GOVERNMENT EXPROPRIATION INCREASES ECONOMIC GROWTH AND RACIAL INEQUALITY: EVIDENCE FROM EMINENT DOMAIN

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Abstract: Is it justified for states to appropriate private property rights? If so, should governments expropriate or regulate? We test three conventional views: insecure property rights cause underinvestment, moral hazard cause overinvestment, or public use cause economic growth. We embed these mechanisms in a model and measure them using the random assignment of U.S. federal court judges setting geographically-local precedent. For a half-century, racial minority Democrats were more likely to strike down government appropriations while Republican former federal prosecutors were more likely to uphold them. We find that pro-government physical takings precedent stimulated subsequent takings, expropriation of larger parcels, highway construction, and growth in construction, transportation, and government sectors as well as agriculture, retail, and financial sectors, overall economic growth, and property values. However, racial minorities were increasingly displaced, unemployed, and living in public housing, and the service sector declined. Pro-government regulatory takings precedent also spurred economic growth and property values, but did not increase displacement or racial inequality.

Keywords: Property rights, displacement, regulation, takings

JEL codes: R38, K11, R52

... nor shall private property be taken for public use, without just compensation.

Bill of Rights – Fifth Amendment in the U.S. Constitution

1 Introduction

What are the long-term consequences of common law institutions on growth and inequality? Cross-country empirics attribute 30% greater stock-market-to-GDP ratios and stronger property rights to common law as opposed to civil law regimes (La Porta et al. 2008), which suggests that the common law can have important effects on economic outcomes through property law. We examine the causal impact of governments' power of expropriation of private property rights on growth and inequality and we measure the channels through which government power has its effects.

A key economic rationale for government expropriation is the failure of Coasean bargaining among numerous property rights owners: Subjective valuation exceeding objective valuation will stymie socially optimal outcomes. Under this view, government power of expropriation leads to economic growth. Yet excessive subjective valuation also occurs when there are high human costs of forced displacement. In India and China, government decisions to expropriate land have led to violent riots. 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). In the former Soviet bloc, legislation allowing governments to expropriate land for the establishment of privately-owned industrial parks is pending. And in the U.S., the *Charles River Bridge* case of 1837 represents a watershed moment. The Massachusetts government had granted exclusive property rights to private investors to build a toll bridge for traffic across the Charles River, but later eviscerated those rights by building a free bridge nearby, which touched off a dispute in which each side claimed to generate the socially optimal outcome (Lamoreaux 2011). Variously referred to as "eminent domain" (United States), "compulsory purchase" (United Kingdom, New Zealand, Ireland), "compulsory acquisition" (Australia), or simply "expropriation" (South Africa and Canada) in different legal systems, the causal impact of governmental powers of expropriation remains an open question for development economics, macroeconomics, urban economics, economic history, and constitutional law.

The Takings Clause of the Fifth Amendment in the U.S. Constitution states, "...nor shall private property be taken for public use, without just compensation." Court decisions conducting eminent domain jurisprudence focus on the definition of public use and what constitutes justifiable expropriation. The definition of public use has broadened over the twentieth century to include *public purpose* based on physical, aesthetic, and monetary benefits (*Berman v. Parker* (1954) held that eradication of blighted neighborhoods qualified as public purpose); *conceiveable* public purpose (*Hawaii Housing Authority v. Midkiff* (1984) held that a state can take land that is owned by a small group of private landowners and redistribute it to a wide group of private residents so long as the public purpose be "con-

ceivable"); and even—economic development (*Kelo v. City of New London* (2005) held that a transfer of private property to another private entity for the purpose of economic development satisfies the public use requirement). In the last fifty years, judges have decided hundreds of these cases. Major legal precedents range from the building of government dams that caused flooding, building sewers that deprived groundwater from wells, and breaking up oligopolies in land ownership. All of these actions constituted a government taking.

Alongside the birth of the regulatory state, individuals have begun challenging government *regulations* and even judicial decisions as takings, which has created novel legal debates on whether the government can even restrict how a property owner develops his or her property. Litigations have arisen over regulations that shorten the fishing year, impose zoning restrictions on hotels and gambling, and require enclosure of racing tracks. Takings Clause jurisprudence affects all regulations that potentially restrict how property owners develop their land or even—potentially diminish the economic value of property (*Pennsylvania Coal Co. v. Mahon*). Regulations that restrict property rights are litigated as government expropriation, yet the modern economy has been characterized as the rise of the regulatory state (Glaeser and Shleifer 2003). The modern administrative state entails regulatory schemes that routinely reassign property rights. Yet, the effect of this sort of state power on American government and society is unknown (Hamburger 2014). The U.S. Federal Courts litigate regulations that can move billions of dollars in stock market prices.[1] Between 1999-2014, Supreme Court decisions moved the market value of publicly traded companies by a net $140 billion (Katz et al. 2015). This paper examines the causal impact of government power of expropriation on growth and racial inequality.

Using random assignment and the fact that judges' biographies predict their decisions, we examine whether judges making different legal precedents for millions of individuals residing in one of 12 U.S. Circuits (also known as U.S. Courts of Appeals) matters. Between 1975 and 2008, over 134 Courts of Appeals cases addressing physical takings were decided in the United States, yielding roughly 107 experiments across 34 years and 12 Circuits. While this number may seem small, analysis of state laws usually examine the impact of up to 50 experiments in the U.S. or 34 in the OECD. Since Courts of Appeals are only to hear cases presenting new legal issues, cases representing big legal changes occur throughout the time period. Because judicial composition of eminent domain cases is unlikely to be correlated with subsequent economic outcomes other than through eminent domain decisions, the random assignment of judges creates exogenous variation in legal precedent that can be used to estimate the causal impact of court-made eminent domain law on growth and racial inequality.

One of the key identification challenges is that judges may make pro-takings decisions when there is urban blight, creating downward bias, or when complementary economic growth is

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[1]"Billions of Dollars at Stake in Supreme Court Power Market Fight" (Bloomberg Business, January 19, 2016).

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anticipated, creating upward bias in ordinary least squares estimates between pro-government takings decisions and subsequent economic outcomes. Data limitations have made it practically impossible to study the causal effects of eminent domain, which is rarely exercised in a random fashion. The ideal research design randomly assigns individuals or firms to different legal rules to help resolve uncertainty about consequential impacts of law (Abramowicz et al. 2011). Thankfully, individuals are not randomly assigned to different systems of justice, nor are judges randomizing their decisions, but the random assignment of law-making judges provides a close approximation.

Even if eminent domain takings were randomized, general equilibrium effects would be difficult to measure. In addition, few centralized data sources document condemnation: Many different types of property rights are subject to government takings and many levels of government (local, state, and national) have the power of eminent domain. We address both issues by focusing on national-level randomization of court-made laws (jurisprudence) that make it harder or easier for subsequent government actors to "take". The U.S. common law system–random assignment of judges, judges interpreting the facts and the law differently and in a manner correlated with their demographic characteristics, and a system of Circuit Courts with regional jurisdiction–allows us to isolate causal effects and incorporate general equilibrium responses. These general equilibrium effects can include (but are not restricted to) capital and labor migration across Circuits. To be sure, since our unit of analysis is Circuit-year, we are subject to the usual concern that the Circuit-years are, at first glance, analyzed as separate markets. However, we note that since our randomization occurs nationally and repeatedly, the general equilibrium effects of labor or capital mobility across Circuit-years are incorporated into our estimates, and these would be the estimates a judge may be interested in. The precedents themselves may eventually spill over across Circuits but it is not immediate, and citation analysis indicates that the precedential impact is an order of magnitude greater within the Circuit than outside.

Consider *Goldstein v. Pataki*, which illustrates intuitions. Here, the Second Circuit allowed New York to take land for a private developer to build a basketball arena to attract the New Jersey Nets basketball team, which was in the Third Circuit. Fifty years earlier, an attempt to build a baseball stadium on the same site failed because it was not considered public use. But the expanded definition of public use allowed the taking, which racial minorities and small business owners protested, and the last homeowner evicted from the land was compensated $3 million. Note that migration occurs both across Circuit boundaries (the basketball team is now called the Brooklyn Nets) and across time (had a baseball stadium been built, there probably would not be a basketball stadium).

We differ from previous papers analyzing the causal effects of law by proposing an estimation strategy for three separate parameters of policy interest: 1) where the counterfactual is the opposite precedent (What if *Goldstein* had been decided the opposite way? This is of

policy interest to a decision-maker determining a case already in front of him or her.), 2) where the counterfactual is no precedent (What if *Goldstein* did not exist? This is of policy interest to a decision-maker factoring in the decision to appeal.), and 3) the local effects of the decision (What if *Goldstein*'s basketball arena did not exist in the original zip codes where the alleged taking took place? This is of policy interest to a decision-maker considering a taking) using data collection from both Circuit *and* District Courts and linking the Circuit cases to the originating zip code(s).

It is important to note that the U.S. Federal Courts, and in particular, the U.S. Courts of Appeals, have strong impact on government power of expropriation. Only 2% of Courts of Appeals cases successfully appeal to the U.S. Supreme Court, so U.S. Courts of Appeals determine the vast majority of decisions that set legal precedent and 98% of their decisions are final. Their decisions are binding precedent within the Circuit, but not outside. They are persuasive precedent on the state courts within the Circuit. In all acts of eminent domain, the government must initiate court proceedings. Regulations that are litigated as expropriation fall under Circuit jurisdiction. Since the Courts of Appeals judges are revealed substantially after the briefs are filed and after litigation costs have been sunk at both the District and Circuit Court level, very few cases settle when the judges are revealed. We also note that the U.S. Courts of Appeal only handles issues of new law–so we should not expect Courts of Appeals cases to influence the decision of a subsequent case that appears in the Circuit.[2]

Guided by the only set of theoretical models in the scientific literature that we are aware of, we embed conflicting views of eminent domain in a simple model whose predictions are consistent with the previous models in the literature. We recognize that we do not have data on every modeling assumption or modeling parameter, so reduced form predictions isolate the channel through which government takings have their effects. Ex ante, the signs of the effects of eminent domain laws on subsequent economic outcomes are theoretically ambiguous. Models in law and economics emphasize how compensation for takings creates moral hazard for property owners, leading to over-investment because property owners do not pay the insurance they receive in the event of a taking that makes their investment generally worthless (Blume, Rubinfeld and Shapiro 1984; Miceli and Segerson 1994; Innes 1997). Property owners may even over-develop to deter government actors from taking their property instead of someone else's property. Models in the development literature found that if governments compensate too little, then the property owner receives less return on his or her investment, leading to under-investment (Besley 1995; Banerjee et al. 2002; Hornbeck 2010). Models in macroeconomics predict that the extractive capacity of the state (i.e., tax technology or shadow of expropriation) leads to faster economic growth because of the public benefits (Mayshar et al. 2011; Slemrod and Yitzhaki 2002; Aguiar and Amador 2011). As far

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[2]In any event, when we run the reduced form relationship between previous judicial assignment and future laws, there is little apparent effect.

as we are aware, "shadow of expropriation" was first used in the economics literature to model foreign investment (Aguiar and Amador 2011), but the concept is apt for eminent domain power since common law precedent involves both the threat and the actual expropriation.

We explore how closely our empirical design emulates a randomized control trial, addressing the problems of omitted variables, reverse causality, and identification of the multiyear response to prior years' decisions. We verify there are no effects in four years of leads before the decision, which is an important omnibus check of potential endogeneity in violation of our research design. We buttress the assumption that demographic characteristics of assigned judges in published cases are exogenous, linked to economic outcomes only through legal precedent, and that the pro-takings/pro-property owner dimension of these cases is the only channel through which eminent domain doctrine has an effect. We also assuage common concerns levied against randomized control trials (RCTs)—i) whether the LATE interpretation of IV estimates is policy relevant and ii) whether general equilibrium effects, including delayed effects, are incorporated.

We assess whether biographical characteristics not used in the first stage are correlated with residuals in the second stage regression. They are not, which suggests that either the pro-takings vs. pro-property owner dimension of these eminent domain cases is the primary channel through which eminent domain jurisprudence has an effect, or other aspects of eminent domain jurisprudence are not polarized along judicial demographic characteristics, which assuages concerns that we pick up something other than pro-takings vs. pro-property owner doctrine. It also assuages concerns that our identification strategy spuriously picks up the effects of legal precedent in another area of law. Each Circuit Court decides many thousands of cases per year, so the judicial panel composition in different legal areas is effectively uncorrelated. Our identification assumption is that the assignment of judicial biographical characteristics correlated with eminent domain precedent is uncorrelated with other social trends, legal decisions, or the assignment in other areas of law of judicial biographical characteristics that predict legal decisions and affect economic outcomes. The large number of biographical characteristics unused in the first stage are equally unlikely to be correlated with the assignment in other legal areas of judicial biographical characteristics that affect decisions in that area and affect economic outcomes.

Results are robust to controls for Circuit-specific time trends and composition of the pool of judges available to be assigned to panels; results are robust to dropping 1 Circuit at a time, varying the lag structure, and collapsing the data to the Circuit-year level; results are robust to using wild bootstrap and simulations that randomly assign legal variation to another Circuit. As Barrios, Diamond, Imbens, and Kolesar (2012) write, "if the covariate of interest is randomly assigned at the cluster level, only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals," which implies similar standard errors across the different methods of accounting for

clustering at the Circuit or Circuit-year level. We also present tests of randomization, assess additional identification concerns, and present robustness to accounting for the potential for litigants to pursue an appeal in response to prior years' outcomes. We elaborate on the use of randomly assigned judges in the lower courts (District Courts) from which an appeal arises as an additional source of exogenous variation to control for the presence of a case.

Our main findings are that rulings making it easier to take physical property rights spur economic growth and property values but increase racial inequality, as minorities become more likely to be displaced, unemployed, and live in public housing. We explore the mechanisms using data on federal transportation projects, sectoral GDP, and data entry experiments, and find evidence consistent with all three mechanisms upon which prior theories built their models: public use, over-investment, and insecure property rights.

As to what range of impact estimates are reasonable, we are guided by the only set of empirical results in the causal inference literature that we are aware of. These papers find that electrification increased female employment by 9.5 percentage points and population growth by 17% (Dinkelman 2011), railroad increased income levels by 16% (Donaldson 2015), rezoning increased employment by 12-21% and weekly wages by 8-13% (Busso et al. 2013), cell phone towers increased producer surplus by 8% and consumer surplus by 6% (Jensen 2007), a 1% increase in government purchases caused 1.5% increase in output (Nakamura and Steinsson 2011), a 1% increase in road length increased GDP by 9% and wages by 11% (Morten and Oliveira 2013), five years of legislation enabling government acquisitions increased median property values by 4% (Collins and Shester 2011), and randomly assigned roads increased property values by 16% (Gonzalez-Navarro and Quintana-Domeque 2014). Roughly 95$ billion is spent per year on place-based policies in the U.S. (Kline and Moretti 2014). It is important to note that these are a subset of all the government activities affected by Takings Clause jurisprudence. Our paper presents the advantage of being based on randomization that is naturally occurring, where general equilibrium effects are incorporated. Whether these effects seem large or small depends on one's perspective: Hacker and Pierson (2016) argue that government regulations fueled economic growth throughout the twentieth century and Gary D. Libecap (2011) find that an early form of land regulation in the 19th century–demarcation–increased property values by up to 30% and had lasting effects persisting across two centuries. The interpretation of these estimates—and ours—is subject to the usual caveats in the literature (Deaton 2010)—causal effects are sufficient, but not necessary conditions for an outcome.[3]

We begin our analysis by documenting that our cases are subsequently cited by state statutes and treatises inside–but not outside–the Circuit Court's regional jurisdiction, which

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[3]A defendant who shares the same first initial as a judge receives 8% longer sentences, but this effect only explains 0.03% variance (Chen and Prescott 2016). See also Deaton's NYU "Debates in Development" lecture on the topic, where he describes causal effects as Insufficient but Non-redundant parts of a condition which is Unnecessary but Sufficient (INUS).

verifies that Courts of Appeals have persuasive precedent on state courts where most takings cases are litigated. We next establish that the random composition of judicial panels was related to Courts of Appeals eminent domain decisions. Judges who are racial minorities have been found to vote differently from white judges on issues such as affirmative action, race harassment, unions, and search and seizure cases where racial minorities are disproportionately affected (Kastellec 2013; Scherer 2004; Chew and Kelley 2009). Racial minorities have their property disproportionately taken (Carpenter and Ross 2009; Frieden and Sagalyn 1991), and minority Democrats were 20 percentage points more supportive of property owners in eminent domain cases from 1950 to 2008 (we refer to judges appointed by Democratic presidents as Democrats for brevity). Federal prosecutors represent the government in Federal Courts, prioritizing which aspects of federal law to enforce on behalf of the government. Federal prosecutors are also known as U.S. Attorneys or U.S. District Attorneys (see references cited in Berdejo and Chen 2016; Chen 2016 on their political decision making). Republican former federal prosecutors were 18 percentage points more likely to uphold a physical taking, reflecting both business and government interests.

Next, we show that jurisprudence making it easier for governments to appropriate property rights affected government actors. Local governments expropriated larger, more expensive land parcels. Parcels were taken more frequently from businesses despite their being more litigious and expensive to displace. Highway construction increased both inside central business districts and as connections to outlying areas. On net, pro-government eminent domain precedent spurred annual economic growth and quarterly house-price growth for four years after the precedent, the largest effects occurring two years after the precedent. Effects on house prices are larger, consistent with over-investment and house prices being more forward-looking relative to contemporaneous variables like GDP. Notably, sectoral GDP effects are concentrated in areas associated with public infrastructure projects: construction, transportation, and governmental. Financial, retail, and agricultural sectors (but not the service sector) also benefited. Eminent domain precedent also had sizable local effects in the original zip code(s) where the takings case arose.

