	(0.205)	(0.961)	(0.0129)	(0.0437)	(0.0251)	(0.129)
Proportion Pro-Taking	0.00433*	0.00369	0.00643	-0.00380	0.161	0.210	0.334	-0.173	0.0268+	0.0288	0.0411	-0.00455
Appellate Decisions$_{t-1}$	(0.00180)	(0.00434)	(0.00486)	(0.0167)	(0.116)	(0.261)	(0.260)	(0.837)	(0.0135)	(0.0289)	(0.0330)	(0.107)
Proportion Pro-Taking	0.00577*	0.00872*	0.00802	0.0187	0.226+	0.375*	0.320	0.796	0.0324*	0.0392	0.0398	0.0983
Appellate Decisions$_{t-2}$	(0.00202)	(0.00395)	(0.00555)	(0.0152)	(0.118)	(0.154)	(0.273)	(0.637)	(0.0145)	(0.0265)	(0.0368)	(0.0884)
Proportion Pro-Taking	0.00545*	0.0170+	0.0136*	0.00171	0.207	0.656	0.539+	-0.00831	0.0369*	0.0911	0.0858*	0.0279
Appellate Decisions$_{t-3}$	(0.00245)	(0.00951)	(0.00607)	(0.0108)	(0.126)	(0.482)	(0.320)	(0.535)	(0.0158)	(0.0620)	(0.0377)	(0.0769)
Proportion Pro-Taking	0.00440	0.0104	0.00930	0.0480*	0.209	0.364	0.386	2.199**	0.0373	0.0781	0.0709+	0.313*
Appellate Decisions$_{t-4}$	(0.00338)	(0.00868)	(0.00650)	(0.0191)	(0.155)	(0.405)	(0.325)	(0.839)	(0.0219)	(0.0532)	(0.0422)	(0.133)
Proportion Pro-Takings	-0.0187*	-0.0299+	-0.0179	0.0137	-0.599*	-1.193*	-0.762+	0.467	-0.102**	-0.187+	-0.118	0.0486
Appellate Decisions$_{t+1}$ * Non-White	(0.00610)	(0.0163)	(0.0111)	(0.0371)	(0.234)	(0.572)	(0.409)	(1.653)	(0.0320)	(0.106)	(0.0733)	(0.201)
Proportion Pro-Takings	-0.0218**	-0.0406*	-0.0341*	-0.00752	-0.654**	-1.460*	-1.167+	0.158	-0.125**	-0.248*	-0.216*	-0.0677
Appellate Decisions$_{t}$ * Non-White	(0.00605)	(0.0192)	(0.0150)	(0.0227)	(0.204)	(0.676)	(0.600)	(0.979)	(0.0331)	(0.113)	(0.0885)	(0.143)
Proportion Pro-Takings	-0.0113	-0.0114	-0.0178	-0.0145	-0.278	-0.280	-0.568	0.0278	-0.0660+	-0.0721+	-0.115+	-0.0904
Appellate Decisions$_{t-1}$ * Non-White	(0.00660)	(0.00907)	(0.0120)	(0.0202)	(0.241)	(0.333)	(0.417)	(0.790)	(0.0320)	(0.0412)	(0.0611)	(0.131)
Proportion Pro-Takings	-0.0134*	-0.00274	-0.00482	-0.0377	-0.367	0.0775	-0.0555	-1.574	-0.0801*	-0.0181	-0.0482	-0.255
Appellate Decisions$_{t-2}$ * Non-White	(0.00568)	(0.00776)	(0.00636)	(0.0348)	(0.214)	(0.323)	(0.250)	(1.244)	(0.0318)	(0.0516)	(0.0448)	(0.222)
Proportion Pro-Takings	-0.0184*	-0.0262	-0.0159	-0.0207	-0.595*	-0.716	-0.417	-1.416	-0.112*	-0.151	-0.109	-0.201
Appellate Decisions$_{t-3}$ * Non-White	(0.00717)	(0.0230)	(0.0126)	(0.0448)	(0.269)	(0.851)	(0.485)	(2.092)	(0.0422)	(0.143)	(0.0784)	(0.296)
Proportion Pro-Takings	-0.0102	-0.0230	-0.0128	-0.0913+	-0.376	-0.643	-0.269	-3.072	-0.0727+	-0.162	-0.0902	-0.536
Appellate Decisions$_{t-4}$ * Non-White	(0.00632)	(0.0202)	(0.0224)	(0.0555)	(0.224)	(0.711)	(0.778)	(2.862)	(0.0351)	(0.117)	(0.131)	(0.358)
Appellate IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV
District IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV
Aggregation Level	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual
N	6720948	6720948	6720948	6720948	6497313	6497313	6497313	6497313	6154598	6154598	6154598	6154598
R-sq	0.095	0.095	0.095	0.093	0.131	0.131	0.131	0.130	0.128	0.128	0.128	0.127
Mean dependent variable (Non-White)	0.655	0.655	0.655	0.655	24.837	24.837	24.837	24.837	3.792	3.792	3.792	3.792
Mean dependent variable (White)	0.742	0.742	0.742	0.742	29.130	29.130	29.130	29.130	4.348	4.348	4.348	4.348
Average interaction lag	-0.015	-0.021	-0.017	-0.034	-0.454	-0.604	-0.495	-1.175	-0.091	-0.130	-0.116	-0.230
Average level effect lag	0.005	0.012	0.010	0.013	0.186	0.456	0.423	0.516	0.032	0.071	0.065	0.090
P value of chi-sq of interaction lags	0.016	0.011	0.001	0.004	0.084	0.003	0.135	0.000	0.019	0.013	0.000	0.000
P value of chi-sq of level effect lags	0.158	0.000	0.000	0.032	0.512	0.008	0.031	0.036	0.342	0.681	0.000	0.004
Average no appeal interaction lag	-0.012	-0.017	-0.010	-0.003	-0.428	-0.551	-0.351	-0.093	-0.078	-0.107	-0.079	-0.043
Average no appeal level effect lag	0.004	0.008	0.008	0.003	0.195	0.340	0.369	0.174	0.027	0.046	0.054	0.031
P value of no appeal interaction lags	0.009	0.002	0.003	0.000	0.056	0.000	0.000	0.003	0.033	0.000	0.001	0.000
P value no appeal level effect lags	0.489	0.000	0.147	0.746	0.085	0.000	0.463	0.541	0.513	0.198	0.017	0.506
Typical												
Conditional interaction effect	-0.0027	-0.0037	-0.0030	-0.0061	-0.0809	-0.1076	-0.0882	-0.2094	-0.0162	-0.0232	-0.0207	-0.0410
Conditional level effect	0.0009	0.0021	0.0018	0.0023	0.0331	0.0813	0.0754	0.0920	0.0057	0.0127	0.0116	0.0160
Unconditional interaction effect - pro	-0.0008	-0.0010	-0.0014	-0.0056	-0.0124	-0.0195	-0.0320	-0.1945	-0.0037	-0.0060	-0.0080	-0.0341
Unconditional interaction effect - anti	0.0010	0.0014	0.0008	0.0002	0.0342	0.0441	0.0281	0.0074	0.0062	0.0086	0.0063	0.0034
Unconditional interaction effect - all	0.0006	0.0008	-0.0003	-0.0052	0.0347	0.0411	0.0066	-0.1843	0.0048	0.0057	0.0007	-0.0294