Turning to inequality, consistent with studies suggesting that individuals displaced through eminent domain relocate within a few miles, government power to expropriate increased racial minority moves within the same county relative to whites, but no such effect was found for moves outside the county. Racial minorities also became less likely to be employed relative to whites and more likely to live in public housing, consistent with concerns that eminent domain projects do not necessarily employ minorities whose homes or employers were expropriated, or that the projects themselves have unequal distributional effects, e.g., in the seminal study of irrigation dams by Duflo and Pande (2007).

Eminent domain need not be total; it could be partial, as in environmental regulations or zoning laws that limit how property owners can develop land. Regulatory takings decisions

in favor of government spur annual economic and quarterly house-price growth and benefit white-collar sectors such as services, government, and financial services. However, unlike physical takings, regulatory takings precedent do not displace individuals and, indeed, we find that they do not lead to condemnations or racial inequality in housing, employment, or displacement. They also do not affect construction and transportation sectors. The last result—in our view—is an important manipulation check of the experimental design that contrasts with the effect of physical takings precedent.

The remainder of the paper is organized as follows. Section 2 presents our model and conceptual framework, Section 3 our empirical framework, Section 4 the main results, Section 5 the mechanisms, and Section 6 concluding remarks and limitations.

2 Theory

2.1 Eminent Domain Governments intend the power of eminent domain to spur economic growth through the provision of public goods such as highways, utilities, blight removal, and commercial development. These rationales were articulated in the 2005 U.S. Supreme Court case, *Kelo v. City of New London*, which justified government transfer of land from one private owner to another on the basis that commercial development satisfied the public use requirement of the constitution.

From a theoretical perspective, hold-up problems arise when subjective valuation exceeds objective valuation, so government expropriation is intended to overcome coordination breakdowns between numerous stakeholders (Buchanan and Yoon 2000). With physical condemnation, the government must initiate court proceedings, though typically a purchase offer is made and bargaining occurs under the shadow of the law. Both residential and commercial tenants can be affected; some jurisdictions do not authorize eminent domain for residential properties (Berliner and Coalition 2006). Business owners have noted that re-establishment expenses are insufficient to cover increased rent and remodeling expenses associated with displacement (Garnett 2006). Communities have reported concerns about job loss and trouble attracting new businesses after the number of evicted businesses jumped from 39,000 in 1963 to 100,000 in 1971 (Frieden and Sagalyn 1991).

Government compensation for land acquisition has been documented to be lower than fair market value (Munch 1976; Chang 2010), which can reduce ex ante incentives to invest. Perceived under-compensation along with the fact that racial minorities have their property disproportionately taken (Carpenter and Ross 2009; Frieden and Sagalyn 1991) has raised concerns that revenue-seeking governments collude with private developers (Byrne 2005) at the expense of disadvantaged groups. This view is echoed by liberal and conservative *Kelo* dissenters arguing that taking land from the poor on behalf of a large pharmaceutical company (Pfizer) amounted to "Reverse Robin Hood."

More recently, courts have broadened the rights of individuals to challenge government regulations as a taking. When these regulations deprive the property owner of all reasonable

use or value of the property, they are deemed a regulatory taking. Examples of alleged regulatory takings include zoning restrictions for the location of hotels (*Dexter 345 Inc. v. Cuomo*, 2011) and regulations shortening the fishing year *(Vandevere v. Lloyd,* 2011).

Pennsylvania Coal Co. v. Mahon (1922) established the doctrine of regulatory takings. *Palazzolo v. Rhode Island* (2001) held that an owner does not waive his right to challenge a regulation as a taking because he purchased the property after the regulation was enacted. As a consequence, many environmental regulations and zoning laws that potentially limit how property owners can use their land are litigated as regulatory takings.

Recently the boundary between physical and regulatory takings has blurred. 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 constitute a regulation. Litigation has further blurred the test for what constitutes a physical or regulatory takings: from a "diminution-of-value" test (*Pennsylvania Coal Co. v. Mahon* (1922) held that whether a regulation constitutes a taking that requires compensation depends on the extent of the diminution of the value of the property) to an "economic impact" / "interference with investment-backed expectations" test (*Penn Central Transportation Co. v. New York City* (1978) held landmark status does not constitute a regulatory taking because it did not prevent the owner from continuing to benefit from the property) to a "permanent physical presence" test (*Loretto v. Teleprompter Manhattan CATV Corp.* (1982) held that 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) to a "total takings" test (*Lucas v. South Carolina Coastal Council* (1992) held that 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) for whether a regulation constitutes a taking that requires compensation.

Legal precedent develops through analogies. For example, *Roe v. Wade* extended the right of privacy under the 14th Amendment's Due Process Clause, which was previously interpreted as precluding government interference in *freedom of contract*[4]; *Roe v. Wade* interpreted Due Process as precluding government interference in a *woman's decision to have an abortion*. This example means that legal precedent develops through analogies at the conceptual level—the impact of cases includes the conceptual innovation, and are not restricted to future instances of identical fact patterns.

2.2 **Model** *Goldstein v. Pataki*, a case in our data, illustrates important economic mechanisms and identification concerns. Here, the Second Circuit Court set a precedent allowing New York to take land for a private developer intending to build a basketball arena that would attract the New Jersey Nets, against the wishes of politicians in New Jersey, in the Third Circuit. The project halted in 2006 when litigation began and the Circuit Court de-

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[4]See, e.g., *Allgeyer v. Louisiana* (1897).

cision occurred in 2008. The last homeowner evicted was compensated $3 million in 2010 and the sports arena was completed in 2012, with thousands of apartment units promised by 2025. Fifty years earlier, an attempt to condemn property for a baseball stadium at the same site failed because, at that time, a privately financed baseball park was not deemed appropriate public use; the baseball team later relocated and became the Los Angeles Dodgers. In 2008, racial minorities and small business owners protested the definition of "public" in the Second Circuit's interpretation of the Takings Clause, but community groups (including some minorities) supporting the project were paid at least $0.5 million by the developer. The project was adjacent to an increasingly desirable neighborhood in New York City. We know the decision created precedent in both the federal and state courts, since the case was cited as precedent by the New York Supreme Court (a state court) within two years and by the District Court in the Second Circuit in 2012, in both cases allowing a physical takings to benefit private parties.

We present a model of physical and regulatory takings that embeds prominent theories of eminent domain. The model obtains the standard result in law and economics models that any compensation that increases with investment is inefficient (e.g., Blume, Rubinfeld and Shapiro 1984; Kaplow 1986; Innes 1997): Socially optimal investment should take into account the fact that the invested capital is lost when the land is taken. In a second view, takings risk, however, leads to underinvestment, unless there is just compensation (e.g., Besley 1995; Banerjee et al. 2002; Hornbeck 2010). In the third view, takings can lead to growth when public use benefits of government activity are taken into account (Barro 1990). We show that part of the reason for conflicting views on eminent domain is that the benchmark for what is socially optimal varies across prior models.

2.3 Investment by Property Owners The risk-neutral property owner invests I in her property and expects $V(I)$ return from investment. Government compensation $C = C(I)$ is a function of investment. The law requires the government to pay the property owner, taking into account a number of factors including book value (the 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 property owner investment.

We assume that compensation increases with investment, but at a decreasing rate: $C(0) = 0$, $C_I(I) > 0$, and $C_{II}(I) < 0$. Let π be the probability that the court allows a taking to occur.

First best optimal investment occurs when marginal benefits equal marginal costs. Following the convention in the literature, "first best" refers to optimal investment when there is no

government intervention and "second best" refers to optimal investment taking government intervention as given. Moreover, government benefit and costs of takings are omitted when there is no government intervention.

(1) $\quad \max\limits_{I} V(I) - I \quad$ i.e., $\quad V'(I) = 1$

Second best optimal investment is achieved at:

(2) $\quad \max\limits_{I} (1 - \pi)V(I) - I$

   i.e.,

(3) $\quad V'(I) = \dfrac{1}{(1 - \pi)} > 1$

Since compensation is just a transfer, it does not enter the optimand. With diminishing returns, $V''(I) < 0$, the second best investment level is below the first best investment level. A taking deprives all value from the original investment, making property owners less willing to invest, but total loss of $V(I)$ from a taking is not necessary for the result.

    The property owner, however, takes compensation into account and maximizes the expected return, ER:

(4) $\quad \max\limits_{I} ER = \max\limits_{I} \{(1 - \pi)V(I) + \pi C(I)\}$

The property owner's optimal investment is achieved when:

(5) $\quad (1 - \pi)V'(I) + \pi C_I(I) = 1$

so that

(6) $\quad V'(I) = \dfrac{1 - \pi C_I(I)}{1 - \pi} < \dfrac{1}{1 - \pi}$

    Equation 6 indicates that the property owner always overinvests compared to the second best optimal investment. The only way to eliminate overinvestment is to set $C_I(I) = 0 = C(I)$, which is contrary to the doctrine of "just compensation." However, this assumes the non-zero probability of takings is fixed and uses the second best as benchmark. With exogenous variation in takings risk in our empirical framework, we use the first best as a

benchmark. From the equations above, we can see that, compared to the first best, "just compensation," $C(I) = V(I)$, results in optimal investment.

2.4 Government Action: Physical Takings

Local governments are constrained to follow court precedent. The property owner perceives the probability λ of government takings, which is allowed by the court with probability π. Then, the property owner's expected return is:

(7) $\quad \max_I ER = \max_I \{(1 - \lambda)(V(I) - I) + \lambda[(1 - \pi)V(I) + \pi C(I)]\}$

With the additional uncertainty of government takings, the property owner's optimal investment is achieved at:

(8) $\quad V'(I) - 1 - \lambda\pi V'(I) + \lambda\pi C_I(I) = 0$ so that $\quad V'(I) = \dfrac{1 - \lambda\pi C_I(I)}{1 - \lambda\pi}$

Taking the total derivative of Equation 8 yields the following expression:

(9) $\quad dI = \dfrac{V'(I) - C_I(I)}{(1 - \lambda\pi)V''(I) + \lambda\pi C_{II}(I)}(\pi d\lambda + \lambda d\pi)$

With "just compensation," $C_I(I) = 1$, it follows that $\dfrac{dI}{d\pi} = 0$.

Both over- and underinvestment relative to first best can occur depending on whether $C_I(I)$ is, respectively, bigger or smaller than 1. For example, if $C_I(I) < 1 < V'(I)$, then $\dfrac{dI}{d\pi}$ and $\dfrac{dI}{d\pi_p} < 0$ because $V''(I) < 0$ and $C_{II}(I) < 0$. Decisions making it easier for the government to appropriate, which increase actual and perceived probability of government takings and the probability that subsequent courts will rule in favor of the government, lead to underinvestment only if there is under-compensation. Overinvestment relative to first best occurs only if there is over-compensation, i.e., $C_I(I) > 1$.

2.5 Government Action: Regulatory Takings

With regulatory takings cases, there is no compensation when the court rules in favor of the government. We assume L is the loss of investment value. If plaintiffs win, they receive compensation (courts are reluctant to simply reverse a regulation—invalidation of a regulatory ordinance without payment of fair value for the use of the property during the period of the taking is considered a constitutionally insufficient remedy. *First English Evangelical Lutheran Church of Glendale v. County of Los Angeles*, 482 U.S. 304 (1987)). The compensation should be equivalent to their loss in investment value.

If governments choose between litigating or regulating (Shleifer 2010) (since to initiate a physical condemnation, the government must bring a case to court), then the investor's

decision becomes:

$$(10) \quad \max_{I}(1 - \pi_p - \pi_r)V(I) + \pi_r(V(I) - L) - I$$

where π_p and π_r are the probabilities that the court allows a physical or regulatory taking, respectively. If L is a function of investment, then investment also declines with the probability of regulations.

2.6 Public Use As we are guided by the only set of theoretical models in the scientific literature that we are aware of, and these models did not model the direct impact of eminent domain decisions, we extend the model as follows. If public services are a complementary input to private production, and production returns to private capital are more sharply diminishing without public services, then government expenditures increase growth (Barro 1990). Let $y = \Phi(k, g) = k\phi(\frac{g}{k})$ in a constant returns to scale model, where y is output, k is private capital, and g is public services. An optimal non-zero level of takings can be derived: $\frac{\delta\gamma}{\delta(\frac{g}{y})} = \frac{1}{\sigma}\phi(\frac{g}{k})(\phi' - 1)$, where γ is growth. Growth rate increases government power of expropriation if the public infrastructure is small enough such that $\phi' > 1$ and declines if public infrastructure is large enough such that $\phi' < 1$. It also reflects the degree of Coasean bargaining among numerous property rights owners and compensation policy.

Barro (1990) makes no mention of eminent domain, but observes that enhanced property rights look like reductions in marginal tax rates. This claim implicitly assumes $C(I) < V(I)$. If eminent domain reduces property rights and increases public infrastructure, then government power of expropriation has ambiguous effects. Inequality increases if those targeted by eminent domain are systematically different from those benefiting from public use projects or if benefits of public projects are unevenly distributed.

2.7 Discussion To summarize, unless public use benefits counteract investment distortions, court decisions that expand government power of expropriation should have no effect on growth or investment when there is just compensation. Government takings and displacement increase if local governments were previously constrained, but GDP, property prices, labor market and housing outcomes, and investment are all invariant to court decisions if $C(I) = V(I)$.

Many studies of eminent domain report under-compensation (Munch 1976; Chang 2010; Fennell 2004; Radin 1982). If $C(I) < V(I)$, then insecure property rights lead to underinvestment, lower property prices, and adverse labor market and housing outcomes (Besley 1995; Field 2005; Hornbeck 2010). Moreover, a precedent making it easier to take may decrease compensation by changing the relative bargaining power of future litigants, further reducing economic growth. Unequal access to courts can theoretically lead to asymmetric bargaining over "just compensation" in the shadow of the law (Mnookin and Kornhauser 1979; Steven-

son and Wolfers 2006) and empirically lead to different court judgments (Chen et al. 2013; 2015; 2014c; 2016d).

The model is intended to clarify that growth occurs only if the public use benefits are substantial, regardless of whether there is over- or under-compensation. Second, inequality arises with under-compensation. It shows that the oft-stated assertion that any compensation leads to over-investment is not generically true, not without additional behavioral assumptions such as one where property owners over-develop to deter government actors from taking their property instead of someone else's property. The unified framework links government power of expropriation to investment, actual and perceived takings risk, displacement, compensation, inequality, and growth—outcomes that we examine.

2.8 Testing the Empirical Predictions The most robust prediction of the model is that outcomes are likely to differ in Circuit-years that issue pro-government Takings jurisprudence. To test this, we will simply compare the outcomes in Circuit-years with pro-government and pro-property owner Takings jurisprudence and perform robustness checks to confirm that the difference seems to be due to Takings jurisprudence.

To link legal precedent with economic growth, we parameterize $\phi(\frac{g}{k}) = e^{\rho Law_{ct}} A_{ct}$. Consider a general dynamic growth equation for log-level of per-capita output:

(11) $y_{ct} = A_{ct} + \alpha_1 y_{ct-1} + ... + \alpha_n y_{ct-n} + \rho_0 Law_{ct} + \rho_1 Law_{ct-1} + ... + \rho_n Law_{ct-n} + \varepsilon_{ct}$

allowing output to depend on n lags of past output and adding an error term.

We assume that A_{ct} evolves according to:

(12) $\Delta A_{ct} = g_c + \gamma_0 Law_{ct} + ... + \gamma_n Law_{ct-n}$

This allows both current and lagged jurisprudence to affect the growth rate of A.

Substituting ΔA_{ct} into a first differenced version of y_{ct} yields a dynamic panel estimation equation of the following form. Dell et al. (2012) show in Monte Carlo simulations that first-differencing is very effective at correctly identifying both level effects and growth effects and that first-differencing avoids relying on cointegration assumptions for identification.

(13) $\Delta y_{ct} = g_c + \alpha_1 \Delta y_{ct-1} + ... + \alpha_n \Delta y_{ct-n} + \gamma_0 Law_{ct} + ... + \gamma_n Law_{ct-n} +$
$\rho_0 \Delta Law_{ct} + \rho_1 \Delta Law_{ct-1} + ... + \rho_n \Delta Law_{ct-n} + \Delta \varepsilon_{ct}$

The "level effects" of law on output appear through ρ. The "growth effects" of law appear

through γ. Rewriting the ΔLaw terms as Law terms yields:

(14)
$$\Delta y_{ct} = g_c + \alpha_1 \Delta y_{ct-1} + ... + \alpha_n \Delta y_{ct-n} + (\gamma_0 + \rho_0) Law_{ct} + (\gamma_1 + \rho_1 - \rho_0) Law_{ct-1} + ...$$
$$+ (\gamma_n + \rho_n - \rho_{n-1}) Law_{ct-n} - \rho_n Law_{ct-n-1} + \Delta \varepsilon_{ct}$$

Relabeling the coefficients on Law yields:

(15) $$\Delta y_{ct} = g_c + \alpha_1 \Delta y_{ct-1} + ... + \alpha_n \Delta y_{ct-n} + \sum_{j=0}^{n+1} \beta_j Law_{cj} + \Delta \varepsilon_{ct}$$

To find the growth effect, consider $\Delta y_{ct-j} = \Delta y$ and $Law_{cj} = Law$. Solving yields:

(16) $$\Delta y_c = \frac{g_c}{1 - \alpha_1 - ... - \alpha_n} + \frac{\sum_{j=0}^{n+1} \beta_j}{1 - \alpha_1 - ... - \alpha_n} Law_c$$

so that the growth effect of jurisprudence is simply $\frac{\sum_{j=0}^{n+1} \beta_j}{1 - \alpha_1 - ... - \alpha_n}$, which is identical to $\frac{\sum_{j=0}^{n} \gamma_j}{1 - \alpha_1 - ... - \alpha_n}$ since the ρ terms all cancel.