Notes: Regressions of employment outcomes use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. 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. Instruments for regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for physical takings are Democratic Nonwhite Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO Instruments are displayed in Appendix Table A3. Significant at +10%, *5%, **1%

Appendix Table 2.4 - Impact of Physical Takings Precedent on Condemnations

Dependent Variable	**Log Federal Compensation**				**Log Non-Residential Displacements**				**Log Non-Residential Relocation Costs**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Proportion Pro-Taking	-0.188	-0.480*	-0.244	0.366	-0.274+	0.0188	-0.00462	-0.348	-0.0546	0.291	0.299	0.159
Appellate Decisions$_{t+1}$	(0.138)	(0.245)	(0.242)	(1.241)	(0.126)	(0.165)	(0.174)	(0.591)	(0.211)	(0.527)	(0.636)	(1.518)
Proportion Pro-Taking	-0.114	-0.328+	-0.167	0.597	-0.0796	-0.208	-0.165	-0.137	0.113	-0.343	-0.362	0.317
Appellate Decisions$_{t}$	(0.137)	(0.194)	(0.255)	(0.412)	(0.146)	(0.300)	(0.254)	(0.454)	(0.181)	(0.551)	(0.495)	(1.169)
Proportion Pro-Taking	-0.544*	-0.518	-1.216**	-0.639	-0.00196	0.00893	0.170	0.277	0.171	0.479+	0.441+	0.633+
Appellate Decisions$_{t-1}$	(0.177)	(0.443)	(0.468)	(0.466)	(0.140)	(0.176)	(0.116)	(0.206)	(0.190)	(0.284)	(0.230)	(0.330)
Proportion Pro-Taking	0.0390	0.209	0.140	0.194	0.151	0.161	0.309*	-0.0432	0.303+	0.436	0.677	0.154
Appellate Decisions$_{t-2}$	(0.172)	(0.194)	(0.344)	(0.462)	(0.0980)	(0.157)	(0.127)	(0.249)	(0.140)	(0.286)	(0.457)	(0.383)
Proportion Pro-Taking	-0.455**	-0.885*	-0.413+	0.151	0.204	0.316+	0.310*	0.0901	-0.304	-0.554+	-0.605+	-0.241
Appellate Decisions$_{t-3}$	(0.119)	(0.364)	(0.242)	(0.696)	(0.115)	(0.173)	(0.155)	(0.200)	(0.281)	(0.305)	(0.335)	(0.674)
Proportion Pro-Taking	-0.424+	-0.631**	-0.714**	-1.412	-0.118	-0.151	-0.0160	-0.0806	0.214	0.674	0.664+	0.451
Appellate Decisions$_{t-4}$	(0.194)	(0.216)	(0.242)	(0.948)	(0.0850)	(0.189)	(0.132)	(0.255)	(0.157)	(0.482)	(0.362)	(0.490)
Appellate IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV
District IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV
Aggregation Level	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year
N	612	612	612	612	663	663	663	663	663	663	663	663
R-sq	0.616	0.614	0.611	0.605	0.310	0.308	0.304	0.305	0.395	0.391	0.387	0.393
Mean dependent variable	1986.309	1986.309	1986.309	1986.309	55.722	55.722	55.722	55.722	80.123	80.123	80.123	80.123
Mean log dependent variable	15.503	15.503	15.503	15.503	3.139	3.139	3.139	3.139	12.117	12.117	12.117	12.117
Average lag effect	-0.300	-0.431	-0.474	-0.222	0.031	0.025	0.122	0.021	0.099	0.138	0.163	0.263
P-value of lags	0.013	0.000	0.000	0.005	0.027	0.000	0.000	0.090	0.088	0.012	0.009	0.426
P-value of leads	0.202	0.050	0.314	0.768	0.053	0.909	0.979	0.555	0.800	0.581	0.638	0.917
Average lag of no appeal	-0.113	-0.199	-0.276	-0.015	0.049	0.040	0.156	0.041	0.109	0.129	0.111	0.163
P-value of no appeal lags	0.093	0.000	0.001	0.112	0.012	0.001	0.000	0.715	0.081	0.071	0.024	0.255

Notes: Significant at +10%, *5%, **1%. Data come from FHWA. Heteroskedasticity-robust standard errors are in parentheses and clustered by circuit. Regressions include circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that circuit-year. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. The appellate instrument selected by LASSO is circuit-year level interactions of judges with in-state bachelor degrees per seat and judges with prior government experience per seat assigned to physical takings cases in a circuit-year. District instruments selected by LASSO are judges with LLM or SJDs born in the 1950s and circuit-year level interactions of judges who attended a public law school and judges with LLM or SJDs per seat assigned to physical takings district cases in a circuit-year. All values are in logs of the underlying value plus one. Means of the underlying values are displayed as mean dependent variable. All data is from 1991-2003 except compensation, which is from 1995-2003.

Federal Compensation: Total of the amounts paid, deposited in court, or otherwise made available to a property owner from federal funds pursuant to applicable law. This includes all parcels acquired during the report year where title or possession was vested in the Agency during the reporting period, whether through purchase in the open market, condemnation, or administrative settlement. Includes expenses incidental to transfer of title. Excludes appraisal costs, negotiator fees and other administrative expenses.

Number of Non-Residential Displacements: Number of businesses, nonprofit organizations, and farms who were permanently displaced during the fiscal year by project or program activities and moved to their replacement location. This includes businesses, nonprofit organizations, and farms, that upon displacement, discontinued operations.

Non-Residential Relocation Costs: Total amount paid for nonresidential moving expenses (actual expense and fixed payment) and for reestablishment expenses. Excludes agency administrative costs.