As we find variation in Law that is randomly assigned, we focus on $\alpha_j = 0$ for all j and estimate:

(17) $$g_{ct} = \theta_c + \theta_t + \sum_{n=0}^{L} \beta_{t-n} Law_{ct-n} + \Delta \varepsilon_{ct}$$

where θ_c are Circuit fixed effects, θ_t are time fixed effects, and Law_{ct} is a vector of annual jurisprudence with up to L lags included. This equation captures the growth effect of law, e.g., the effect of law on features, such as institutions that influence productivity growth.

The growth equation allows separate identification of level effects and growth effects through the examination of β_j. In particular, both effects influence the growth rate in the initial period. The difference is that the level effect eventually reverses itself. For example, a jurisprudential shock may affect government spending, but after a few periods, spending returns to normal. By contrast, the growth effect appears during the jurisprudential shock and is not reversed. A failure to innovate in one period leaves the Circuit permanently further behind. The growth effect is identified as the summation of the jurisprudential effects over time. Following the convention in the growth literature, we are interested in the distributed lag effect and test for joint significance of the lags.

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Another way to think about our research design is that laws are not likely to have an immediate impact. Laws might not be instantaneously capitalized in prices (DellaVigna and Pollet 2007). Expectations or statutory regimes may take time to adjust. In sum, the simple model of takings embeds competing views of eminent domain. Our model suggests that making it easier for the government to reassign property rights, even with compensation, typically leads to lower economic growth because of distortion in investment incentives, unless there is failure of Coasean bargaining among numerous property owners. If there are no public use benefits and there is fair market compensation, a greater risk of takings has no effect on economic outcomes. Introducing under-compensation, a greater risk of takings leads to under-investment and lower property prices, and these investment distortions would invariably lead to lower economic growth. Positive growth effects would be due to the benefits of public projects that redress failure of Coasean bargaining. Positive effects on property prices could be due to the capitalization of public projects or to over-investment.

3   Empirical Strategy

3.1   U.S. Courts of Appeals A foundational understanding of the U.S. federal courts is important to understanding our identification strategy, which relies on the law-making function of common law courts. This making of law occurs because a judge's decisions in current cases become precedent for use in future cases in the same court and in lower courts of the same jurisdiction. There are three layers of federal courts: District, Circuit, and the U.S. Supreme Court. The 94 U.S. District Courts serve as the general trial courts, where a jury is drawn to decide *issues of facts*. If a party appeals the decision, the case goes up to a Circuit Court, which decides *issues of law*; they take facts as given from District Courts and have no juries. The 12 U.S. Circuit Courts, also known as federal appellate courts, are only to hear cases presenting new legal issues (only 10-20% of District Court opinions are appealed). Cases that reach the Courts of Appeals are the more challenging and controversial cases with the greatest likelihood to set new precedent. Figure 1 displays District Court boundaries in dotted lines and Circuit Court boundaries—encompassing between 5 and 13 Districts each—in solid lines. The Federal Circuit Court does not have jurisdiction based on geography and handles mostly patent cases, so is omitted.

In deciding issues of law, Courts of Appeals provide new interpretations or distinctions of pre-existing precedents or statutes. These new distinctions expand or contract the space under which an actor is found liable (Gennaioli and Shleifer 2007). For example, in *Martino v. Santa Clara Valley Water Dist.*, 703 F.2d 1141, the Ninth Circuit held that a taking had occurred, when the local government issued an ordinance requiring that the property owner obtain permits and establish dedications for a flood control project before the property owner could develop his property. In *Moore v. Costa Mesa*, 886 F. 2d 260, which distinguished *Martino*, the Ninth Circuit subsequently held that a conditional variance that affects only a small portion of the property owner's property is not a taking.

Each Circuit Court decides many thousands of cases per year (less than 1 case per Circuit per year is related to eminent domain), but only 2% of Circuit cases successfully appeal to the U.S. Supreme Court, so Courts of Appeals determine the vast majority of decisions each year that set legal precedent. Circuit Court decisions are *binding precedent*, but 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. Circuit Court decisions are *persuasive precedent* on state courts within the Circuit. Persuasive precedent must be adopted by the state courts to become binding precedent.

Each case in the Courts of Appeals receives *three randomly assigned judges* out of a pool of judges, numbering roughly 8 to 40 depending on the size of the Circuit. These judges are appointed for life by the U.S. President and their positions and decisions are highly esteemed. Except for retirement, Courts of Appeals judges typically leave the bench only for a position in the U.S. Supreme Court. State officials are instructed to establish and annually update a set of guidelines based on federal and state law to assist state agencies in identifying and analyzing actions that may result in a taking; they have reported adjusting their acquisitions and land-use regulations to avoid exposure to costly litigation after Courts of Appeals decisions (Frost and Lindquist 2010; U.S. Department of Transportation, Federal Highway Administration 2005; Pollak 2001). Even in matters of an easement for a bicycle path, government actors refrained from the exactment because of their attention to regulatory takings precedent in the Courts of Appeals (Pollak 2001). Newspapers, advocates, and community organizers highlight change in the legal landscape after Courts of Appeals decisions (Pastor 2007; Eagle 2007; Sandefur 2005) and property owners perceive greater takings risk after major eminent domain decisions (Nadler and Diamond 2008; Nadler et al. 2008). Since judges follow precedent (Chen et al. 2014a) and markets respond to Courts of Appeals decisions (Araiza et al. 2014), we may expect to see an effect of Circuit Court decisions on property buyers, sellers, and government actors.

The courts are polarized by group identity, personal experience, and legal philosophy. A large literature documents the causal effects of biographical characteristics, experience, and legal philosophy on judicial decisions. For example, law and economics thought causes conservative jurisprudence, Courts of Appeals judges behave more partisan before Presidential elections, wins and losses of sports games affect judicial decisions, ideological perfectionism affects decisions, decisions on recent cases affect the next decision, and explanatory power of extraneous factors persist after employing the best prediction models of judges' decisions (Ash et al. 2016; Barry et al. 2016; Berdejó and Chen 2014; Chen 2016; Chen and Eagel 2016; Chen et al. 2015a; 2014; 2015b; 2016c; 2016b).

In our setting, racial minority Democrats have worked for the NAACP, a civil rights organization that advocates for racial minorities and backed the Private Property Rights

Protection Act (if the Act passes, a state or political subdivision that exercises eminent domain for economic development would be cut off from receiving federal economic grants for two years). Republican federal prosecutors have prior experience advocating on behalf of business and government interests. The assignment of these judges results in different outcomes. In some Circuits, judges are randomly assigned when cases arise, while in other Circuits, judges are randomly assigned to panels up to a year in advance and cases are randomly assigned to panels. Some judges take a reduced caseload if retired or visiting, but all are randomly assigned by a computer algorithm. From discussions with government officials at the Courts of Appeals, it appears that randomization occurs. More importantly, in Chen and Sethi (2011), they formally test for randomization by showing that case characteristics as determined by District Courts are not correlated with the characteristics of the Circuit judges assigned to the case.

3.2 Data

3.2.1 Legal Cases

We follow and use the regulatory takings database collected by Sunstein et al. (2006). We employ an identical methodology to create a physical takings database of 180 physical takings precedents from 1950-2008. First, we select major Supreme Court precedent: *Berman v. Parker*, 348 U.S. 26 (1954); *Hawaii Housing Authority v. Midkiff*, 467 U.S. 229 (1984); *Loretto v. Teleprompter Manhattan CATV Corp.*, 458 U.S. 419 (1982); *Kelo v. City of New London*, 545 U.S. 469 (2005); *Yee v. City of Escondido*, 503 U.S. 519 (1992). We include *Loretto v. Teleprompter* because it involves a physical presence of government and may have subsequently guided government officials as to what constitutes a physical taking. Second, we select all Courts of Appeals cases citing these cases. Third, we restrict to three-judge cases that discussed whether the government had physically invaded, was present on the property, or had taken the property. These cases focus on the meaning of "public use" and what constitutes a taking (damages would be determined by *finders of fact* in District Courts whereas Circuit Courts focus on *matters of law*). Fourth, we remove cases that overlap the regulatory takings database. For 1976-2008, 33 cases are removed, resulting in 134 cases in the analysis frame. Finally, we coded the Circuit decisions to the zip codes where the alleged taking took place.

Figure 2 shows a map for the location of original Courts of Appeals takings controversies in our database. The Eleventh Circuit was created in 1981 by splitting it off from the Fifth Circuit; Fifth Circuit decisions before this split are considered binding precedent in the Eleventh Circuit. We account for this in our analyses. A wide range of economic policies are affected by Takings Clause jurisprudence. Policies affected by physical takings jurisprudence include dams, sewers, beach protection, and river-diversion. Regulations are also affected because physical takings precedent influences regulatory takings precedent even if the 33 regulatory takings cases are excluded. The cases in our physical takings database include litigation around a government-built dam that flooded land, sewer construction that deprived

property owners of well water, beach protection, and government diversion of a river. A vote is coded as pro-property owner if the judge voted to grant any relief to the party alleging a violation of the Takings Clause. Appendix Table 1 provides the list and coding of cases. Figure 3 plots the quantity of eminent domain cases that were decided pro-property owner or pro-government over time and Table 1 displays summary statistics. There are 0.33 physical takings Circuit cases per Circuit-year; 27% of Circuit-years had physical takings cases.

We restrict our cases to physical takings precedent, but because of the blurring of the boundaries between physical and regulatory takings (both in terms of the government actions and the threshold tests), physical takings precedent may subsequently be cited by regulatory takings cases or by local government actors when issuing regulations. Thus, our data may be overinclusive. As placebo check, we utilize the collation by Sunstein et al. (2006) of 220 regulatory takings precedents in Courts of Appeals from 1979-2004. The cases were identified by tracking the citations of the following landmark Supreme Court decisions: *Lucas v. South Carolina Coastal Council*, 505 U.S. 1003 (1992); *Nollan v. California Coastal 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). The data includes a range of regulatory takings decisions regarding zoning restrictions on hotels and gambling, noise regulations requiring enclosure of car racing facilities, and environmental regulations shortening the fishing year. Regulations should not be expected to have the same effects as physical takings on highway construction and racial inequality.

We collected District Court cases, state court cases, and state treatises that cited our Circuit Court cases. We also collected District Court cases that cite the same Supreme Court precedents as our Circuit cases, administrative data on these cases from the Administrative Office of the U.S. Courts (AOC), and PACER filings on District Court cases. Sixteen years of Public Access to Court Electronic Records are available on open source sites for 33 Districts. We used PACER data to obtain judge identities that are missing in the AOC data. The latter two datasets, together, assess whether judicial biographies affect the publication of District cases.

3.2.2 Judicial Biographies We compiled information on judges' characteristics from the Appeals Court Attribute Data, District Court Attribute Data,[5] Federal Judicial Center, and our own data collection. The final dataset includes information on vital statistics. Variables include: geographic history, education, occupational history, governmental positions, military service, religion, race, gender, and political affiliations. Raw data on religion come from Goldman (1999).[6] Judges whose religions remained missing or unknown were coded as having no publicly known religious affiliation. As social issues divide starkly along religious lines in the U.S., it is reasonable to hypothesize that judges with different religious to may come to

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[5]http://www.cas.sc.edu/poli/juri/attributes.html
[6]Additional religion data are available at http://courseweb.stthomas.edu/gcsisk/religion.study.data/cover.htm.

different conclusions in certain cases (Chen and Lind 2007, 2014). We filled in missing data by searching transcripts of Congressional confirmation hearings and other official or news publications on Lexis.

In our data analysis, the average Circuit-year includes 17.66 judges who are available for assignment to panels. Judges occasionally visit from District Courts or specialized courts. In robustness checks, we omit visiting judges. The expected number of racial minority Democrats per seat is 0.06, and the expected number of Republican former federal prosecutors per seat is 0.04 (Table 1). We calculate the expectations based on the composition of the Circuit pool of judges available to be assigned in any Circuit-year assuming that all judges have an equal probability of assignment. Expected number of judges per seat is a proportion varying from 0 to 1. Senior judges sit less frequently and we weigh their characteristics accordingly in calculating expectations. In robustness checks, we omit senior judges and use the exact months in which judges are appointed or retire to calculate their availability.

3.2.3 Economic Outcomes

We constructed a panel of roughly 40,000 zip codes followed quarterly from 1975 to 2008 by using Fiserv Case-Shiller Weiss house-price indices at the zip code level. We merge this data at the annual level. When zip code-specific price indices were unavailable, we substituted the price index for the next geographic level (e.g., county, then division, CBSA, or state). This imputation is standard in the literature. It should not affect our results since we cluster our standard errors at a higher level. The indices are based on repeat sales data on single-family homes.

A limitation of Fiserv is that the type of land being sold may change in response to takings law, so we also collected repeat mortgage transactions handled by Fannie Mae or Freddie Mac, from the Federal Housing Finance Agency. We also used house-price indices in the 20 metropolitan areas provided by S&P Case-Shiller. An earlier draft presented replications of our findings in both datasets. We calculated zip code-specific population estimates for 2005 from the U.S. Census. As standard in the literature, we weight our analyses by population, which alleviates the concern that Fiserv may include non-residential zip codes.[7]

We obtain state-level yearly GDP from the Bureau of Economic Analysis. We aggregate across all industries by year.[8] The average local GDP growth of 5% is very close to the annualized average quarterly change in log price index of 1.2% in our data. Sectoral GDP is obtained from the same source. Since industry categorization changes in 1998, we omit 1998 when examining first-differences.

We obtain residential displacement, housing, and labor market outcomes from the Current Population Survey (CPS). We weight our analysis with CPS-provided weights.

3.2.4 Government Actions

As far as we are aware, only one dataset uniformly measures the exercise of eminent domain across jurisdictions. The Uniform Relocation Assistance and

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[7]The Census data documentation is located at: http://www.census.gov/prod/cen2000/doc/sf1.pdf.
[8]http://www.bea.gov/regional/gsp/default.cfm#download.

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.

To acquire property, local governments must offer just compensation to the owners and give the owners reasonable time to consider offers. Condemnation proceedings occur only after the parties are unable to reach an agreement through negotiations required by the statute. Around 80% of the property acquisitions are settled before the government pursues a condemnation proceeding, but our data include settlement. In a property acquisition, displaced residents are also 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.

We collected annual state-level real property acquisitions, condemnations, compensation, and displacement expenses for 1991-2009 from the Federal Highway Administration.[9] Specifically, we collected: aggregate compensation for all parcels acquired per state-year whether through open market purchase, condemnation, or administrative settlement, aggregate parcels acquired per state-year, and displacement and relocation costs.

We correlate these statistics with sectoral GDP and find that all sectors, except for the service sector, are positively correlated with condemnations. Notably, we also find that pro-government physical takings precedent has a negative impact on the service sector.

We also collected data on miles of highway construction from 1950 to 2008. The data is obtained from the Form PR-511 Database and Federal Highway Administration publications. We collected 2-digit highways (rays away from MSAs that connect central business districts with regions outside the central city), 3-digit highways (circles around MSAs that primarily serve urban areas), 2-digit federal highways (interstate between MSAs), and planned highways. This data was used in a prior study that found that highway construction caused economic growth (Baum-Snow 2007).

We do not imply that eminent domain only affects highway construction–the wide range of economic policies affected by our physical takings database includes dams, sewer construction, beach protection, and river-diversion–and the modern administrative state entails regulatory schemes that routinely reassign property rights. Rather, we used these data because it is collected in a uniform, mandatory manner across jurisdictions, and we have reason to believe the government actions being measured would impact economic growth and racial inequality (Semuels 2016).

3.3 State Response to Courts of Appeals Precedent The vast majority of takings are initiated by state and local officials and litigated in state courts. So, before proceeding, we present evidence that our takings precedents are followed by states within the Circuit but not outside. We find that state governments enact statutory amendments to comply

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

with Circuit Court decisions, and state courts write decisions in response to Circuit Court precedent.

First, citation counts indicate that our physical takings cases are cited by state statutes and treatises ten times as frequently within the Circuit than outside the Circuit (the average number of citations per state by state statutes and treatises is 0.55 inside the Circuit and 0.05 citations outside the Circuit). For regulatory takings, inside the Circuit, they receive 0.7 citations by state statutes and 1.1 citations by treatises, but outside the Circuit, 0.03 citations by state statutes and 0.3 citations by treatises.

Second, we read each case involving state or local government officials.[10] The reason for the citation reveals that state citations to cases where the state lost are statutory amendments complying with the Circuit Court precedent. In one exception, the purpose of the citation was to distinguish from the Courts of Appeals decision 15 years later, which suggests that the decision was presumed to be precedent in the meantime.

We also examined the subsequent state reaction to 15 Courts of Appeals judgments where the state lost the case ("pro-property owner" cases). These 15 cases, which come from the 48 pro-property owner Courts of Appeals decisions in the sample of 220 total regulatory takings cases, were subsequently cited in state annotated statutes inside the relevant Circuit. In 3 of the 15 cases, the state clearly reacted to the Circuit Court judgment. In the remainder, the case was included in a list of citations that reflected an annual update of relevant federal and state law.

For example, *Kruse v. Village of Chagrin Falls*, 74 F.3d 694 (6th Cir. 1996) (OH): Landowner's takings claim ripe for review under federal law because there were no other reasonable remedies available to them under Ohio's appropriation statutes after a taking occurred without notice or compensation; no statutory framework existed for inverse condemnation claim. And writ of mandamus, at best a wholly equitable and post-injury procedure, was inadequate (Ohio Rev. Code §§ 163.01-163.62).

State reaction: In *State ex rel. Hensley v. City of Columbus*, 2011 Ohio 3311 (Ohio Ct. App., Franklin County, 2011) the Ohio Court of Appeals implicitly highlighted the fact that for 15 years the presumption was the Circuit Court law was in force. It indicated that, contrary to 6th Circuit's decision in Kruse, mandamus is the appropriate means to compel proceedings to compensate for a taking of private property, pursuant to a 1994 decision of the Ohio Supreme Court (State ex rel. Levin v. Sheffield Lake, 1994 Ohio 385).

Philip Morris, Inc. v. Reilly, 312 F.3d 24 (1st. Cir. 2002) (MA): Invalidated state disclosure law requiring publication of lists of cigarette additives, since lists were trade secrets protected by takings clause. (Mass. Gen. Laws ch. 94, § 307B).

State reaction: Currently pending legislation would reinstate statute with a section indi-

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[10]We do not include cases where the federal government is litigant as those cases would go to the Federal Circuit.

cating that a tobacco company's cigarette additive list would be protected as a trade secret to the extent provided under federal law. (Mass SB 1120, apparently about to be passed; referred to senate ethics and rules committee Sept. 24, 2012.)

Dakota, Minn. & R.R. Corp. v. S.D., 362 F.3d 512, (8th Cir. 2004) (SD): Invalidated portions of statute delegating state's eminent domain power to railroads, since it granted utilities easements in the railroads' rights-of-way without charge, which constituted a taking of railroad property without just compensation. (S.D. Codified Laws § 49-16A-75.3(5)).

State reaction: In *Dakota, Minn. & E. R.R. Corp. v. Rounds*, 422 F. Supp. 2d 1073 (D.S.D. 2006), the District Court noted pending amendments to the offending statute, which would require a reasonable fee for the easements, would render the case moot; amendments came into effect in June 2006, three months after the District Court decision.

In sum, these quantitative and qualitative citations suggest that we can feel reasonably confident that state courts are predominantly following the precedent of Circuit Courts that contain them.

Moreover, federal courts are also following precedent from the Circuit containing them, but not cases from outside: citations by subsequent Circuit and District cases inside the Circuit are an order of magnitude larger than citations outside the Circuit.[11] Reading these District cases also indicate that District Court cases are following Circuit precedent.

Judicial compliance in District Courts to Courts of Appeals decisions is verified in other quantitative work: A key challenge to examining whether law has precedent using only the data on cases in the courts is to address the endogenous selection of cases into courts based on legal standard. Chen et al. (2014a) examines cases filed before the Courts of Appeals decision but resolved after the decision. Such a methodology requires relatively high frequency judicial decision-making (e.g., on piercing corporate veil cases) for all relevant cases filed in District Courts, along with random assignment of judges setting precedent.

3.4 Empirical Strategy The econometric challenge behind knowing whether common laws have impacts is at least three-fold. First, legal precedent develops through extensive cross-fertilization of legal doctrine between different areas of law via elaborate analogies. The earlier example from abortion jurisprudence highlighted how *Roe v. Wade* was analogized from *freedom of contract*.[12] With cross-fertilization, real outcomes may be misattributed to one legal rule when many legal rules are changing simultaneously. The conventional approach would be to control for other legal rules, but it is practically infeasible to code–much less select–all the possible related doctrinal areas. This example also means that legal precedent develops through analogies at the conceptual level—the impact of cases includes the conceptual innovation, and are not restricted to future instances of identical fact patterns.

Second, Besley and Case (2000) cautioned against causal interpretation of correlations

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[11]The average case receives 7 citations.

[12]See, e.g., *Allgeyer v. Louisiana* (1897).

between real outcomes and laws because constituents can influence policies. This concern is a particularly trenchant for court cases (Klarman 2005), because the legal doctrine often instructs judges to take account the community standards, i.e., norms (Chen and Yeh 2013), which feeds into the definition of public use, so it will be difficult to distinguish between laws causing real outcomes from real outcomes causing laws. Courts may also incorporate future trends in decision-making: if property prices are expected to increase, then courts may be less likely to rule that a taking meets the criteria for public use such as blight removal, which would lead to a downward bias in OLS estimates. Or it may be more likely to rule a taking meets the criteria for public use because of anticipated complementarities (e.g., the *Goldstein v. Pataki* example of building a sport stadium near a booming development), which would lead to an upwards bias in OLS estimates.

Third, judges are *consequentialist* (Chen and Schonger 2013, 2015): They take into account the potential consequences of their decisions–at least some judges on both the left (Breyer 2006) and right (Posner 1998) do. Judge Richard Posner has lamented that, "[judicial] opinions lack the empirical support that is crucial to sound constitutional adjudication" (Posner 1998); similarly Justice Breyer remarked, "I believe that a[n] interpretive approach that undervalues consequences, by undervaluing related constitutional objectives, exacts a constitutional price that is too high" (Breyer 2006). Consequentialism (in our case, maximizing economic growth or efficiency or minimizing racial inequality or unfairness) can bias the correlation between future outcomes and today's decisions if they desire similar consequences while sitting on other cases.

We overcome these three challenges with random variation in legal precedent using biographical characteristics of judges: We would not want judges to randomize their decisions in the interest of legal science, but the judges themselves are randomly assigned and their background correlates with the way they decide, effectively creating a clinical trial that randomizes pro-government *vs.* pro-property owner jurisprudence. Policy-makers and scholars have sought to evaluate legal institutions and sought estimates of the causal effects of common law decisions (Posner 1998; Breyer 2006); see also, World Bank Law and Justice Institutions Performance Evaluation and calls for randomizing law (Abramowicz et al. 2011).

Because of the randomization built into the U.S. Federal Courts, the basic empirical strategy is straightforward. The effect of Takings Clause jurisprudence is obtained by comparing the means of the outcomes of interest after pro-government precedent and pro-property owner precedent. Note that this difference is not an estimate of the comparison between pro-government precedent and no precedent. The government and economic activities can be different than what they would have been if there was no precedent whatsoever. We are mostly interested in the effect of pro-government precedent, rather than pro-property owner precedent, *when there is a precedent, as this would be the effect of the decision of a judge deliberating on the case.*

Denoting Y_{ict} as the value of the outcome of interest for individual (state, zip code, or person) i (say, GDP) and Law_{ct} as a dummy equal to 1 if the decision was pro-government, this estimate is simply:

(1) $\qquad E[Y_{ict}|Law_{ct} = 1] - E[Y_{ict}|Law_{ct} = 0] \equiv \beta_1.$

We expect $\beta_1 > 0$ if failure of Coasean bargaining makes $\phi' > 1$. Since we are interested in effects over time, we specify a distributed lag. We extend our specification to include the presence of a decision, $1[M_{ct-n} > 0]$. We focus on four years of lags and one lead ($n = -1$ to 4) and vary the lag structure for robustness. M is the number of cases, which is typically 0 or 1 (so typically $Law_{c(t-n)}$ is 1 (100% pro-government) or 0 (100% pro-property owner)). Since most of our data is yearly, we take the average law measurements in each Circuit-year, which also preserves the structure of our randomization (the moment condition for causal inference is the easiest to interpret, but previous drafts considered, estimated, and validated robustness to alternative specifications). We also considered weighting our estimates by the number of cases in a Circuit-year, where weights are the geometric mean of $M_{c(t-n)} + 1$ over the distributed lag. The statistical significance of the results increases, so we present more conservative estimates without weighting.

(2) $\qquad g_{ct} = \theta_c + \theta_t + \sum_{n=0}^{L} \beta_{1t-n} Law_{ct-n} + \sum_{n=0}^{L} \beta_{2t-n} 1[M_{ct-n} > 0] + \varepsilon_{ct}$

Analogizing to coin flips, β_1 captures the effect of the heads-or-tails coin flip (pro-government vs. pro-property owner precedent), $\beta_1 + \beta_2$ captures the effect of the heads coin flip and the presence of the coin (pro-government precedent vs. no decision), and β_2 captures the effect of the tails coin flip and the presence of the coin (pro-property owner precedent vs. no decision).

We examine several outcomes: change in log yearly state GDP, change in log quarterly zip code property prices, change in log miles of federal highways, condemnation statistics, whether an individual moved in the last year, and employment status. We will test whether there are differences in outcomes of racial minorities by running the regression:

(3) $\qquad Y_{ict} = \theta_c + \theta_t + \sum_{n=0}^{L} \beta_{3t-n} Law_{ct-n} + \sum_{n=0}^{L} \beta_{4t-n} 1[M_{ct-n} > 0] +$
$\qquad \qquad \sum_{n=0}^{L} \beta_{5t-n} Law_{ct-n} * B_{ict} + \sum_{n=0}^{L} \beta_{6t-n} 1[M_{ct-n} > 0] * B_{ict} + \eta X_{ict} + \varepsilon_{ict}$

B_{ict} represents a dummy indicator for being non-white. We expect $\beta_5 < 0$ if minorities are disproportionately targeted by eminent domain and there is under-compensation. We report the average and the individual coefficients. We consider $n = 0$ as a lag because some statistics

refer to calendar year. Most of the effects appear with some slight delay so excluding $n = 0$ in joint significance tests does not affect our results.

In what follows, we report a mix of robustness checks along with the main results so the reader can make their own judgment as to the most central findings. We are guided by an ordering of presentation that verifies whether the "randomized control trial" works, after which we would present additional outcomes and mechanisms using only the baseline model (earlier drafts had many additional tables).

In principle, we have up to 1,632 experiments (34 years x 12 Circuits (x 4 quarters)). With random treatment assignment, adding controls can add precision to the estimates if the controls are strong predictors of the outcomes. We show (in Tables 4 and 5 and also in the Appendix) that our main estimates are robust to the inclusion or exclusion of:

- Person-level age, gender, and educational attainment, and state-level fixed effects. These enter as dummies with the exception of age.
- Characteristics of judges available to be assigned. We calculate the expectations based on the composition of the Circuit pool of judges available to be assigned in any Circuit-year assuming that all judges have an equal probability of assignment. Expected number of judges per seat is a proportion varying from 0 to 1. Senior judges sit less frequently and we weigh their characteristics accordingly in calculating expectations. The results are not sensitive to omitting senior judges and using the exact months in which judges are appointed or retire to calculate their availability.
- Circuit-specific time trends to allow different Circuits to be on different trajectories with respect to outcomes.

We also present a specification with four years of leads and one lag ($n = -4$ to 1).

For standard errors, as Barrios, Diamond, Imbens, and Kolesar (2012) write, "if the covariate of interest is randomly assigned at the cluster level, only accounting for non-zero covariances at the cluster level, and ignoring correlations between clusters, leads to valid standard errors and confidence intervals," so we expect to see similar results whether clustering standard errors at the Circuit or Circuit-year level (Barrios et al. (2012) show that random assignment of treatment addresses serial and spatial correlation across treatment units). Our results are also unaffected using the standard approach with U.S. data, 50 state clusters (see Tables 4 and 5). Appendix Tables 4 and 5 also report regressions where each datapoint corresponds to 1 experimental subject (34 years x 12 Circuits). We check results using randomization inference that assigns the legal variation to another Circuit and the robustness of our results to using wild bootstrap. In addition, the leads specification serves as an omnibus check for insufficient adjustment of standard errors. A prior draft clusters standard errors at the Circuit-year level and this draft clusters at the Circuit level, and the

results are unchanged.[13]

3.4.1 Instrumental Variable Ascertaining a causal effect from judicial decisions to social trends is difficult without idiosyncratic variation in judicial decisions. Since Law_{ct} and ϵ_{ict} may be correlated due to uncontrolled-for social trends or other legal developments that correlate both with Law_{ct} and outcomes Y_{ict}, we develop an instrumental variable for Law_{ct} using judges' biographical characteristics. We use biographical characteristics because the number of eminent domain cases yields sharp demographic effects but not judge-specific effects, as there are too few cases with which to identify judge-specific effects (there are roughly 180 life-time appointed judges and only 134 cases). In this paper, we do not analyze heterogeneity in terms of, e.g., early or late pro-takings decisions, or the compounding effects of, e.g., pro-takings decisions, and we only analyze the average effects.

Judges with different conceptions of duty or empathy may render different decisions. In particular, racial minority Democrats are more likely than racial minority Republicans to have worked for the NAACP, a civil rights organization that advocates for racial minorities and backed the Private Property Rights Protection Act. If the Act passes, a state or political subdivision that exercises eminent domain for economic development would be cut off from receiving federal economic grants for two years. Federal prosecutors have been found to be politically and legally partisan, so we may expect Republican former federal prosecutors to be primed to support business interests and favor government takings.

We develop an instrumental variable using judges' shared biographical characteristics and we will refer to the number of judges assigned to eminent domain panels with these characteristics as N_{ct} (we drop the subscript n to ease the exposition on the instrumental variables construction).

If a Circuit-year has a higher fraction of pro-government judges (N_{ct}/M_{ct}) assigned, the precedent for that year will be that much more pro-government. Were we only interested in the contemporaneous effect of Law_{ct}, the moment condition for causal inference is $\mathbf{E}[(N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct}))\varepsilon_{ict}] = 0$. $\mathbf{E}(N_{ct}/M_{ct})$ is the expected proportion of judges who tend to be pro-property owner in takings cases. Figure 3 illustrates the identification strategy. The jagged line displays N_{ct}/M_{ct} and the smooth line displays $\mathbf{E}(N_{ct}/M_{ct})$ in each of the 12 Circuits. The smooth lines indicate the underlying variation in judge-specific characteristics within Circuits over time. The jagged line indicates the random year-to-year variation in racial minority Democrats per seat. We estimate how outcomes $\triangle Y_{ict}$ respond to idiosyncratic variation in excess proportion, $N_{ct}/M_{ct} - \mathbf{E}(N_{ct}/M_{ct})$.

Next, we construct an instrumental variable whose moment conditions are implied by the original moment condition. Consider an instrument, $p_{ct} - \mathbf{E}(p_{ct})$, where p_{ct} is the proportion of judges who tend to be pro-property owner in takings cases and p_{ct} is defined as 0 when

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[13]We thank our NBER discussant Bentley MacLeod for recommending for a related paper that we cluster at the Circuit level.

there are no cases:

$$(4) \qquad p_{ct} = \begin{cases} N_{ct}/M_{ct} & \text{if } \mathbf{1}[\text{M}_{\text{ct}} > 0] = 1 \\ 0 & \text{if } \mathbf{1}[\text{M}_{\text{ct}} > 0] = 0 \end{cases}$$

Without loss of generality, N_{ct} is the number of racial minority Democrats per seat in takings cases in Circuit c and year t. These two conditional moment conditions imply $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = 0$ unconditionally: $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{Pr}[M_{ct} > 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} > 0] + \mathbf{Pr}[M_{ct} = 0]\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}|M_{ct} = 0] = 0$. Furthermore, $\mathbf{E}[(p_{ct} - \mathbf{E}(p_{ct}))\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}[\mathbf{E}(p_{ct})\varepsilon_{ict}] = \mathbf{E}(p_{ct}\varepsilon_{ict}) - \mathbf{E}(p_{ct})\mathbf{E}(\varepsilon_{ict}) = \mathbf{E}[p_{ct}\varepsilon_{ict}]$. Thus, p_{ct} and $p_{ct} - \mathbf{E}(p_{ct})$ both serve as valid instruments. Notably, as Tables 4 and 5 and the Appendix show, the results are unaffected by controlling for $\mathbf{E}(p_{ct})$. As Table 2 shows, $\mathbf{E}(p_{ct})$ is uncorrelated with Law_{ct}. These checks assuage concerns that $\mathbf{E}(p_{ct})$ is correlated with outcomes (or correlated with decisions in other areas of law that affect the outcomes we examine) in a manner that drives our results.

This draft presents estimates using the following identification assumption (moment condition) for causal interpretation: $\mathbf{E}[\frac{N_{ct}}{M_{ct}}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0]] = 0$. Early drafts obtained similar results using $\mathbf{E}[N_{ct}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0], M_{ct}] = 0$, which looks at the number of pro-takings decisions controlling for the number of decisions, and $\mathbf{E}[N_{ct}\varepsilon_{ict}|\mathbf{E}(\frac{N_{ct}}{M_{ct}}), \mathbf{1}[M_{ct} > 0], Q_{ct}] = 0$, which controls for the size of the court docket and checks if pro-takings vs. pro-property owner decisions had opposite-signed effects. For the reasons we outline next, checking for opposite-signed effects is a poor test, and this motivates our current specification.

All lags and leads of Law_{ct} are instrumented for in the actual implementation. As standard, we also lag and interact the instruments as we lag and interact Law_{ct}.

3.4.2 Interpretation of $\beta_{1n}Law_{c(t-n)} + \beta_{2n}\mathbf{1}[M_{c(t-n)} > 0]$: Dummying for the presence of a case permits identification of additional counterfactuals.

- β_{1n} captures the effect of pro-government precedent where the counterfactual is a pro-property owner precedent.
- $\beta_{1n} + \beta_{2n}$ captures the effect of pro-government precedent where the counterfactual is no precedent.
- β_{2n} captures the effect of pro-property owner precedent where the counterfactual is no precedent.