Appendix Table 3.1A - Impact of Regulatory Takings Precedent on House Prices

Dependent Variable	ΔLog Price Index								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proportion Pro-Taking	-0.00349**	-0.00192	-0.00951+	0.00429	0.00203	0.0102	0.00679	0.0206	0.0106
Appellate Decisions$_{t+1}$	(0.000985)	(0.00932)	(0.00517)	(0.00500)	(0.00210)	(0.0139)	(0.0215)	(0.0227)	(0.0204)
Proportion Pro-Taking	0.00156	-0.0108	-0.00615	0.0119*	0.00652	0.0155	0.00503	0.0153+	-0.00131
Appellate Decisions$_{t}$	(0.00232)	(0.0116)	(0.00591)	(0.00593)	(0.00687)	(0.0283)	(0.0150)	(0.00914)	(0.00822)
Proportion Pro-Taking	0.00201	0.00419	0.00500	0.00152	0.00132	0.00120	0.00368	0.0132	0.0114+
Appellate Decisions$_{t-1}$	(0.00135)	(0.0133)	(0.00715)	(0.00794)	(0.00623)	(0.0166)	(0.0156)	(0.0127)	(0.00665)
Proportion Pro-Taking	0.000963	0.0111	0.00676	-0.00281	-0.00330	0.0109	-0.0112	0.00299	0.00419
Appellate Decisions$_{t-2}$	(0.00102)	(0.00966)	(0.00987)	(0.00680)	(0.00496)	(0.0154)	(0.0156)	(0.0178)	(0.0159)
Proportion Pro-Taking	0.00273	0.0166	0.00887	0.00680	-0.000192	-0.00738	-0.000768	0.000989	-0.00523
Appellate Decisions$_{t-3}$	(0.00158)	(0.0159)	(0.00564)	(0.00783)	(0.00456)	(0.0206)	(0.0153)	(0.0108)	(0.0110)
Proportion Pro-Taking	0.00257+	0.00474	0.00524+	0.00874*	0.00934	0.00525	-0.00528	0.0149	0.00887
Appellate Decisions$_{t-4}$	(0.00121)	(0.00867)	(0.00292)	(0.00361)	(0.00599)	(0.0150)	(0.0178)	(0.0276)	(0.0188)
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Y	Y	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	N	Lasso IV	N	Lasso IV
Aggregation Level	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
N	2486744	2486744	2486744	2486744	2486744	250	250	250	250
R-sq	0.082	.	0.024	0.042	0.044	0.197	0.111	0.048	0.087
Mean dependent variable	0.011	0.011	0.011	0.011	0.011	0.013	0.013	0.013	0.013
Average lag effect	0.002	0.005	0.004	0.005	0.003	0.005	-0.002	0.009	0.004
P-value of lags	0.086	0.001	0.208	0.000	0.000	0.963	0.958	0.000	0.050
P-value of leads	0.005	0.837	0.066	0.390	0.333	0.464	0.752	0.363	0.606
Average lag of no appeal	0.003	0.005	0.002	0.005	0.002	0.005	-0.005	0.008	-0.002
P-value of no appeal lags	0.208	0.029	0.373	0.000	0.101	0.965	0.479	0.003	0.093
P-value of unconditional (Law_{ct} + 1[M_{ct} > 0]) lags	0.532	0.000	0.095	0.164	0.000	0.965	0.118	0.447	0.025
Typical									
Conditional effect	0.0008	0.0021	0.0017	0.0021	0.0013	0.0021	-0.0008	0.0038	0.0017
Unconditional effect - pro	-0.0003	0.0002	0.0009	0.0002	0.0005	0.0002	0.0011	0.0007	0.0025
Unconditional effect - anti	-0.0004	-0.0007	-0.0003	-0.0007	-0.0003	-0.0007	0.0007	-0.0011	0.0003
Unconditional effect - all	-0.0008	-0.0006	0.0006	-0.0006	0.0002	-0.0006	0.0019	-0.0005	0.0028

Notes: Significant at +10%, *5%, **1%. 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, a dummy for whether there were no cases in that circuit-year. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic Non-White Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO instruments are displayed in Appendix Table A3.

Appendix Table 3.1B -- Impact of Regulatory Takings Precedent on House Prices -- Robustness of IV Estimates Across Controls

	The Effect of Appellate Regulatory Takings Precedent on ΔLog Price Index		
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
A. Add Circuit-Specific Trends	0.003	0.031	0.592
B. No Fixed Effects	0.006	0.721	0.942
C. State Cluster	0.005	0.012	0.843
D. Control for Expectation	0.007	0.000	0.576
E. Use Population Weights	-0.004	0.054	0.244
F. Add 2-year Lead	0.008	0.005	0.032
G. Drop 1 Circuit			
Circuit 1	0.004	0.002	0.710
Circuit 2	0.003	0.391	0.552
Circuit 3	0.001	0.235	0.763
Circuit 4	0.007	0.003	0.742
Circuit 5	0.007	0.000	0.753
Circuit 6	0.004	0.446	0.217
Circuit 7	0.006	0.000	0.892
Circuit 8	-0.000	0.024	0.794
Circuit 9	0.014	0.001	0.858
Circuit 10	0.006	0.000	0.900
Circuit 11	0.002	0.133	0.883
Circuit 12	0.005	0.001	0.813
H. Circuit-quarter laws	0.013	0.000	0.658

Notes: Significant at +10%, *5%, **1%. 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 regulatory takings precedent, corresponding to column 2 in Table 5, Panel A. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Expectation control is the expected probability of being assigned a Black judge per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 3.1C -- Impact of Regulatory Takings Precedent on House Prices
Robustness of IV Distributed Lag Estimates Across Controls, Lag Structure, Leads, and Local Effects