3.4.3 Interpretation of 2SLS: In common law, hard cases are the compliers whose decisions turn on the identity of the judge and they lack strong legal precedent. Hard cases precede easy cases, which are the always/never-takers. Easy cases follow the legal precedent established previously. Cases that reach the Courts of Appeals are the more challenging and legally innovative cases. According to Courts of Appeals judges, 5–15% of cases are "legally

indeterminate" and have no strong legal precedent (Edwards and Livermore 2008). These are the cases where judges seek guidance, and at present, from policy arguments that speculate about the consequences of a legal rule (Posner 1998; Breyer 2006; Abramowicz et al. 2011). Ambiguity has been shown theoretically to cause polarization along partisan lines (Baliga et al. 2013). Since judges follow precedent, the complier cases precede the always-taker/never-taker cases.

At first glance, our 2SLS estimates capture only the effects of hard cases, and as typical in the instrumental variables literature, the external validity is limited to the sets of cases likely to respond to the instrument. On a second look, β_{1n} captures the effect of hard cases n years ago, which can be decomposed into delayed direct effects *as well as* the effects of subsequent easy cases that cite these hard cases. That is, $\sum_{n=0}^{\infty} \beta_{1n} = \sum_{n=0}^{\infty} TOT_{ct}^{n} = \sum_{n=0}^{\infty} LATE_{ct}^{n}$ even though in general, we only know $LATE$ and not TOT. Recall, $TOT \equiv E[Y_{1i} - Y_{0i}|R_i = 1] = E[Y_{1i} - Y_{0i}|R_{1i} > R_{0i}]Pr(R_{1i} > R_{0i}|R_i = 1) + E[Y_{1i} - Y_{0i}|R_i = 1]Pr(R_{1i} = R_{0i} = 1|R_i = 1)$, where R_i indicates whether i received treatment, $R_{1i} > R_{0i}$ indicates whether individual i is a complier and $R_{1i} = R_{0i} = 1$ denotes an always-taker, under the assumption of no defiers.

Indirect inference suggests that the effect of hard eminent domain cases is largely through cases subsequently not litigated or not published in Courts of Appeals: few cases occur per Circuit-year and contemporaneous judicial composition is not correlated with subsequent takings decisions in the Circuit. The absence of subsequent easy cases following prior hard cases in the published record is not surprising since Courts of Appeals cases should bring issues of new law. Litigants should settle the easy cases, even if they do not, judges should leave easy cases unpublished. Since published cases are predominantly hard cases, they are responding to biographical characteristics. In addition, the strong first stage suggests that any bias that results from the presence of non-compliers is likely to be small. The bias from non-monotonicity is given by $\frac{Pr[Defier]}{Pr[Complier]-Pr[Defier]}(\beta^{Complier} - \beta^{Defier})$, which is likely to be small even if there are non-compliers, since the magnitude of the first stage is large (the denominator grows and the numerator shrinks) using biographical characteristics as the instrument.

More concretely, if a Circuit-year has a higher fraction of Republican former federal prosecutors assigned, the precedent that year will be that much more pro-government. We are interested in the subsequent effects of that precedent on government and economic outcomes. We are able to do so because the identity of a judge on a case does not directly affect outcomes except through the precedent. The court decision is taken as precedent by subsequent courts. Also, judge identity do not predict stock prices at the time of resolution controlling for the manner in which the case was resolved and judge identity do not predict stock prices at the moment that judges are revealed (Badawi and Chen, 2014).

It is also worth noting that for Takings Clause jurisprudence, pro-government vs. pro-property owner is the materially relevant legal doctrine for economic outcomes. A very in-

teresting feature of the institutional setting, however, is that it is possible to assess this hypothesis in conjunction with another. If there are other aspects of eminent domain doctrine that are sensitive to judges' biographical characteristics, and if these other aspects of eminent domain doctrine affect government and economic outcomes, we should observe correlations between Circuit-year biographical characteristics not used in the first stage and 2SLS residuals. They are not, which suggests that the pro-government vs. pro-property owner dimension of these cases is the primary channel through which eminent domain doctrine has an effect or that other aspects of eminent domain doctrine are not polarized along judicial demographic characteristics. An early draft of Chen and Sethi (2011) compared the effects of sexual harassment law with gender discrimination law (since these areas of law are closely related) in a specification that included both areas of law and instruments for each. The data on these laws was collected by Sunstein et al. (2006). When each area of law was instrumented for by its assignment of judges, the addition of another area of law did not affect the coefficient on the other area of law, which assuage concerns of leakage.

3.4.4   Interpretation of Experiment vs. Population: The population TOT of the Circuit = (Experimental: TOT_{direct}) * P(exposure$_{\text{direct}}$) + (TOT_{indirect} of individuals) * P(exposure$_{\text{indirect}}$). The experiments estimate TOT_{direct} for individuals. The known parameters are TOT_{Circuit} and TOT_{direct}. The unknown parameters are TOT_{indirect} and the probabilities. If one's priors are that P(exposure$_{\text{direct}}$) is small, then indirect exposure may be large. For example, thought leaders may issue cues (Baum and Groeling 2009; Cohen 2003; Bullock 2011; Clark et al. 2014) that shape perceptions after decisions (Dolbeare and Hammond 1968). Data limitations make it practically impossible to study all the channels through which law has its effects, but Clark et al. (2014) finds significant use of Twitter after several court decisions. We will present the results of an experiment where subjects are randomly made aware of a recent Courts of Appeals decision.

3.4.5   Randomization Check According to interviews, each court implements randomization differently. In some Circuits, two to three weeks before the oral argument, a computer program randomly assigns available judges to panels who will hear cases. In other Circuits, judges are randomly assigned to panels up to a year in advance; cases that arise are randomly assigned to panels. Some judges take a reduced caseload if retired or visiting, but all are randomly assigned by a computer algorithm. Senior judges can opt out of death penalty cases in some Circuits, but they would do so before random assignment. Chen and Sethi (2011) formally tests for randomization by showing that case characteristics as determined by District Courts are not correlated with the characteristics of the Courts of Appeals judges assigned to the case.

Even if judges are randomly assigned, because our data comprise published opinions, several additional issues need to be considered: settlement, publication, and strategic use of

keywords or citation. In Courts of Appeals, judges are revealed very late, after litigants file their briefs, sometimes only a few days before the hearing, if there is a hearing, which gives little opportunity and incentive for settlement upon learning the identity of the panel. Most of the litigation costs are sunk by that point, and when the D.C. Circuit began announcing judges earlier, it did not affect settlement rates (Jordan 2007). Unpublished cases are not supposed to have precedential value. Unpublished cases are deemed as routine and easy: studies find that judicial ideology predicts neither the decision in unpublished cases (Keele et al. 2009) nor the decision to publish (Merritt and Brudney 2001). To rule out strategic use of keywords or citation of Supreme Court precedent, we propose an omnibus test to collectively address deviations from strict exogeneity: we examine how similar the string of actual panel assignments is to a random string, the details of which are developed in Chen (2013). To see random strings as an omnibus test: Suppose racial minority Democrats publish cases and other judges do not. In order for this to explain any effects, we should expect racial minority Democrats to violate the random strings test.

We assess deviations from random assignment by examining whether the sequence of proportions of judges is similar to a random process. Figure 3 suggests visually that panel composition is not serially correlated. Formally, we:

1. Propose a statistic that can be computed from the sequence of numbers of racial minority Democrats 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 racial minority Democrats 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 jth case depends on the outcome in the j-1thcase. This statistic can detect whether judicial assignments are "clustered," meaning a higher than expected number of back-to-back 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 nth 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.

Number of Runs: Instead of simulating 1000 random strings, we compute the exact statistic for number of runs. This test captures violations of randomization at the case level rather than Circuit-year. In power calculations, this test has less Type II error compared to the other tests. These random strings tests also have lower Type II error compared to regression.

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

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

Random strings test complements standard randomization checks (e.g., examinations of (1) leads and (2) correlations between judicial composition and pre-determined case characteristics). If pre-determined covariates occur randomly over time, checks of (2) miss non-random serial correlation in judicial composition while the random strings test would miss correlations between judicial composition and pre-determined covariates.

We also stack the strings across Circuits and across biographical characteristics and run an autocorrelation test and compare the F statistic with F statistics generated from randomly assigning available judges to cases. The results are consistent with randomization.

Other variations from random assignment include: remanded cases from the Supreme Court are returned to the original panel; en banc cases that are heard by the entire pool of judges (or a significant fraction in the Ninth Circuit); judges with conflict of interests opt out after random assignment, which is extremely rare. We do not use remanded or en banc cases, which are also relatively infrequent. Judges can also take sick leave or go on vacation, but this is determined far in advance. Not accounting for vacation, sick leave, senior status, en banc, remand, and recusal can lead to the inference that judges are not randomly assigned (Hall 2010; Chilton and Levy 2015).

Our identification strategy—like the identification strategy of papers that use the patent officer assignment or disability application reviewer assignment, which are not explicitly random (Maestas et al. 2013; Galasso et al. 2015)—assumes that idiosyncratic deviations from random assignment are ignorable. Even a gold-standard random process — the roll of a die — has a deterministic element. If known with precision, the force and torque applied

to the die, the subtle air currents, the hardness of the surface, etc., might allow us (or a physicist) to determine with certainty the outcome of these "random" rolls. Despite this obvious non-randomness, we would still have faith in the outcome of a trial with treatment assignments based on die rolls because we are certain that the factors affecting the assignment have no impact on the outcome of interest and hence are ignorable.

3.4.6   District Courts Litigants' decision to appeal may respond to previous years' legal decisions, however, so controlling for $\mathbf{1}[M_{ct} > 0]$ may bias the coefficient for Law_{ct}; the bias is more severe for more distant lags and non-existent for the most advanced lead. We assess whether this potential endogeneity is a significant concern by comparing $\beta_{1(t-n)}$ when we instrument for $\mathbf{1}[M_{ct} > 0]$ using the random assignment of District Court judges. District judge demographic characteristics are correlated with reversal rates in the Courts of Appeals (Haire, Songer and Lindquist 2003; Sen 2015; Barondes 2010; Steinbuch 2009); and expected reversal rates could encourage litigants to pursue an appeal. If $\mathbf{1}[M_{ct} > 0]$ and Law_{ct} are both identified, estimates should be roughly invariant to the inclusion or exclusion of additional lags and leads (including lags that are important predictors of the outcome improves statistical precision, but losing data at the beginning and end period reduces precision) and lead coefficients being 0 provide an omnibus check of our instrumental variable being endogenous to pre-existing trends.

District Courts assign one judge to a case randomly or rotationally (Taha 2009; Bird 1975). Cases being returned on remand from the Courts of Appeals are not randomly assigned. We do not use remanded cases in our dataset. For example, one District told us that random assignment occurs within 24 hours of a case filing, which is handled in the order of its arrival. Waldfogel (1995) reports that one District Court uses three separate randomization wheels and each wheel corresponds to the anticipated case length. Related cases (meaning that one decision will substantially resolve all cases), if filed within a few weeks, may be consolidated. Waldfogel (1995) reports that plaintiffs can argue the case is related to another pending case and, if the judge agrees, the cases will be consolidated. A clerk reported 8% of filed cases were accepted as related in 1991 in SDNY. In another District Court, if a clerk identifies and two judges agree that a new civil case is related to another open civil case, they will be consolidated in the interests of justice or judicial economy. The clerk brings the possible connection to the attention of the judge of the new case, who then confers with the judge of the earlier case to determine whether they are in fact related cases. Consolidation would only occur for relatively high-frequency case types, which does not include eminent domain. For the handful of District cases that do overlap such that they are consolidated, we assume the decisions about case relatedness occur in a manner exogenous to judge assignment.

To instrument for $\mathbf{1}[M_{ct} > 0]$, we define our District IV in two ways. Both definitions rely on the random assignment of District Court judges and the fact that some are more likely to be appealed than others. We use the first definition for data availability reasons, and

we present the second definition if data were to become available in the future. In the first definition, $w_{ct} = \frac{\sum_{d=1}^{J} K_{cdt} * \left(\frac{L_{cdt}}{K_{cdt}} \right)}{\sum_{d=1}^{J} K_{cdt}}$, where K_{cdt} denotes the number of cases filed in District court d within Circuit c at time t (J goes from 5 to 13 depending on the District). L_{cdt} denotes the number of judges with a particular characteristic assigned to cases. The intuition is that assigning District judges who are disproportionately appealed leads to an appeal in the Circuit, $\mathbf{1}[M_{ct} > 0]$. Note that this assumes $K_{cdt} > 0$. An approximation is to define $K_{cdt} * \left(\frac{L_{cdt}}{K_{cdt}} \right)$ as 0 if $K_{cdt} = 0$. Then, the instrument can be constructed if $\sum_{d=1}^{J} K_{cdt} > 0$, which holds for both physical and regulatory takings.

In the second definition, $\tilde{w}_{ct} = \sum_{\tilde{d}=1}^{\tilde{J}} K_{c\tilde{d}t} * \left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right)$, where $\tilde{d}$ denotes District courthouse or court.[14] A District Court has several courthouses (also referred to as Divisions) and in some District Courts, random assignment is at the courthouse level. The second definition does not rely on the exogeneity of the location of takings controversies, $K_{c\tilde{d}t}$. It also does not rely on $K_{c\tilde{d}t} > 0$, though it still relies on $\sum_{\tilde{d}=1}^{\tilde{J}} K_{c\tilde{d}t} > 0$. It is not possible to merge courthouse location for most of the District Court cases (we tried to link the courthouse information from the Administrative Office of the U.S. Courts database via the docket number). This results in $\tilde{w}_{ct}$ being undefined in over 50% of Circuit-years and thus requiring another dummying out for missing strategy. When we use $\tilde{w}_{ct}$, we include a dummy for missing values in $\tilde{w}_{ct}$ and define $\tilde{w}_{ct}$ to be 0 when it would otherwise be missing. In our main tables, we use w_{ct}, but in the Appendix we also report estimates that use $\tilde{w}_{ct}$ for robustness. The estimates are unaffected, which assuage concerns that the litigants' decision to appeal may respond to previous years' legal decisions.

The ideal construction of $\tilde{w}_{ct}$ takes a weighted sum across wheels of deviation from expectations, $E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right)$, separately for senior and non-senior judges. Senior judges can elect not to be assigned to certain wheels. Another District Court uses, instead of wheels, thirteen computer generated decks of cards–one deck for each case category and an identical number of cards (two or five) for each active judge.[15] The decks refill when the majority of the deck has been exhausted. Senior judges can request to be assigned to certain decks. Even within a deck, senior judges can ex ante request a "bye" for specialized case types. Within each Dis-

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[14]

Fact The Law of Iterated Expectations (LIE) implies $E\left(K_{c\tilde{d}t} * \left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \right) = 0$.

Proof Using LIE, $E\left(K_{c\tilde{d}t} * \left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \right) = E\left(E\left[K_{c\tilde{d}t} * \left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \middle| K_{c\tilde{d}t} \right] \right)$. Rearranging results in: $E\left(K_{c\tilde{d}t} E\left[\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \middle| K_{c\tilde{d}t} \right] \right)$. Again by LIE: $E\left[\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \middle| K_{c\tilde{d}t} \right] = E\left[E\left(\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) * \epsilon_{ct} \middle| \epsilon_{ct}, K_{c\tilde{d}t} \right) \middle| K_{c\tilde{d}t} \right]$. Rearranging once again: $E\left[\epsilon_{ct} E\left(\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) \middle| \epsilon_{ct}, K_{c\tilde{d}t} \right) \middle| K_{c\tilde{d}t} \right]$. The expression $\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right)$ is the deviation of the ratio of judge assignment characteristics from the mean, so it should be independent of ϵ_{ct} and $K_{c\tilde{d}t}$. Therefore, $E\left(\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} - E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}} \right) \right) \middle| \epsilon_{ct}, K_{c\tilde{d}t} \right) = 0$.

[15]http://www.mnd.uscourts.gov/cmecf/Order-for-Assignment-of-Cases.pdf

trict Court are several courthouses (also referred to as Divisions). The appropriate Division is determined by where the parties are located and where the cause of action arose. Some Divisions get their own deck of cards. Taha (2009) reports that in 29 Districts, a case may be assigned to any judge in that District, while in the others, the cases are assigned to a geographic Division within the District and randomly assigned to one of the judges in that Division.

However, since $E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}}\right)$ is uncomputable for senior judges since we would need to know the senior "byes" in every District courthouse, we drop senior District judges for calculating $\tilde{w}_{ct}$; we also drop visiting (judges routinely visit other courts to assist with caseload) and magistrate judges (they assist District Court judges but do not have life tenure and we do not have their biographical data) for similar reasons, collectively resulting in less than 10% sample loss. Non-ideological cases are referred to magistrate judges (Nash 2015), so omitting them will not matter. Identification is unaffected by dropping judges even if they are in the same wheel. Some courts spin separate random wheels for District judges and for magistrate judges. In some Districts, parties can decline assignment to a magistrate judge within a certain time period and request another random draw. This will not affect identification because it happens before the random assignment that we use. In some Districts, when the federal government is a litigant on the case, the U.S. attorney can pick the wheel. As stated earlier, we do not include cases where the federal government is litigant as those cases would go to the Federal Circuit.

Thus, conditional on case type, there is random assignment at the court or courthouse level, and we must only calculate the yearly expected composition of judges in District courthouses, $E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}}\right)$, and we drop judges whose $E\left(\frac{L_{c\tilde{d}t}}{K_{c\tilde{d}t}}\right)$ is unknowable in the robustness check that uses $\tilde{w}_{ct}$.