	The Effect of Appellate Regulatory Takings Precedent on ΔLog Price Index					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. Add Circuit-Specific Trends	-0.008	0.005	0.007	0.013	-0.000	
	(0.010)	(0.013)	(0.007)	(0.014)	(0.008)	
B. No Fixed Effects	-0.005	0.006	0.010	0.016	0.003	
	(0.018)	(0.031)	(0.029)	(0.034)	(0.009)	
C. State Cluster	-0.011	0.004	0.011	0.017	0.005	
	(0.010)	(0.014)	(0.009)	(0.012)	(0.006)	
D. Control for Expectation	-0.010	0.003	0.014	0.018	0.012	
	(0.014)	(0.014)	(0.009)	(0.015)	(0.013)	
E. Use Population Weights	-0.019	-0.011	0.004	0.002	0.006	
	(0.016)	(0.018)	(0.015)	(0.014)	(0.007)	
F. Drop 1 Circuit						
Drop Circuit 1	-0.011	0.003	0.010	0.015	0.005	
	(0.010)	(0.012)	(0.010)	(0.014)	(0.008)	
Drop Circuit 2	-0.014	0.002	0.007	0.015	0.004	
	(0.012)	(0.016)	(0.010)	(0.015)	(0.011)	
Drop Circuit 3	-0.008	0.003	0.004	0.007	-0.002	
	(0.012)	(0.021)	(0.018)	(0.018)	(0.008)	
Drop Circuit 4	-0.010	0.014	0.010	0.022	-0.001	
	(0.019)	(0.030)	(0.015)	(0.027)	(0.022)	
Drop Circuit 5	-0.008	0.006	0.013	0.017	0.006	
	(0.012)	(0.013)	(0.009)	(0.013)	(0.008)	
Drop Circuit 6	-0.011	0.002	0.009	0.010	0.011	
	(0.012)	(0.009)	(0.010)	(0.014)	(0.010)	
Drop Circuit 7	-0.009	0.006	0.012	0.015	0.005	
	(0.012)	(0.012)	(0.008)	(0.013)	(0.008)	
Drop Circuit 8	-0.012	-0.001	0.009	0.006	-0.004	
	(0.010)	(0.009)	(0.012)	(0.014)	(0.006)	
Drop Circuit 9	-0.008	0.019	0.020	0.034	0.003	
	(0.030)	(0.028)	(0.017)	(0.029)	(0.024)	
Drop Circuit 10	-0.011	0.007	0.012	0.019	0.004	
	(0.014)	(0.016)	(0.009)	(0.018)	(0.012)	
Drop Circuit 11	-0.023	0.003	0.003	0.029	-0.002	
	(0.018)	(0.026)	(0.010)	(0.036)	(0.033)	
Drop Circuit 12	-0.011	0.004	0.011	0.017	0.005	
	(0.012)	(0.013)	(0.010)	(0.016)	(0.009)	
G. Lag Structure						
1 Lag	-0.001	0.004				
	(0.014)	(0.007)				
2 Lags	-0.001	0.008	0.008			
	(0.016)	(0.011)	(0.008)			
2 Leads, 4 Lags	-0.007	0.001	0.019	0.013	0.014	
	(0.012)	(0.014)	(0.014)	(0.014)	(0.013)	
1 Lead, 5 Lags	-0.010+	0.003	0.003	0.020	0.004	0.014
	(0.005)	(0.015)	(0.015)	(0.019)	(0.009)	(0.016)
4 Leads, 1 Lag	0.002	0.010	0.004	0.000	0.002	0.005
(t0, t1, f4, f3, f2, f1)	(0.008)	(0.015)	(0.016)	(0.017)	(0.017)	(0.007)
	(q0)	(q4)	(q8)	(q12)	(q16)	Mean
H. Circuit-quarter laws	0.002	0.024	0.015	0.006	-0.0001	0.013
	(0.020)	(0.023)	(0.031)	(0.026)	(0.011)	
I. Circuit-quarter laws (Law_{ct})	-0.004	0.041	0.031	0.020	-0.011	0.026
controlling for	(0.034)	(0.027)	(0.036)	(0.044)	(0.021)	
Local takings decision ($LocalLaw_{ict}$)	0.093	0.021	-0.029	-0.148*	-0.111	-0.054
	(0.061)	(0.062)	(0.113)	(0.066)	(0.074)	

Notes: Significant at +10%, *5%, **1%. 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 regulatory takings precedent, corresponding to column 2 in Table 5, Panel A. Coefficients on the lags are shown here. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Expectation control is the expected probability of being assigned a Black judge per seat in a circuit-year. Population weights are based on the 2005 U.S. Census estimates at the zip-code level.

Appendix Table 3.1D -- Impact of Regulatory Takings Precedent on House Prices - Robustness of IV Distributed Lag Estimates in Levels

	The Effect of Appellate Regulatory Takings Precedent on Log Price Index					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. No Circuit-Specific Trends	-0.147**	-0.174+	-0.075	-0.034	0.076	
	(0.056)	(0.104)	(0.111)	(0.104)	(0.089)	
B. No Fixed Effects	-0.285	-0.705	-0.356	-0.516	0.227	
	(0.371)	(0.809)	(0.521)	(0.881)	(0.455)	
C. State Cluster	-0.148*	-0.152*	-0.075	0.021	0.040	
	(0.068)	(0.065)	(0.073)	(0.080)	(0.101)	
D. Control for Expectation	-0.183*	-0.168	-0.078	-0.000	0.047	
	(0.076)	(0.107)	(0.125)	(0.103)	(0.153)	
E. Use Population Weights	-0.086	-0.111	-0.084	-0.026	-0.050	
	(0.121)	(0.071)	(0.087)	(0.112)	(0.186)	
F. Drop 1 Circuit						
Drop Circuit 1	-0.139	-0.153	-0.079	0.013	0.039	
	(0.092)	(0.098)	(0.082)	(0.082)	(0.111)	
Drop Circuit 2	-0.099	-0.093	-0.09	-0.009	-0.035	
	(0.11)	(0.1)	(0.098)	(0.06)	(0.153)	
Drop Circuit 3	-0.119	-0.097	-0.036	0.045	0.041	
	(0.143)	(0.145)	(0.105)	(0.109)	(0.167)	
Drop Circuit 4	-0.146*	-0.162	-0.057	0.025	0.064	
	(0.058)	(0.162)	(0.121)	(0.1)	(0.163)	
Drop Circuit 5	-0.166+	-0.162+	-0.067	0.03	0.057	
	(0.085)	(0.085)	(0.089)	(0.086)	(0.087)	
Drop Circuit 6	-0.124	-0.141+	-0.089	-0.027	0.031	
	(0.097)	(0.084)	(0.087)	(0.04)	(0.106)	
Drop Circuit 7	-0.15+	-0.139	-0.055	0.03	0.043	
	(0.089)	(0.094)	(0.072)	(0.069)	(0.087)	
Drop Circuit 8	-0.106	-0.125	-0.031	0.067	0.01	
	(0.112)	(0.107)	(0.083)	(0.135)	(0.138)	
Drop Circuit 9	-0.289**	-0.291	-0.018	0.128	0.183+	
	(0.081)	(0.194)	(0.232)	(0.201)	(0.108)	
Drop Circuit 10	-0.155+	-0.14	-0.082	0.025	0.03	
	(0.091)	(0.108)	(0.102)	(0.073)	(0.134)	
Drop Circuit 11	-0.105	-0.121	-0.153*	-0.005	-0.058	
	(0.114)	(0.142)	(0.071)	(0.072)	(0.161)	
Drop Circuit 12	-0.15	-0.154	-0.076	0.021	0.04	
	(0.098)	(0.105)	(0.088)	(0.081)	(0.115)	
G. Lag Structure						
1 Lag	-0.144	-0.043				
	(0.107)	(0.050)				
2 Lags	-0.158	-0.071	-0.083			
	(0.100)	(0.044)	(0.085)			
2 Leads, 4 Lags	-0.154	-0.165	-0.039	0.024	0.076	
	(0.141)	(0.108)	(0.099)	(0.105)	(0.088)	
1 Lead, 5 Lags	-0.094	-0.135	-0.174	0.020	0.027	0.119
	(0.157)	(0.108)	(0.127)	(0.096)	(0.112)	(0.110)

Appendix Table 3.2A - Impact of Regulatory Takings Precedents on Economic Growth

Dependent Variable	ΔLog GDP								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Proportion Pro-Taking	-0.000509	-0.00644	-0.0171	-0.0437	-0.00450	0.0126	0.0254	0.0240	0.0201
Appellate Decisions$_{t+1}$	(0.00386)	(0.0153)	(0.0456)	(0.0518)	(0.0218)	(0.0181)	(0.0190)	(0.0630)	(0.0179)
Proportion Pro-Taking	-0.000851	-0.00985	0.00299	0.0312	0.0143	0.0121	0.00856	0.0148	0.0168
Appellate Decisions$_{t}$	(0.00486)	(0.0218)	(0.0472)	(0.0385)	(0.0364)	(0.0461)	(0.0284)	(0.0260)	(0.0188)
Proportion Pro-Taking	0.00341	0.00200	0.0436+	-0.0289	0.00367	-0.0135	-0.00438	-0.0159	0.0128
Appellate Decisions$_{t-1}$	(0.00211)	(0.0137)	(0.0223)	(0.0337)	(0.0150)	(0.0254)	(0.0195)	(0.0413)	(0.0203)
Proportion Pro-Taking	0.00833	0.0398*	0.0307	0.0229	0.0108	0.0361	0.0221	0.00107	0.0206
Appellate Decisions$_{t-2}$	(0.00560)	(0.0197)	(0.0360)	(0.0416)	(0.0194)	(0.0294)	(0.0318)	(0.0461)	(0.0203)
Proportion Pro-Taking	0.0105*	0.00587	0.0296	0.000356	0.0439+	0.00197	0.00367	0.0375	0.0122
Appellate Decisions$_{t-3}$	(0.00344)	(0.0208)	(0.0278)	(0.0400)	(0.0264)	(0.0169)	(0.0137)	(0.0468)	(0.0143)
Proportion Pro-Taking	0.00319	0.0421	0.0129	0.00575	-0.00156	0.0245	0.0134	0.0718	0.0411+
Appellate Decisions$_{t-4}$	(0.00555)	(0.0257)	(0.0158)	(0.0449)	(0.0178)	(0.0307)	(0.0251)	(0.0587)	(0.0230)
Appellate IV	N	Y	Y	Lasso IV	Lasso IV	Y	Y	Lasso IV	Lasso IV
District IV	N	N	Lasso IV	N	Lasso IV	N	Lasso IV	N	Lasso IV
Aggregation Level	State-Year	State-Year	State-Year	State-Year	State-Year	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
N	1065	1065	1065	1065	1065	250	250	250	250
R-sq	0.243	0.124	0.072	0.044	0.059	0.405	0.430	.	0.272
Mean dependent variable	0.056	0.056	0.056	0.056	0.056	0.057	0.057	0.057	0.057
Average lag effect	0.005	0.016	0.024	0.006	0.014	0.012	0.009	0.022	0.021
P-value of lags	0.024	0.066	0.103	0.002	0.004	0.318	0.960	0.026	0.141
P-value of leads	0.897	0.673	0.707	0.399	0.836	0.487	0.182	0.703	0.262
Average lag of no appeal	0.007	0.015	0.026	0.009	0.024	0.011	0.010	0.019	0.022
P-value of no appeal lags	0.004	0.061	0.000	0.005	0.259	0.385	0.768	0.093	0.244
P-value of unconditional ($Law_{ct} + 1[M_{ct} > 0]$) lags	0.124	0.434	0.189	0.014	0.382	0.167	0.005	0.546	0.636
Typical									
Conditional effect	0.0021	0.0067	0.0101	0.0025	0.0059	0.0051	0.0038	0.0093	0.0088
Unconditional effect - pro	-0.0006	0.0009	-0.00003	-0.0010	-0.0035	0.0008	-0.0001	0.0019	0.0003
Unconditional effect - anti	-0.0010	-0.0021	-0.0036	-0.0013	-0.0034	-0.0015	-0.0014	-0.0027	-0.0031
Unconditional effect - all	-0.0017	-0.0014	-0.0039	-0.0023	-0.0071	-0.0009	-0.0016	-0.0010	-0.0030

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. 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. Instruments for appellate regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO instruments are displayed in Appendix Table A3.

Appendix Table 3.2B -- Impact of Regulatory Takings Precedent on Growth -- Robustness of IV Estimates Across Controls

	The Effect of Appellate Regulatory Takings Precedent on ΔLog GDP		
	Average of yearly lags	P-value of lags	P-value of leads
	(1)	(2)	(3)
A. Add Circuit-Specific Trends	0.013	0.195	0.960
B. No Fixed Effects	0.022	0.005	0.967
C. State Cluster	0.016	0.048	0.735
D. Control for Expectation	0.017	0.036	0.428
E. Use Population Weights	-0.007	0.000	0.854
G. Drop 1 Circuit			
Circuit 1	0.011	0.061	0.522
Circuit 2	0.013	0.058	0.747
Circuit 3	0.015	0.387	0.517
Circuit 4	0.024	0.245	0.916
Circuit 5	0.017	0.074	0.685
Circuit 6	0.013	0.293	0.525
Circuit 7	0.017	0.104	0.686
Circuit 8	0.010	0.067	0.883
Circuit 9	0.024	0.171	0.938
Circuit 10	0.019	0.266	0.803
Circuit 11	0.013	0.034	0.829
Circuit 12	0.014	0.000	0.376

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. 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 regulatory takings precedent, corresponding to column 2 in Table 10, Panel A. Instruments for appellate regulatory takings are Black judges per seat assigned to appellate regulatory takings cases in a circuit-year. Expectation control is the expected probability of being assigned a Black judge per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 3.2C -- Impact of Regulatory Takings Precedent on Growth
Robustness of IV Distributed Lag Estimates Across Controls, Lag Structure, and Leads