Unlike for Courts of Appeals cases, we cannot use the random strings test as an omnibus assessment for violations of random assignment, because some Districts use rotational assignment or random drawing of judges from card decks without replacement. So we discuss the concerns qualitatively and suggest another empirical test. First, District Courts judges are revealed much earlier than Courts of Appeals judges. Ideally, we would use docket filings in the Administrative Office of the U.S. Courts pertaining to physical and regulatory takings, but judges are omitted for most cases prior to 2000, so we must use published District opinions to construct our District IV. So, we buttress the assumption that settlement, publication, and strategic use of keywords or citations are exogenous: 1) in District Courts, judges are much more constrained and ideology has been found to play hardly any role. Judicial ideology does not predict settlement rates (Ashenfelter et al. 1995; Nielsen et al. 2010), settlement fees (Fitzpatrick 2010), publication choice (Taha 2004), or decisions in published or unpublished cases (Keele et al. 2009)—this last fact is consistent with the District judge identity only affecting outcomes through the presence of an appeal but not through the Dis-

trict Court decision, but this exclusion restriction is not necessary for the empirical analysis; 2) we examine these issues directly as follows.

Since the random strings test is ineffective for District Courts, we test whether District Court judicial biographical characteristics in *filed* cases jointly predict publication. We link PACER filing data, which has judge identity, to AOC data, which has information on publication. We obtained all freely available PACER (Public Access to Court Electronic Records) data on District cases from 32 districts for 1980 to 2008 for a total of 359,595 non-duplicated cases. This data contains the name of the District where the case was filed, the filing and termination date (missing for 10% of cases), the assigned docket number, and the name of the District or magistrate judge presiding on the case. We merge the names of the judges into the Administrative Office of the U.S. Courts (AOC) database. We use LASSO to select biographical characteristics and no characteristic was chosen. We assume that remaining deviations from random assignment, like vacation days, are ignorable.

In sum, what is the difference between pro-government vs. *pro-property owner* precedent and the difference between pro-government vs. *no* precedent? The U.S. Federal Courts effectively randomize both the coin flip (pro-government vs. pro-property owner; what if *Goldstein v. Pataki* had been decided in favor of the property owners?) and the presence of the coin itself (pro-government vs. no decision vs. pro-property owner; what if *Goldstein v. Pataki* did not exist?). They do so because the District Court judges are also randomly assigned, which identifies an exogenous component of the presence of an appeal. However, our paper is primarily interested in the coin flip itself rather than the presence of the coin, which is addressed and analyzed in far more detail in Chen et al. (2014b), so we devote little space to discussing the latter in the results that follow.

3.4.7 Many Instruments A large number of biographical characteristics serve as valid instruments, which would result in a weak instruments problem if we used them all. Roughly thirty characteristics that enter in levels (Democrat, male, racial 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), judge-level interactions (e.g., racial minority Democrats), and panel-level interactions (e.g., fraction of judge seats assigned to Democrats multiplied by fraction of judge seats assigned to racial minorities) yielding a total of several thousand possible instruments.

There are two ways to reduce dimensionality: a priori theory and model selection. LASSO (least absolute shrinkage and selection operator) is commonly used for model selection (Bel-

loni et al. 2012). LASSO has sparseness and continuity, which OLS lacks. With OLS, large subsets of covariates are deemed important, resulting in too many instruments, which makes 2SLS susceptible to a weak instruments problem. Small changes in the data result in different subsets of covariates deemed important. Formally, LASSO modifies OLS by adding a data penalty for having too many large coefficients. The model minimizes the sum of squares subject to the sum of the absolute value of the coefficients being less than a constant, which tends to set some coefficients to exactly 0 and hence reduces model complexity.

The larger the set of IV candidates, the more severe the LASSO penalty. Reducing the number of biographical characteristics by removing candidates that are over 99% correlated with one another increases the possibility of selecting candidates. Candidate instruments must have scores that satisfy a central limit theorem to reduce the chance that LASSO picks up spurious signals. Dropping candidate instruments that deviate from its modal value[16] with less than 5% frequency yields another threshold choice. We compare results with and without LASSO model selection in Appendix Tables 4 to 8. We also report results from LIML (limited information maximum likelihood), which is weak-instruments robust and uses all the instruments.

In Appendix Table 8, we report the use of the Dantzig selector that accounts for correlation in the IV candidates and thus results in a less severe penalty. We also employ the following sensitivity check. With many endogenous variables and many instruments, there is a danger of overfitting with instruments from the wrong year. In robustness checks, we use the contemporaneous instruments to predict $Law_{c(t)}$ and $\mathbf{1}[M_{ct} > 0]$ and use the fitted values as instrumental variables in 2SLS.[17]

Chen et al. (2014b) develops another solution. It presents a visual Hausman test, which can also be analogized to partial identification or set identification: the 2SLS results are visually presented for a variety of instruments that have a strong first stage. It also proposes, implements, and compares a solution that omits $\mathbf{1}[M_{ct} > 0]$ altogether by defining $Law_{c(t-n)}$ as the average of -1/0/+1 (pro-property owner/no precedent/pro-government decisions). This robustness check (also reported in Appendix Table 8) imposes the additional identification assumption—that pro-property owner and pro-government owner decisions have opposite ef-

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[16]The modal value is typically 0.

[17]To see how, suppose:

$$(5) \qquad Y_{ict} = \beta_{10}Law_{c(t)} + \beta_{11}Law_{c(t-1)} + ... + \varepsilon_{ict}$$

Let the first stage be: $L_{c(t)} = Z_0\Pi_0 + u_0$, where $Z_0 = \begin{bmatrix} p_{c(t)} \end{bmatrix}$ and $L_{c(t-1)} = Z_1\Pi_1 + u_1$, where $Z_1 = \begin{bmatrix} p_{c(t-1)} \end{bmatrix}$. Set $\hat{X} = \begin{bmatrix} \hat{L}_{c(t)} & \hat{L}_{c(t-1)} & ... & \hat{L}_{c(t-j)} \end{bmatrix}$ for $j = 0, 1, ...,$ where $\hat{L}_{c(t-j)} = Z_j\hat{\Pi}_j = Z_j(Z_j'Z_j)^{-1}Z_j'L_{c(t-j)}$.

Observe that $\hat{\beta} = (\frac{\hat{X}'X}{n})^{-1}\frac{\hat{X}'Y}{n} = \beta + (\frac{\hat{X}'X}{n})^{-1}\frac{\hat{X}'\varepsilon}{n}$. Let $\hat{Q} = (\frac{\hat{X}'X}{n})$, then $\sqrt{n}(\hat{\beta} - \beta) = \hat{Q}^{-1}\frac{\hat{X}'\varepsilon}{\sqrt{n}} \cdot \frac{1}{\sqrt{n}}\hat{X}_j'\varepsilon = \frac{1}{\sqrt{n}}\frac{X_jz_j}{n}(\frac{z_j'z_j}{n})^{-1}z_j'\varepsilon = \hat{\Gamma}\sqrt{n}\frac{z_j'\varepsilon}{n}$. Since $\sqrt{n}\frac{z_j'\varepsilon}{n} \to N(0, \Phi_j)$, so $\sqrt{n}(\hat{\beta} - \beta) \to N(0, V), V = Q^{-1}\Gamma\Phi\Gamma Q^{-1}$.

fects of equal size in absolute value. Our main specifications do not impose this identification assumption.

4   Results

4.1   First Stage: The Effect of Judge Identity on Court Decisions The Republican party has traditionally been associated with business interests, which favor growth over inequality, and libertarian interests, which disfavor government intrusion. These conflicting perspectives complicate political lines on eminent domain. Simply being Republican or Democrat does not predict eminent domain votes, which Sunstein et al. (2006) document and we confirm. However, Republican former federal prosecutors have litigated on behalf of the government and represent business and government interest, and they were 18 percentage points more likely to uphold a physical taking from 1958-2008. Racial minority Democrats have worked for the NAACP, a civil rights organization that advocates for racial minorities and backed private property rights, and they were 20 percentage points more likely to strike down a physical taking. These estimates are reported in Table 2 Columns 1 and 2. The number of observations slightly differ because data on whether a judge was a former federal prosecutor (U.S. Attorney) is sometimes missing.

In Column 3, we examine this relationship at the panel-level and include both biographical characteristics in the specification. An additional minority Democrat on one of the 134 three-judge panels decreased the chances of a pro-government decision by 19 percentage points while an additional Republican former federal prosecutor increased the chances of a pro-government decision by 23 percentage points. The similarity in point estimates with Columns 1 and 2 is consistent with their presence being pivotal. In Column 4, we examine this relationship at the Circuit-year level for the 107 Circuit-years with at least 1 case. The estimates indicate that an additional minority Democrat on a three-judge panel reduced the proportion of pro-government decisions by 21 percentage points, while an additional Republican former federal prosecutor increased the proportion of pro-government decisions by 31 percentage points.

At this point in the table, we should expect some sensitivity across columns because the effect of judge identity is analyzed at different levels of data aggregation in order to build intuition. In particular, the Circuit-year level estimates differ from the case level since cases are not evenly distributed across Circuit-years. For an example, suppose that there are 4 cases, one case each with 0, 1, 2, or 3 judges who are minority Democrat, and suppose that the panel makes a pro-taking decision when there are 3 Republican former federal prosecutors. If 1 Circuit-year has the case with 3 minority Democrats 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-government/difference in minority Democrats assigned per seat) but when the 1 Circuit-year has the case with 2 minority Democrats, the coefficient at the Circuit-level is 1.5.

In Column 5, we control for Circuit and time fixed effects, the expected proportion of minority Democrats and Republican former federal prosecutors, and a dummy indicator for whether there were cases, $1\left[M_{ct} > 0\right]$. The number of observations increases to the complete time-frame of 402 Circuit-years. The Eleventh Circuit was not founded until 1981, so the Eleventh Circuit has 6 fewer observations than the other Circuits. The Eleventh Circuit was created by splitting it off from the Fifth Circuit; Fifth Circuit decisions before this split are considered binding precedent in the Eleventh Circuit. We account for this split in our analyses by assigning pre-1981 precedent in the Fifth Circuit to observations in the Eleventh Circuit.

The point estimates are essentially unaffected by these controls, and this stability is important. This stability indicates that the variation in Takings Clause jurisprudence is due to the assignment of these judges and not fixed characteristics of the Circuit or time period nor characteristics of the pool of judges available to be assigned. The F-statistic increases slightly due to the controls, since the controls are predictive of the independent variable (this can be seen in Figure 3), and the increase is not due to the increase in sample size from dummying out for missing values. Redefining $Law_{ct} = Law_{ct} * 1\left[M_{ct-n} > 0\right]$, i.e., defined to be 0 when $1\left[M_{ct-n} > 0\right] = 0$, and regressing it onto the instrument $p_{ct} = \frac{N_{ct}}{M_{ct}} * 1\left[M_{ct-n} > 0\right]$, while including $1\left[M_{ct-n} > 0\right]$ as a control, neither affects the coefficient nor the F-statistic on the joint significance of the biographical characteristics. Dummying out for missing values is standard in the literature. The joint F-statistics of the two biographical characteristics are above the weak instruments threshold.

When we weight our estimates by the number of cases in a Circuit-year, where weights are the geometric mean of $M_{c(t-n)} + 1$ over the distributed lag, the statistical significance of the results increases. We also show in Appendix Table 8 that our results have strong Anderson-Rubin weak instruments-robust test statistics. We did not consider the solution employed in Galasso et al. (2015), which uses the predicted estimate from the first stage as the final instrument. Doing so would greatly increase the F-statistics, and Angrist and Pischke (2008) suggests to avoid doing so in order to not obtain identification from functional form assumptions. Thus, the first stage results should be interpreted as conservative estimates in terms of statistical significance relative to these alternative methods.

Columns 6 and 7 report the first stage analyzed at the Fiserv-level. The coefficients differ from those in Columns 4 and 5 because the Circuits do not contain the same number of zip codes. Notably, the joint F-statistics of the two biographical characteristics are substantially above the weak instruments threshold. The coefficients are similar moving from Column 6 to 7, re-confirming that we identify exogenous variation in Takings Clause jurisprudence due to the assignment of these judges and not fixed characteristics at the Circuit or year level nor characteristics of the pool of judges available to be assigned. Also, our inclusion of $1\left[M_{ct} > 0\right]$ and dummying out for missing values do not bias our coefficients.

To check whether our linear specifications miss important aspects of the data, Figure 4 presents nonparametric local polynomial estimates of the first stage. Estimation proceeds in two steps. In the first step, we regress the proportion of decisions that were pro-government on Circuit and year fixed effects and we regress the instrument on the same. Next, we take the residuals from these two regressions and use a nonparametric local polynomial estimator to characterize the relationship between the instrument and pro-government decisions. Figure 4 shows there is no flat relationship throughout the distribution. These figures also show the tremendous variation across Circuits and years, which will be useful in estimating the impact of Law_{ct}.

While there is a large literature on physical takings, there is less on regulatory takings. Since we have less by way of prior literature to guide us in determining biographical characteristics that affect regulatory takings decisions, we employ LASSO in selecting the Circuit-level instruments. There is also relatively little literature on the appeal of takings decisions, so we also employed LASSO at the District level. The LASSO-selected instruments for District IVs are: (i) Born in 1920s and attended public institution for baccalaureate (BA) and (ii) Evangelical * Born in 1940s with an F-statistic of 27.56 for Fiserv (Zip-Year); (i) Born in 1920s and attended public institution for BA and (ii) Born in 1920s and above median wealth with F-statistic of 9.15 for GDP (State-Year); and (i) Born in 1920s and attended public institution for BA and (ii) Black Prior Law Professor with F-statistic of 29.00 for CPS (Individual-Year). It is to be expected that the LASSO choice of instruments would slightly differ since the time-frame of the data, level of aggregation, and weighting differs slightly across outcome datasets. In any event, as we will see, employing both the District and Circuit IV renders similar results as to employing only the Circuit IV.

4.2 Economic Growth: Property Prices and GDP

Figure 6 displays our central results—the effect of having pro-government Takings Clause jurisprudence on property prices and GDP—and the central falsification test. In property prices, growth is 2% points higher two years after pro-government takings precedent than after pro-property owner precedent. In GDP, growth is 1% point higher two years after pro-government takings precedent than after pro-property owner precedent. Null effects are displayed in the left-side of each pair of graphs. The null effects assure that the growth effects of Takings Clause jurisprudence are not due to secular trends in growth before the court decisions. The null effects are expected since randomly assigned judges do not affect property prices and growth before they are assigned.

Table 3 reports the average effect of pro-government Takings Clause jurisprudence over the subsequent four years. In property prices, it is 1.2% points ($p < 0.01$) while in GDP, it is 1.1% points ($p < 0.01$) (Panel A Column 2). The lead effects are an insignificant 0.3% points and 0.2% points (Panel B Column 2). Column 1 reports OLS estimates, where the lag effects are smaller and insignificant, while the lead effects are similar to the lead effects

when instrumented. Downward bias in OLS estimates occur when judges make pro-takings decisions when there is urban blight. Notably, the estimates are similar when instrumenting for $\mathbf{1}[M_{ct} > 0]$ (Column 3).

Furthermore, the addition of Circuit-specific time trends (Table 4), removing Circuit and year fixed effects, clustering standard errors at the state level, controlling for the expected seat assignments to minority Democrats and Republican former federal prosecutors, weighting the data using population weights, and dropping one Circuit at a time affect neither the size nor significance of the impacts. Table 5 presents the full set of distributed lag coefficients underlying the estimates of Table 4. They consistently show that the effects rise after the first year and are strongest in the second year after the decision as seen in Figure 6. We also checked robustness to lagged dependent variables, wild bootstrap (Appendix Table 3), and clustering standard errors at the Circuit-year level.

The lower half of Table 5 shows that adding or subtracting a lag or two makes little difference. Estimates are also robust to specifications containing 10 years of lags. Notably, since the lags do not sum to 0, we observe growth rather than level effects. Panel B presents a specification with four leads and 1 lag. The magnitudes of the leads are small and insignificant as seen in Figure 6.

The larger effects on house prices than on GDP in Figure 6 are consistent with house prices increasing due to over-investment or being more forward-looking relative to contemporaneous variables like GDP. All estimates are subject to the usual caveats that causal effects are sufficient, but not necessary conditions for an outcome (Deaton 2010). To illustrate, Chen et al. (2016a) use a machine learning approach to analyze the effects of court laws and finds that, of 18 factors that predict abortion attitudes, the court variables comprise 25-30% of the sixth factor.

4.2.1 Further Robustness Note that Takings Clause jurisprudence has a growth impact when the data is collapsed to 1 experimental subject per Circuit-year. Appendix Tables 4 and 5 collapse the data to the Circuit-year level using population weights. Appendix Table 4 Column 6 shows that the average lag effect on property price growth is 1.3% points ($p < 0.01$). The lead effect is an insignificant 0.6% points. Appendix Table 5 Columns 6 shows that the average lag effect on GDP growth is 1.2% points ($p < 0.01$) and the lead effect is an insignificant 0.2% points. Columns 7-9 of the tables show the results with alternative specifications, where LASSO is used to instrument for Law_{ct} and $\mathbf{1}[M_{ct} > 0]$ (Belloni et al. 2012). Columns 1-5 show the results using the full data. The tables in the main text report Columns 1-3, as standard in the literature (Bertrand et al. 2004).

The estimates have similar size and significance as in the non-collapsed data. Appendix Tables 4 and 5 lower half calculate the *typical* effects of Law_{ct} taking into account how many cases typically occur. The conditional effect refers to an effect conditional on a case being in front of a judge. It is a scaling of β_1. The unconditional effect includes the ef-

fect of the presence of a case, β_2. To compute the effect of pro-government precedent in a typical Circuit-year, multiply the coefficient on Law_{ct} by $\mathbf{E}[Law_{ct}|\mathbf{1}[M_{ct} > 0]]$, the typical proportion of decisions that are pro-government when there are Circuit cases, and by $\mathbf{E}[\mathbf{1}[M_{ct} > 0]]$, the proportion of Circuit-years with a Circuit case. To calculate the typical effect of pro-government precedent taking into account the presence of an appeal: $\mathbf{1}[M_{ct} > 0]*\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]+Law_{ct}*\mathbf{E}[\mathbf{1}[Progovernment_{ct} > 0]]$.