	The Effect of Appellate Regulatory Takings Precedent on ΔLog GDP					
	(t0)	(t1)	(t2)	(t3)	(t4)	(t5)
A. Add Circuit-Specific Trends	-0.004	0.003	0.032+	-0.001	0.034+	
	(0.019)	(0.010)	(0.017)	(0.015)	(0.018)	
B. No Fixed Effects	0.015	-0.010	0.054	0.019	0.033	
	(0.026)	(0.033)	(0.042)	(0.043)	(0.039)	
C. State Cluster	-0.010	0.002	0.040*	0.006	0.042*	
	(0.018)	(0.016)	(0.015)	(0.015)	(0.021)	
D. Control for Expectation	-0.017	-0.003	0.043*	0.007	0.054	
	(0.026)	(0.018)	(0.022)	(0.024)	(0.034)	
E. Use Population Weights	-0.047*	-0.033*	0.007	0.003	0.034*	
	(0.020)	(0.014)	(0.022)	(0.032)	(0.014)	
F. Drop 1 Circuit						
Drop Circuit 1	-0.020	-0.000	0.034*	0.004	0.037+	
	(0.018)	(0.010)	(0.015)	(0.022)	(0.022)	
Drop Circuit 2	-0.009	-0.000	0.030+	0.010	0.036	
	(0.023)	(0.012)	(0.018)	(0.018)	(0.025)	
Drop Circuit 3	-0.009	0.005	0.044	0.002	0.032	
	(0.026)	(0.016)	(0.029)	(0.026)	(0.031)	
Drop Circuit 4	0.009	0.009	0.049+	0.012	0.044	
	(0.022)	(0.020)	(0.029)	(0.022)	(0.034)	
Drop Circuit 5	-0.007	0.005	0.043*	0.003	0.042+	
	(0.023)	(0.014)	(0.019)	(0.019)	(0.023)	
Drop Circuit 6	-0.010	0.012	0.040	-0.011	0.037	
	(0.032)	(0.015)	(0.025)	(0.022)	(0.028)	
Drop Circuit 7	-0.008	0.006	0.041*	0.005	0.040+	
	(0.023)	(0.011)	(0.019)	(0.021)	(0.023)	
Drop Circuit 8	-0.012	0.005	0.030*	0.001	0.023+	
	(0.019)	(0.010)	(0.015)	(0.020)	(0.013)	
Drop Circuit 9	-0.013	-0.017	0.055+	0.033	0.064	
	(0.033)	(0.043)	(0.033)	(0.030)	(0.055)	
Drop Circuit 10	-0.006	-0.001	0.037	0.007	0.056+	
	(0.027)	(0.017)	(0.023)	(0.021)	(0.029)	
Drop Circuit 11	-0.019	-0.004	0.037+	-0.000	0.053	
	(0.023)	(0.022)	(0.021)	(0.027)	(0.040)	
Drop Circuit 12	-0.022	0.001	0.035+	0.009	0.047*	
	(0.021)	(0.015)	(0.020)	(0.024)	(0.024)	
G. Lag Structure						
1 Lag	0.011	0.012				
	(0.022)	(0.010)				
2 Lags	-0.000	0.032*	0.032+			
	(0.025)	(0.013)	(0.019)			
2 Leads, 4 Lags	-0.002	0.006	0.051	0.001	0.051+	
	(0.026)	(0.023)	(0.031)	(0.024)	(0.028)	
1 Lead, 5 Lags	-0.014	0.006	0.045	0.003	0.026	-0.019
	(0.024)	(0.010)	(0.029)	(0.015)	(0.019)	(0.022)
4 Leads, 1 Lag	0.027	0.012	-0.003	0.008	0.015	0.002
(t0, t1, f4, f3, f2, f1)	(0.017)	(0.020)	(0.031)	(0.019)	(0.026)	(0.021)

Notes: Significant at +10%, *5%, **1%. State-level GDP data are from the Bureau of Economic Analysis. 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 regulatory takings precedent, corresponding to column 2 in Table 10, Panel A. Coefficients on the lags are shown here. Instruments for appellate regulatory takings are Black judges per seat assigned to appellate regulatory takings cases in a circuit-year. Expectation control is the expected probability of being assigned a Black judge per seat in a circuit-year. Population weights are based on the 2005 US Census estimates at the zip-code level.

Appendix Table 3.3A - Impact of Regulatory Takings Precedent on Housing Inequality