Appendix Tables 6 and 7 report the same set of robustness checks for GDP as Tables 4 and 5 did for property prices. Appendix Table 8 reports similar estimates with further sensitivity checks. The instrument is $p_{ct} - \mathbf{E}(p_{ct})$ rather than p_{ct} conditional on $\mathbf{E}(p_{ct})$, the LASSO penalty employs Dantzig selector to adjust for correlation among instrument candidates, judges are dropped when we lack information about their expected assignment probability, and separate first-stages are implemented to reduce over-fitting. Notably, the Dantzig selector chooses minority Democrats and Republican prior U.S. Attorneys as instruments. Columns 4-5 define Law_{ct} as the average of: +1: pro-takings and -1: pro-property owner.

4.2.2 Sectoral Impacts Table 12 provides prima facie evidence on the mechanisms. Physical takings jurisprudence that increased government power of eminent domain spurred annual growth in the government sector by 0.3% points ($p < 0.01$), the transportation and utilities sector by 1.4% points ($p < 0.01$), and the construction sector by 3.9% points ($p < 0.01$), as we expect with public infrastructure (Panel C Column 1). Eminent domain power also contributes to annual growth in the sectors of agriculture by 5.7% points ($p < 0.01$); retail by 1.7% points ($p < 0.01$); and finance, insurance, rental, and estate by 2.2% points ($p < 0.05$). Growth in the service sector is adversely affected by 9.2% points ($p < 0.01$).

Since the effects are observed outside of the government, transportation, and construction sectors, we can be confident that the impact of Takings Clause jurisprudence is not due to the local effect of allowing or disallowing a takings to proceed but due to the precedent that gives more latitude to reassign property rights. That is, while some of these differences are mechanical (allowing a physical takings to proceed directly leads to government spending, property development, and GDP growth as an accounting matter), these sectoral results suggest that the precedent set by the decision is more important than the decision on the litigated government taking. Table 12 also reports calculations of typical effects. Statistical significance come from the estimates of the coefficients, so significance tests are not repeated to save space.

4.2.3 Local Impacts This inference is further supported in Table 5 Panel C. We use zip code-level data to distinguish the local effect of eminent domain decisions affirming or overturning a local taking from the precedential effect of making it easier for subsequent takings. For example, following *Midkiff*, which allowed Hawaii to condemn property because an oligopoly in land ownership was "injuring the public tranquility and welfare," prices

doubled within six years in the locality (Kanner, 2009; Roth, 1992).

$$(1) \quad g_{ict} = \theta_c + \theta_t + \sum_{n=0}^{L} \beta_{7t-n} Law_{ct-n} + \sum_{n=0}^{L} \beta_{8t-n} 1\left[M_{ct-n} > 0\right] +$$
$$\sum_{n=0}^{L} \beta_{9t-n} LocalLaw_{ict-n} + \sum_{n=0}^{L} \beta_{10t-n} \mathbf{1}[\text{LocalM}_{ict-n} > 0] + \varepsilon_{ict}$$

We separately instrument Law_{ct} and $LocalLaw_{ict}$ using judicial assignment in cases that occur in zip code i and in cases that occur in Circuit c. The effects of eminent domain cases are largely through their precedential power (0.7% points compared to 1% point when not controlling for the local effects). We present every fourth quarter in the quarterly lags to save space, but the means are the mean for quarters 0-16.

4.3 Economic Inequality: Racial Disparities and Displacement Government power of expropriation is perceived to have disparate impacts. Previous empirical studies also document that low-valued land is undercompensated (Munch 1976; Chang 2010). Urban renewal efforts have displaced hundreds of thousands of families and tens of thousands of businesses (Garnett 2006). Racial minorities are not employed in the same numbers as those whose homes or businesses are displaced by eminent domain. *Poletown Neighborhood Council v. City of Detroit* awarded General Motors property, but it subsequently employed fewer people than the combined employment of displaced businesses (Somin 2004). We use race to proxy for inequality because of its salience (Semuels 2016) and because unobservables are not rank-invariant to potential treatment status, so quantile regressions by price and income are inappropriate. In particular, the same parcels that have relatively lower property prices without pro-government precedent would not be the ones that would have relatively lower property prices with pro-government precedent.

To the best of our knowledge, the only quantitative study on the distance that individuals move when displaced through eminent domain, is a Canadian study showing that over half move less than 3 miles when displaced and 99% move less than 15 miles (Sherwood, 1975). Qualitative studies report that gentrification–which may or may not include eminent domain–led to displaced persons moving within the same city rather than to a different city (LeGates and Hartman 1982).

4.3.1 Displacement and Housing Every year, 10% of Americans make within-county moves and 6% make out-county moves, according to the March CPS. Using this data (which asks whether an individual moved within the county in the last year, whether an individual moved outside the county in the last year, whether an individual lives in public housing, and whether an individual lives below the poverty line) we find that pro-government physical takings precedent increase within-county moves of racial minorities by 0.1% more than they do of whites (Table 6). This difference is statistically significant at the 1% level. Non-whites are no more likely to make moves from outside the county than whites after pro-government

takings decisions. Whites were 0.2% more likely to make within-county moves and 0.007% more likely to make out-of-county moves after pro-government takings decisions. No effect is found before the decision (Panel B). A previous draft presents further analysis on the impact on racial inequality. Racial minorities also became 0.3% more likely to live in public housing and 0.7% more likely to live below the poverty line.

4.3.2 Employment Every year, 74% of whites are employed and 66% of non-whites are employed.[18] Table 7 reports that eminent domain power increased employment overall, which is consistent with improved economic growth; to calculate: multiply the non-interacted average lag effect by 0.78 (the proportion white) and add the interaction effect multiplied by 0.22 (the proportion non-white). However, racial disparities increased as minorities were 1.7-2.1% less likely to be employed than whites ($p < 0.01$, $p < 0.05$) (Table 7 Panel A Columns 2-3).

4.3.3 Condemnations With increased power of expropriation, we should see government actors acquiring more parcels, providing less compensation per parcel, or taking more expensive property. In some jurisdictions, eminent domain is not authorized for residential properties (Berliner and Coalition 2006). Business owners have noted that re-establishment expenses are insufficient to cover increased rent and remodeling expenses associated with displacement (Garnett 2006). Communities have reported concerns about job loss and trouble attracting new businesses after the number of evicted businesses jumped from 39,000 in 1963 to 100,000 in 1971 (Frieden and Sagalyn 1991).

We examine state condemnations for federally funded transportation projects to measure the behavioral response to government power of expropriation. Variables include: 1) aggregate compensation for all parcels acquired per state-year whether through open market purchase, condemnation, or administrative settlement; 2) aggregate parcels acquired per state-year; 3) displacement and relocation costs.[19] When courts are more likely to uphold a taking, we may see governments exercising increased power of expropriation.

We find that land parcels are taken more frequently from businesses, despite their being more litigious and expensive to displace than residential tenants. Table 8 reports that land parcels were taken 12% more frequently from businesses ($p < 0.01$), but 7% less frequently from residences ($p < 0.05$). Relocation costs of displaced business were 16% higher ($p < 0.01$), but lower for displaced residences ($p < 0.01$). Because of the shift in the type of parcel taken, compensation increased by 12.5% ($p < 0.01$) and number of parcels decreased by 10.3% ($p < 0.01$). These estimates vary in magnitudes across specifications as they are estimated in levels due to the time frame of the available data. No lead effects are found.

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[18]These percentages come from MORG CPS. As standard in the literature, individuals who are unemployed or not-in-labor force are counted as "not employed".

[19]Displaced entities are eligible to receive reimbursements for moving and relocation expenses and the added costs of becoming reestablished at the new location (separately for residential and commercial tenants).

4.3.4 Highways Our highway data includes 2-digit highways (rays away from MSAs that connect central business districts with regions outside the central city), 3-digit highways (circles around MSAs that primarily serve urban areas), 2-digit federal highways (interstate between MSAs), and planned highways. Table 9 shows that government power of expropriation increased highway construction. Highway rays increased by 4% points ($p < 0.1$), highway circles by 0.7% points ($p < 0.01$), and planned highways by 3.3% points ($p < 0.05$). Interstate highways did not increase significantly, but since interstate highways connect across Circuits, the effects will be muted because Circuits face different treatments. Notably, the broad-based growth across sectors in Table 12, especially for agriculture, is consistent with the increase in highway rays connecting rural areas with central business districts.

5 Mechanisms

Even though the differences in government and economic activity seem to be aligned with the differences in judges' revealed preferences (some judges prioritize growth and economic efficiency, while others prioritize equality and fairness), the results we have discussed so far could potentially be attributed to other characteristics of eminent domain power. Expropriation need not be total, but could be partial such as zoning, environmental regulation, or flooding. In the U.S., this is called a regulatory taking and also is influenced by physical takings precedent. One recent study finds that rezoning six neighborhoods increased total employment by 12-21% and weekly wages by 8-13%, amounting to $269 million per year (Busso et al. 2013).

5.1 Impact of Regulatory Takings Tables 11 and 12 report the effect of one dimension of eminent domain power–the precedential effects of regulatory takings jurisprudence. The full list of cases is displayed in Appendix Table 9. Table 11 reports the average effect of pro-government regulatory takings jurisprudence over the subsequent four years. It is 0.2% points in property prices ($p < 0.01$) and GDP ($p < 0.05$) (Panel A Column 2). The lead effects are insignificant (Column 2). In contrast to physical takings precedent, there are no significant effects on racial disparities in within-county moves or employment status (Panel B) and no significant effects on displacements for federal transportation projects or highway construction (Panel C). Panel D verifies that these results are not due to weak instruments. Regulatory takings precedent is more frequent than physical takings precedent, so the scaled effects taking into account the typical jurisprudence per Circuit-year yields similar effects of both sets of jurisprudence (Table 12 Panels A and B). Table 12 Panel A reports typical effects for physical takings jurisprudence and Panel B for regulatory takings jurisprudence. Statistical significance come from the estimates of the coefficients so are not repeated to save space.

Regulations do not necessarily lead to property development, and the construction or transportation and utilities sectors were not significantly affected (Table 12 Panel C). However, pro-government regulatory takings precedent increased growth in the sectors of gov-

ernment by 0.04% points ($p < 0.05$), services by 3.8% points ($p < 0.01$), finance, insurance, rental, and estate by 0.3% points ($p < 0.01$), sectors associated with white-collar industries. It decreased growth in the sectors of manufacturing by 0.9% points ($p < 0.01$) and wholesale by 0.6% points ($p < 0.05$).

5.2 Perceived Takings Risk and Investment TOT may be further broken down between direct precedential and indirect expressive effects (Sunstein 1996; Chen and Yeh 2013, 2014b). Government actors are instructed to keep abreast of the latest laws, but investors may only hear about laws indirectly. Simply hearing about a legal decision can shift people's preferences and experienced utility (Chen and Yeh 2014a) and mobilize individuals to donate to campaigns (Chen et al. 2014b). Newspaper accounts of prominent decisions, such as *Kelo v. City of New London*, made ordinary citizens feel vulnerable (Nadler et al. 2008).

We randomly expose data entry workers to newspaper articles summarizing Courts of Appeals eminent domain decisions and assess whether perceived takings risk responds to eminent domain decisions. The methodology is provided in more detail elsewhere (Chen and Horton 2014; Shaw, Horton and Chen 2011; Chen 2011; Manski 2004). For eminent domain, we ask whether legal decisions shift stated expectations. We hired 266 workers to enter data. After completing the lock-in task (all workers completed 3 paragraphs involving Tagalog translations of Adam Smith's *The Wealth of Nations*), workers in each of 4 treatment groups and 1 control group were asked to transcribe abbreviated newspaper summaries of an eminent domain decision that was either a regulatory or physical takings decision and either a pro-government or pro-property owner outcome. The control group proceeded immediately to the perceived takings risk question:

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

We paid subjects 10 cents to complete each paragraph; a paragraph takes about 100 seconds to enter, so the offered payment was equivalent to \$28.80 per day. To compare, the U.S. federal minimum base wage for tipped waiters is \$2.15/hour (\$17.20 per day) and the federal minimum wage is \$7.25/hour (\$58/day). The exact paragraphs follow below. Original newspaper articles are available on request.

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

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paraan. Ang tunay na puwersa ng mga kuru-kuro na ito ay sapat na, sa kanilang mga masasakting frame, upang gumawa ng na galis o hindi mapalagay damdam complained ng.

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

Regulatory Pro-Government (*Rector, Wardens & Members of Vestry of St. Bart's Church* (1990)): The Federal Court of Appeals upheld the landmark designation of St. Bartholomew's Church in New York City against a constitutional challenge by the Episcopal Parish. The parish argued that landmark status interfered with its property rights. The church had applied for permission to demolish its landmark Community House, to make way for a new office tower, income from which would support church activities. However, in affirming a judgment by a lower court, the Second Circuit Court states that the New York City Landmarks law did not violate the Church's Fifth Amendment right against government takings of property without just compensation, because the church had failed to prove that it could not continue its religious practice in its existing facilities.

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

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

We found that subjects exposed to any eminent domain decision increased their self-reported takings risk by 10% relative to the control group that was not exposed to eminent domain decisions ($p < 0.05$). Figure 7 displays the distribution of responses for the control group and the treatment groups. Both OLS regressions and the Wilcoxon-Mann-Whitney test for

differences in distributions indicate that $p < 0.05$. Pro-property owner and pro-government decisions had no significant difference.

Recall that β_1 captures the effect of the heads-or-tails coin flip (pro-government vs. pro-property owner precedent), $\beta_1 + \beta_2$ captures the effect of the heads coin flip and the presence of the coin (pro-government precedent vs. no decision), and β_2 captures the effect of the tails coin flip and the presence of the coin (pro-property owner precedent vs. no decision). Table 10 Panel C presents calculations of $\beta_1 + \beta_2$ and their joint significance: 0.1-0.2% points for property prices ($p < 0.01$) and GDP ($p < 0.05$).

In sum, these results suggest that the primary effects of Takings Clause jurisprudence is through addressing a failure of Coasean bargaining–the difference in legal precedent that expand or contract the actions that governments can take to reassign property rights, rather than shifts in expectations. A full discussion of β_2 is for another paper. In principle, we can separately identify the effect of the presence of a case from the effect of pro-property owner precedent in a high-frequency outcome dataset during the time window after a case is announced and before the case resolution.

6 Conclusion

Our paper contributes to a literature on the long-run consequences of institutions such as common law (Acemoglu et al. 2001; Dell 2010; La Porta et al. 1998), ongoing debates on whether institutions are mainly products of economic or social determinism (Rosenberg 1993; Klarman 2004), and whether accidents, leaders, and decisions have significant impacts on society (Banerjee and Duflo 2014).

We present a simple model of takings that clarifies competing views of the effects of government power of appropriation. It is frequently believed that a society that fails to protect property rights against governmental restriction also fails to support rule of law and economic growth (North and Thomas 1973; North and Weingast 1989; De Soto 2000). Our model suggests that making it easier for the government to take property rights, even with compensation, typically leads to lower economic growth because of distortion in investment incentives, unless the public use channel dominates. If there are no public use benefits and there is fair market compensation, a greater risk of takings has no effect on economic outcomes. Introducing under-compensation, a greater risk of takings leads to under-investment and lower property prices, and these investment distortions would invariably lead to lower economic growth. Positive growth effects would be due to the benefits of public projects.

U.S. Federal Court judges articulating government power to reassign property rights have important effects on government and economic activity. The effects of their decisions seem to be largely attributable to a shift in government and economic activity directly relevant to the preferences of the judges.

Minority Democrats assigned as judges under the Federal Court system decide eminent

domain cases in a manner more closely linked to racial minorities' concerns: minorities have their property disproportionately taken and are undercompensated, and minority Democrats are 20 percentage points more likely to vote to protect property rights. Republican former federal prosecutors decide cases in a manner more closely linked to business and government interests, and they are 18 percentage points more likely to vote to uphold government takings.

Using data on all U.S. takings precedent in Courts of Appeals, we show that rulings making it easier to take physical property rights spur economic growth and property values, but increase racial inequality as minorities become more likely to be unemployed and displaced.

Consistent with shifts in legal standard and a subsequent increase in actual government activity, states displace larger and more expensive commercial tenants for federal transportation projects and increase highway construction. Property values in the local zip code(s) where the original takings occurred also increase. The economic growth impacts are concentrated in the sectors of construction, transportation and utilities, and government, as one would expect with physical takings, but also in agriculture, retail, and finance and real estate. In contrast to physical takings precedent, decisions making it easier to regulate without having to compensate affect neither condemnations for federal transportation projects nor racial inequality in employment status nor displacement. Economic growth effects are smaller and concentrated in the services, government, and finance and real estate sectors.

Decisions that expanded government power of expropriation increased government reassignment of property rights: local governments displaced larger and more expensive commercial tenants for federal transportation projects and highway construction increased. GDP and property values increased. But racial disparities also increased: minorities became more likely to move and be unemployed relative to whites. Furthermore, results remain virtually identical when including controls.

We show that interpreting the text in different ways impacted growth and inequality in a manner consistent with the "Reverse Robin Hood" concerns that the notion of *public use* has been instrumentalized to help the rich at the expense of the poor (Reinhart 2015). Moreover, these findings have implications beyond eminent domain policy. Policies that improve minorities' bargaining power and access to courts may ameliorate some of the disparate impacts of eminent domain.