Dependent Variable	Home Ownership				Live in Public Housing				Living Below Poverty Line			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Proportion Pro-Taking	0.00309	-0.0231	-0.000434	-0.000965	-0.00113	0.00621	0.00141	0.00409	0.00379	-0.0132	0.0102	0.0168
Appellate Decisions$_{t+1}$	(0.00352)	(0.0312)	(0.0250)	(0.0192)	(0.000915)	(0.00965)	(0.00585)	(0.00795)	(0.00260)	(0.0101)	(0.0152)	(0.0182)
Proportion Pro-Taking	0.00540*	-0.0136	0.00763	-0.00638	-0.00177+	-0.00196	-0.00675	0.00742	-0.000802	0.0110	0.00904	0.00462
Appellate Decisions$_{t}$	(0.00241)	(0.0198)	(0.0151)	(0.0110)	(0.000911)	(0.00732)	(0.00803)	(0.00822)	(0.00307)	(0.0150)	(0.0218)	(0.0154)
Proportion Pro-Taking	0.00685*	-0.0192	0.00373	-0.0137	-0.000292	-0.00345	-0.00163	0.00890+	0.000537	0.0181+	0.00137	0.0129
Appellate Decisions$_{t-1}$	(0.00287)	(0.0210)	(0.0177)	(0.0124)	(0.00112)	(0.00484)	(0.00614)	(0.00466)	(0.00346)	(0.0104)	(0.0130)	(0.00854)
Proportion Pro-Taking	0.00151	0.00748	-0.0221	-0.0184	-0.000534	-0.00368	-0.00362	0.00303	0.00196	-0.00852	-0.0240	0.00176
Appellate Decisions$_{t-2}$	(0.00324)	(0.0149)	(0.0175)	(0.0131)	(0.000678)	(0.00460)	(0.00328)	(0.00796)	(0.00274)	(0.0164)	(0.0242)	(0.0209)
Proportion Pro-Taking	-0.00269	-0.00252	0.000705	0.0159	-0.00175+	0.00212	-0.000897	-0.0100	-0.000623	-0.00711	-0.0116	-0.0151
Appellate Decisions$_{t-3}$	(0.00293)	(0.0167)	(0.0159)	(0.0332)	(0.000833)	(0.00476)	(0.00597)	(0.0141)	(0.00301)	(0.0226)	(0.0164)	(0.0419)
Proportion Pro-Taking	-0.000317	0.00499	-0.0157	-0.0153	-0.000260	0.00385	0.00865	0.00706	-0.00287	0.00133	-0.00133	-0.0102
Appellate Decisions$_{t-4}$	(0.00264)	(0.0228)	(0.0165)	(0.0206)	(0.00105)	(0.00601)	(0.00580)	(0.00978)	(0.00337)	(0.0240)	(0.0115)	(0.0185)
Proportion Pro-Takings	-0.0182	0.00575	-0.0488	0.0802	0.00470	0.0186	0.0379	-0.0240	-0.000804	-0.0240	-0.0306	-0.146+
Appellate Decisions$_{t+1}$ * Non-White	(0.0175)	(0.0848)	(0.0908)	(0.0755)	(0.00381)	(0.0309)	(0.0371)	(0.0312)	(0.0134)	(0.0716)	(0.0525)	(0.0880)
Proportion Pro-Takings	-0.0228+	0.0122	-0.166*	-0.0408	0.0104*	0.0177	0.0319	0.00287	0.00679	-0.0751	0.0192	0.00374
Appellate Decisions$_{t}$ * Non-White	(0.0115)	(0.0643)	(0.0779)	(0.0438)	(0.00391)	(0.0215)	(0.0329)	(0.0237)	(0.0126)	(0.0736)	(0.0682)	(0.0645)
Proportion Pro-Takings	-0.0348**	-0.0379	0.00652	0.0186	0.0153+	0.0259	0.0113	-0.0163	0.0231*	0.0000549	0.00262	-0.0343
Appellate Decisions$_{t-1}$ * Non-White	(0.00955)	(0.0485)	(0.0605)	(0.0308)	(0.00781)	(0.0211)	(0.0228)	(0.0141)	(0.00882)	(0.0385)	(0.0244)	(0.0276)
Proportion Pro-Takings	-0.0388**	-0.0795+	0.0666	-0.0282	0.0141*	0.0197	0.0171	0.00688	0.0284+	0.119	0.110+	0.110
Appellate Decisions$_{t-2}$ * Non-White	(0.00893)	(0.0481)	(0.0612)	(0.0839)	(0.00594)	(0.0310)	(0.0270)	(0.0337)	(0.0135)	(0.0853)	(0.0644)	(0.0868)
Proportion Pro-Takings	-0.0216*	-0.0537	-0.0920	-0.115	0.00292	0.0274+	0.0428	0.0552	0.0129	0.0866	0.00384	-0.0358
Appellate Decisions$_{t-3}$ * Non-White	(0.00871)	(0.0511)	(0.0640)	(0.175)	(0.00269)	(0.0162)	(0.0271)	(0.0742)	(0.0132)	(0.0646)	(0.0473)	(0.201)
Proportion Pro-Takings	-0.0117	-0.0157	0.00713	0.0181	0.00563	-0.0249	-0.0368*	-0.00831	0.0190	-0.00143	0.0168	0.0981
Appellate Decisions$_{t-4}$ * Non-White	(0.0117)	(0.0498)	(0.0788)	(0.124)	(0.00361)	(0.0162)	(0.0179)	(0.0483)	(0.0109)	(0.0606)	(0.0302)	(0.125)
Appellate IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV
District IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV
Aggregation Level	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual
N	3227637	3227637	3227637	3227637	3227637	3227637	3227637	3227637	3227637	3227637	3227637	3227637
R-sq	0.065	0.065	0.062	0.063	0.024	0.022	0.019	0.020	0.043	0.041	0.041	0.034
Mean dependent variable (Non-White)	0.512	0.512	0.512	0.512	0.080	0.080	0.080	0.080	0.266	0.266	0.266	0.266
Mean dependent variable (White)	0.714	0.714	0.714	0.714	0.017	0.017	0.017	0.017	0.119	0.119	0.119	0.119
Average interaction lag	-0.026	-0.035	-0.036	-0.029	0.010	0.013	0.013	0.008	0.018	0.026	0.031	0.028
Average level effect lag	0.001	-0.003	-0.005	-0.002	-0.001	-0.000	-0.001	-0.001	0.000	0.002	-0.005	-0.007
P value of chi-sq of interaction lags	0.015	0.000	0.013	0.000	0.016	0.000	0.081	0.205	0.035	0.549	0.000	0.607
P value of chi-sq of level effect lags	0.126	0.307	0.733	0.000	0.114	0.733	0.010	0.000	0.244	0.299	0.222	0.279
Average no appeal interaction lag	-0.046	-0.048	-0.040	-0.084	0.015	0.011	0.005	0.027	0.040	0.043	0.041	0.071
Average no appeal level effect lag	0.006	0.001	0.003	0.004	-0.001	-0.000	-0.000	-0.001	-0.004	-0.001	-0.013	-0.013
P value of no appeal interaction lags	0.000	0.000	0.095	0.000	0.000	0.000	0.427	0.000	0.000	0.000	0.001	0.002
P value no appeal level effect lags	0.060	0.861	0.483	0.932	0.012	0.871	0.214	0.532	0.051	0.512	0.000	0.198
Typical												
Conditional interaction effect	-0.0110	-0.0147	-0.0152	-0.0122	0.0042	0.0055	0.0055	0.0034	0.0076	0.0110	0.0131	0.0118
Conditional level effect	0.0004	-0.0013	-0.0021	-0.0008	-0.0004	0.0000	-0.0004	-0.0004	0.0000	0.0008	-0.0021	-0.0029
Unconditional interaction effect - pro	0.0070	0.0040	0.0004	0.0205	-0.0016	0.0012	0.0035	-0.0072	-0.0080	-0.0058	-0.0029	-0.0159
Unconditional interaction effect - anti	0.0064	0.0067	0.0056	0.0118	-0.0021	-0.0015	-0.0007	-0.0038	-0.0056	-0.0060	-0.0057	-0.0099
Unconditional interaction effect - all	0.0139	0.0112	0.0064	0.0331	-0.0039	-0.0005	0.0028	-0.0112	-0.0140	-0.0123	-0.0091	-0.0265

Notes: Regressions of housing outcomes use March CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. 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. Instruments for regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for physical takings are Democratic Nonwhite Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO Instruments are displayed in Appendix Table A3. Significant at +10%, *5%, **1%

Appendix Table 3.3B - Impact of Regulatory Takings Precedent on Employment Inequality

Dependent Variable	Employment Status				Hours Worked				Log Real Earnings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Proportion Pro-Taking	-0.00111	0.0398	0.0193	0.0190*	-0.171	1.352	0.387	0.778	-0.0154	0.193	0.111	0.103
Appellate Decisions$_{t+1}$	(0.00243)	(0.0345)	(0.0336)	(0.00910)	(0.114)	(1.446)	(1.403)	(0.548)	(0.0166)	(0.180)	(0.195)	(0.0678)
Proportion Pro-Taking	-0.00205	0.00842	-0.0000714	-0.00930	-0.196+	-0.224	-0.497	-0.824	-0.0192	0.0125	0.00101	-0.0496
Appellate Decisions$_{t}$	(0.00209)	(0.0207)	(0.0114)	(0.0207)	(0.103)	(0.745)	(0.424)	(1.053)	(0.0134)	(0.116)	(0.0657)	(0.121)
Proportion Pro-Taking	-0.000683	0.0170	0.0121	0.00831	-0.0972	0.501	0.464	0.355	-0.0107	0.0520	0.0503	0.0396
Appellate Decisions$_{t-1}$	(0.00281)	(0.0223)	(0.0114)	(0.00974)	(0.119)	(1.036)	(0.720)	(0.585)	(0.0155)	(0.127)	(0.0673)	(0.0659)
Proportion Pro-Taking	0.000887	0.0290+	0.00325	-0.0146	-0.0349	1.187+	0.457	-1.088	-0.00249	0.134	0.0142	-0.0887
Appellate Decisions$_{t-2}$	(0.00210)	(0.0161)	(0.0156)	(0.0209)	(0.0908)	(0.713)	(0.859)	(0.940)	(0.0118)	(0.0988)	(0.0806)	(0.124)
Proportion Pro-Taking	0.000540	0.0247	0.0183	0.0289	0.0330	1.451	1.103	1.756	0.00128	0.130	0.0881	0.195+
Appellate Decisions$_{t-3}$	(0.00223)	(0.0226)	(0.0173)	(0.0196)	(0.115)	(1.136)	(0.786)	(1.114)	(0.0138)	(0.143)	(0.0881)	(0.107)
Proportion Pro-Taking	0.00210	0.00616	-0.00924	-0.0202	0.172	0.432	-0.0499	-0.843	0.0148	0.0379	-0.0393	-0.0998
Appellate Decisions$_{t-4}$	(0.00244)	(0.0215)	(0.0232)	(0.0304)	(0.0967)	(0.944)	(0.949)	(1.375)	(0.0131)	(0.127)	(0.138)	(0.165)
Proportion Pro-Takings	-0.0126	-0.0870	-0.0246	-0.0195	-0.301	-2.769	-0.248	-1.141	-0.0595	-0.377	-0.0785	-0.100
Appellate Decisions$_{t+1}$ * Non-White	(0.00735)	(0.103)	(0.0476)	(0.0274)	(0.242)	(4.176)	(1.776)	(1.589)	(0.0393)	(0.559)	(0.255)	(0.163)
Proportion Pro-Takings	-0.0163*	-0.0677	-0.0366	0.0198	-0.481+	-2.079	-0.613	1.990	-0.0890*	-0.321	-0.224	0.105
Appellate Decisions$_{t}$ * Non-White	(0.00691)	(0.0534)	(0.0547)	(0.0361)	(0.223)	(2.217)	(1.735)	(2.099)	(0.0333)	(0.305)	(0.291)	(0.195)
Proportion Pro-Takings	-0.0153+	-0.0923	-0.0371	-0.0110	-0.492+	-3.608	-1.562	-0.385	-0.0826+	-0.472	-0.201	-0.0581
Appellate Decisions$_{t-1}$ * Non-White	(0.00752)	(0.0605)	(0.0281)	(0.0158)	(0.266)	(2.355)	(1.198)	(0.880)	(0.0415)	(0.335)	(0.135)	(0.0973)
Proportion Pro-Takings	-0.0101	-0.132+	-0.0213	0.0309	-0.240	-5.203	-1.488	2.405	-0.0594	-0.726	-0.113	0.153
Appellate Decisions$_{t-2}$ * Non-White	(0.00597)	(0.0798)	(0.0346)	(0.0474)	(0.224)	(3.441)	(1.366)	(2.528)	(0.0377)	(0.475)	(0.195)	(0.260)
Proportion Pro-Takings	-0.00622	-0.101	-0.0337	-0.0745	-0.124	-4.295	-1.385	-3.905	-0.0321	-0.570	-0.162	-0.416
Appellate Decisions$_{t-3}$ * Non-White	(0.00653)	(0.0906)	(0.0345)	(0.0759)	(0.245)	(3.774)	(1.233)	(3.628)	(0.0404)	(0.525)	(0.184)	(0.407)
Proportion Pro-Takings	-0.00795	-0.0178	0.0183	0.0557	-0.284	-1.397	0.438	3.255	-0.0566	-0.170	0.0633	0.279
Appellate Decisions$_{t-4}$ * Non-White	(0.00774)	(0.0511)	(0.0298)	(0.0819)	(0.277)	(2.203)	(1.090)	(4.134)	(0.0468)	(0.274)	(0.191)	(0.461)
Appellate IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV	N	Y	Y	Lasso IV
District IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV	N	N	Lasso IV	Lasso IV
Aggregation Level	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual
N	5341620	5341620	5341620	5341620	5171040	5171040	5171040	5171040	4892691	4892691	4892691	4892691
R-sq	0.093	0.090	0.092	0.092	0.126	0.124	0.126	0.125	0.125	0.123	0.124	0.124
Mean dependent variable (Non-White)	0.660	0.660	0.660	0.660	25.085	25.085	25.085	25.085	3.817	3.817	3.817	3.817
Mean dependent variable (White)	0.750	0.750	0.750	0.750	29.527	29.527	29.527	29.527	4.405	4.405	4.405	4.405
Average interaction lag	-0.011	-0.082	-0.022	0.004	-0.324	-3.316	-0.922	0.672	-0.064	-0.452	-0.127	0.013
Average level effect lag	0.000	0.015	0.005	0.003	-0.039	0.525	0.295	0.151	-0.004	0.065	0.023	0.030
P value of chi-sq of interaction lags	0.169	0.331	0.137	0.958	0.263	0.425	0.086	0.918	0.002	0.410	0.043	0.951
P value of chi-sq of level effect lags	0.336	0.130	0.773	0.397	0.010	0.064	0.650	0.426	0.012	0.212	0.900	0.206
Average no appeal interaction lag	-0.015	-0.067	-0.010	-0.014	0.135	0.560	0.330	0.380	0.018	0.065	0.036	0.047
Average no appeal level effect lag	0.003	0.014	0.004	0.007	-0.573	-2.771	-0.508	-0.328	0.001	0.195	0.089	0.493
P value of no appeal interaction lags	0.025	0.170	0.375	0.728	0.008	0.255	0.607	0.714	0.035	0.243	0.824	0.467
P value no appeal level effect lags	0.281	0.106	0.822	0.505	0.011	0.112	0.549	0.608	-0.095	-0.381	-0.070	-0.106
Typical												
Conditional interaction effect	-0.0046	-0.0345	-0.0093	0.0017	-0.1365	-1.3967	-0.3883	0.2830	-0.0270	-0.1904	-0.0535	0.0055
Conditional level effect	0.0000	0.0063	0.0021	0.0013	-0.0164	0.2211	0.1243	0.0636	-0.0017	0.0274	0.0097	0.0126
Unconditional interaction effect - pro	0.0012	-0.0084	-0.0054	0.0071	-0.1891	-1.6151	-0.5170	0.1348	-0.0340	-0.2157	-0.0675	-0.0129
Unconditional interaction effect - anti	0.0021	0.0094	0.0014	0.0020	-0.0189	-0.0784	-0.0462	-0.0532	-0.0025	-0.0091	-0.0050	-0.0066
Unconditional interaction effect - all	0.0035	0.0016	-0.0039	0.0092	-0.2094	-1.6991	-0.5665	0.0778	-0.0367	-0.2255	-0.0729	-0.0199

Notes: Regressions of employment outcomes use MORG CPS. Heteroskedasticity-robust standard errors are in parentheses. Observations are clustered at the circuit level. 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. Instruments for regulatory takings are Black judges per seat assigned to regulatory takings cases in a circuit-year. Instruments for physical takings are Democratic Nonwhite Appointees per seat and Republican Prior US Attorneys per seat assigned to physical takings cases in a circuit-year. LASSO Instruments are displayed in Appendix Table A3. Significant at +10%, *5%, **1%