Extrapolating these findings on government power of expropriation to other countries needs caution. In the U.S., executive power is relatively constrained by the judicial and legislative branches; moreover, there is a tradition of strong individual property rights, which can lead to potential hold-up problems. For example, a U.S. property owner owns the oil that is discovered under their own land, but the same is not true for most European countries. Spending on public projects, e.g., 95$ billion is spent per year on place-based policies in the U.S. (Kline and Moretti 2014), may be more likely with government power of expropriation. Countries with potential for agglomeration, complementarities, fiscal multipliers, and

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transitions between steady states may also find large effects of eminent domain power.

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Table 1 - Summary Statistics of Takings Precedent	
Circuit-Year Level	Mean [Standard Deviation]
Panel A: Physical Takings Cases (1975-2008)	
Number of Judges	17.66 [7.72]
Number of Physical Takings Panels	0.33 [0.63]
Proportion of Circuit-Years with Physical Takings Panels	27%
Proportion of Pro-Government Physical Takings Decisions when Circuit-Year has Panels	66%
Expected # of Racial Minority Democratic Appointees per Seat when the Circuit-Year has Panels	0.06 [0.06]
Expected # Republican Prior U.S. Attorneys per Seat when Circuit-Year has Panels	0.04 [0.06]
Number of Physical Takings District Cases	1.54 [1.96]
N (circuit-years)	402
Panel B: Regulatory Takings Cases (1979-2004)	
Number of Judges	17.81 [7.46]
Number of Regulatory Takings Panels	0.71 [0.99]
Proportion of Circuit-Years with Regulatory Takings Panels	46%
Proportion of Pro-Government Regulatory Takings Decisions when Circuit-Year has Panels	78%
Expected # of Judges with ABA scores of well-qualified or better per Seat when Circuit-Year has Panels	0.64 [0.13]
Number of Regulatory Takings District Cases	1.58 [1.55]
N (circuit-years)	310

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

Panel A

				Outcome: Pro-Takings				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Racial Minority Democratic Variable	-0.203		-0.570	-0.615	-0.666	-0.943	-0.518	-0.534
	(0.0686)		(0.186)	(0.193)	(0.177)	(0.389)	(0.184)	(0.174)
Republican Prior U.S. Attorney Variable		0.176	0.677	0.929	0.963	0.831	0.553	0.540
		(0.0741)	(0.235)	(0.272)	(0.231)	(0.243)	(0.215)	(0.216)
N	394	307	134	107	402	218	357691	4054704
R-sq	0.017	0.008	0.076	0.108	0.693	0.69	0.062	0.686
F-statistic	8.800	5.638	12.540	9.010	15.220	10.59	34.975	42.747
Pro-Takings measure	Judge Vote	Judge Vote	Panel Vote	Percentage	Percentage	Percentage	Percentage	Percentage
Controls	No	No	No	No	Yes	Yes	No	Yes
Analysis level	Judge	Judge	Panel	Circuit-year	Circuit-year	Circuit-year 1950-1993	Circuit-quarter zip	Circuit-quarter zip

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

Table 3 - House Prices and GDP Impacts

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

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

Table 4 - House Price Impacts -- 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. 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

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 2. Instruments for appellate physical takings are racial minority Democrats 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 racial minority Democratic per seat and a Republican 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 - Dynamic Housing Price Response

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

Notes: Data consist of Fiserv Case-Shiller/FHFA zip-code level price indices. Heteroskedasticity-robust standard errors are in parentheses and clustered by Circuit. The baseline regression includes Circuit fixed effects, year and quarter fixed effects, and a dummy for whether there were no cases in that circuit-year. Expectation controls are expected number of racial minority Democratic appointees per seat and expected number of Republican prior U.S. Attorneys per seat. Population weights are 2005 US Census pop. estimates by zip-code.

Table 6 - Displacement Impacts

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

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

Table 7 - Employment Impacts

	OLS	Appellate IV	Appellate and District IV	Obs	Mean Dep. Variable Non-White	White
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Average Interaction Lag Effect						
Employment Status	-0.015	-0.021	-0.017	6720948	0.655	0.742
Joint P-value	0.016	0.011	0.001			
Average Level Lag Effect						
Employment Status	0.005	0.012	0.010	6720948	0.655	0.742
Joint P-value	0.158	0.000	0.000			
Panel B						
Average Interaction Lead Effect						
Employment Status	-0.019	-0.030	-0.018	6720948	0.655	0.742
Joint P-value	0.011	0.067	0.108			
Average Level Lead Effect						
Employment Status	0.004	0.005	0.002	6720948	0.655	0.742
Joint P-value	0.131	0.356	0.622			

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

Table 8 - Parcels Acquired for Federal Transportation Projects Impacts

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

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

Table 9 - Highway Construction Impacts

Average Lag Effect	OLS (1)	Appellate IV (2)	Appellate and District IV (3)	Obs (4)	Mean Dependent Variable (5)
ΔLog Miles of 2-digit Highways	0.000	0.029	0.040	7805	0.079
Joint P-value of lags	0.168	0.001	0.071		
Joint P-value of leads	0.593	0.855	0.936		
ΔLog Miles of 3-digit Highways	-0.003	0.012	0.007	7805	0.041
Joint P-value of lags	0.081	0.272	0.000		
Joint P-value of leads	0.948	0.943	0.733		
ΔLog Miles of 2-dig Fed Hghwys	-0.002	0.024	0.029	7805	0.085
Joint P-value of lags	0.017	0.040	0.704		
Joint P-value of leads	0.838	0.666	0.962		
ΔLog Miles of Planned Highways	-0.001	0.016	0.033	7805	0.072
Joint P-value of lags	0.034	0.398	0.034		
Joint P-value of leads	0.831	0.194	0.151		

Notes: MSA-level data come from Baum-Snow (2007). Heteroskedasticity-robust standard errors are clustered by Circuit. Regressions include Circuit fixed effects, year fixed effects, a dummy for whether there were no cases in that Circuit-year. All values are in logs of the underlying value plus one. Data range: 1950-1993.

Table 10 - Impacts of Decisions (Including Presence of Appeals)

	OLS	Appellate IV	Appellate and District IV	Obs	Mean Dependent Variable
	(1)	(2)	(3)	(4)	(5)
Panel A					
Average Lag Effect of Law$_{ct}$					
ΔLog Quarterly Price Index	0.002	0.012	0.007	3989626	0.012
Joint P-value of lags	0.032	0.000	0.001		
Joint P-value of leads	0.108	0.505	0.684		
ΔLog Annual GDP	0.001	0.011	0.011	1671	0.066
Joint P-value of lags	0.254	0.000	0.009		
Joint P-value of leads	0.890	0.810	0.453		
Panel B					
Average Lag Effect of Present$_{ct}$					
ΔLog Quarterly Price Index	-0.003	-0.010	-0.006	3989626	0.012
Joint P-value of lags	0.094	0.000	0.153		
Joint P-value of leads	0.732	0.706	0.861		
ΔLog Annual GDP	-0.002	-0.009	-0.010	1671	0.066
Joint P-value of lags	0.040	0.000	0.000		
Joint P-value of leads	0.886	0.620	0.414		
Panel C					
Average Lag Effect of Law$_{ct}$ + Present$_{ct}$					
ΔLog Quarterly Price Index	-0.001	0.002	0.001	3989626	0.012
Joint P-value of lags	0.000	0.000	0.000		
ΔLog Annual GDP	-0.001	0.002	0.001	1671	0.066
Joint P-value of lags	0.040	0.025	0.048		

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

Table 11 - Impacts of Regulatory Takings Precedent

Panel A	Average Lag Effect		Obs	Mean Dep. Variable	
	OLS	LASSO IV			
House Prices and GDP	(1)	(2)	(3)	(4)	
ΔLog Quarterly Price Index	0.002	0.003	2486744	0.11	
Joint P-value of lags	0.086	0.000			
Joint P-value of leads	0.005	0.333			
ΔLog Annual GDP	0.005	0.002	1065	0.56	
Joint P-value of lags	0.024	0.017			
Joint P-value of leads	0.897	0.918			
Panel B	Average Interaction Lag Effect		Obs	Non-White	White
Displacement					
Within-County Move in Last Year	0.002	-0.002	2916474	0.118	0.091
Joint P-value of lags	0.003	0.692			
Joint P-value of leads	0.816	0.194			
Out-County Move in Last Year	-0.000	0.002	2916474	0.063	0.061
Joint P-value of lags	0.031	0.000			
Joint P-value of leads	0.523	0.316			
Employment Status	-0.011	0.004	5341620	0.660	0.750
Joint P-value of lags	0.169	0.958			
Joint P-value of leads	0.115	0.476			
Panel C	Average Lag Effect				
Log Residential Displacements	0.202	1.127	663	3.508	
Joint P-value of lags	0.383	0.496			
Joint P-value of leads	0.594	0.719			
Log Commercial Displacements	0.203	-0.209	663	3.139	
Joint P-value of lags	0.182	0.777			
Joint P-value of leads	0.683	0.687			
ΔLog Miles of 2-digit Highways	0.001	0.003	2453	0.007	
Joint P-value of lags	0.529	0.636			
Joint P-value of leads	0.113	0.401			
ΔLog Miles of Planned Highways	-0.000	-0.001	2453	0.002	
Joint P-value of lags	0.000	0.163			
Joint P-value of leads	0.097	0.796			
Panel D	Fiserv	GDP	CPS	FHWA	
First stage F-statistic					
Appellate LASSO IV	48.36	37.48	50.39	30.01	
District LASSO IV	6.53	6.43	11.51	9.24	

Table 12 - Impact Analysis

Panel A

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

Panel B

	Regulatory Takings			
	(1) Effect of Law_{ct}	(2) $\times\ E(Law_{ct}|M_{ct}>0)$	(3) $\times\ E(1[M_{ct}>0])$	(4) = Typical Effect
ΔLog Annual GDP	0.002	0.78	0.46	0.001
ΔLog Quarterly Price Index	0.003	0.78	0.46	0.001

Panel C

	Sectoral Impacts					
	Physical Takings			Regulatory Takings		
	Effect of Law_{ct}	Joint P-value		Effect of Law_{ct}	Joint P-value	
ΔLog Sectoral Annual GDP		Lags	Leads		Lags	Leads
Construction	0.039	0.001	0.227	-0.016	0.145	0.405
Transportation and Utilities	0.014	0.005	0.311	0.006	0.885	0.725
Government	0.003	0.002	0.470	0.0004	0.027	0.312
Retail	0.017	0.001	0.768	0.002	0.152	0.274
Agriculture	0.057	0.000	0.674	-0.076	0.312	0.303
Finance, insurance, rental, estate	0.022	0.014	0.919	0.003	0.002	0.850
Services	-0.092	0.001	0.456	0.038	0.000	0.830
Wholesale	0.013	0.213	0.300	-0.006	0.033	0.756
Mining	0.018	0.690	0.236	-0.062	0.735	0.465
Manufacturing	0.007	0.784	0.169	-0.009	0.000	0.844
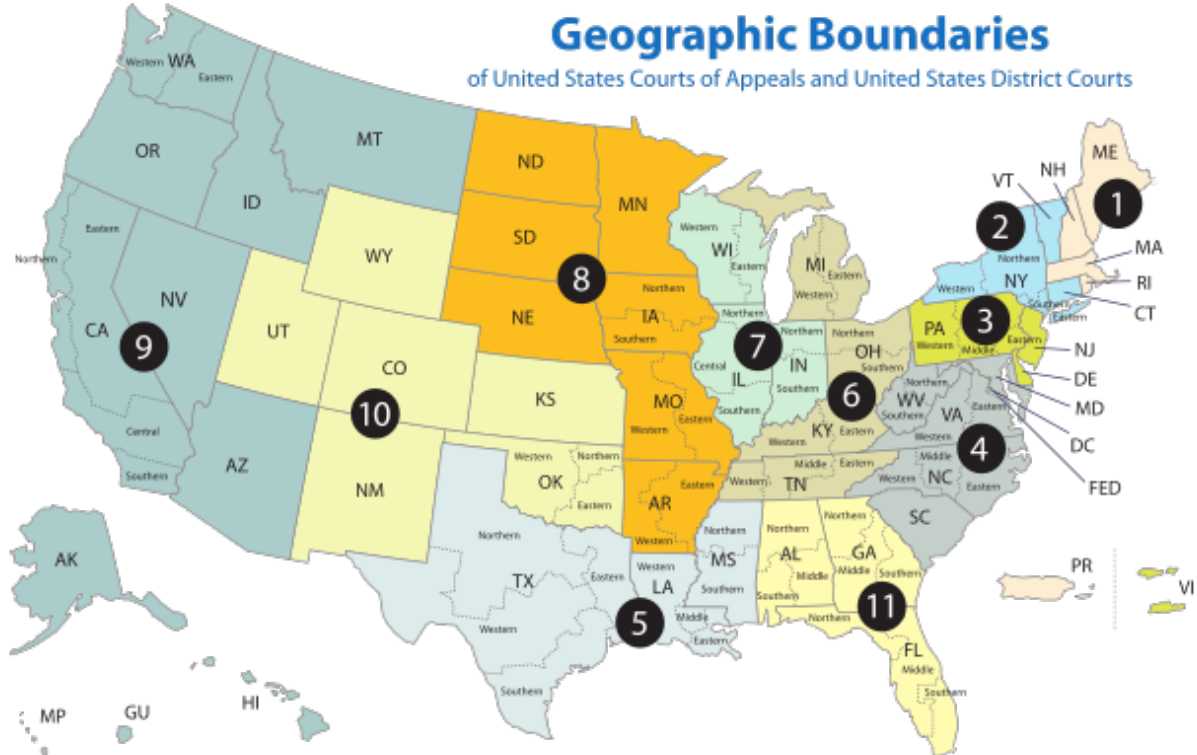
Figure 1

Figure 2 – Map of Alleged Takings
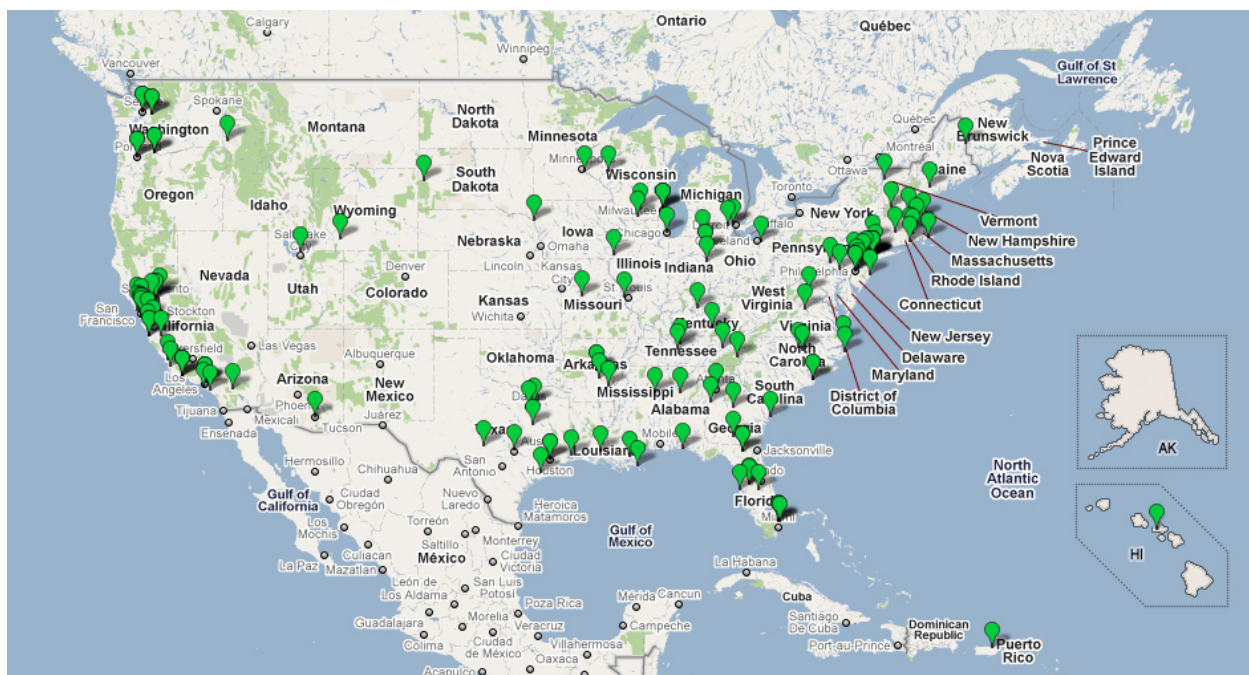
Figure 3 – Random Variation in Judicial Composition

Random Variation by Circuit: Minority Democratic Appointees

Circuit 1 · Circuit 2 · Circuit 3 · Circuit 4 · Circuit 5 · Circuit 6 · Circuit 7 · Circuit 8 · Circuit 9 · Circuit 10 · Circuit 11 · Circuit 12

Expected # of Minority Democratic Appointees per seat
Actual # of Minority Democratic Appointees per seat

Random Variation by Circuit: Republican Prior U.S. Attorneys

Circuit 1 · Circuit 2 · Circuit 3 · Circuit 4 · Circuit 5 · Circuit 6 · Circuit 7 · Circuit 8 · Circuit 9 · Circuit 10 · Circuit 11 · Circuit 12

Expected # of Republican Prior U.S. Attorneys per seat
Actual # of Republican Prior U.S. Attorneys per seat

Figure 4 – Local Polynomial Estimates of First Stage
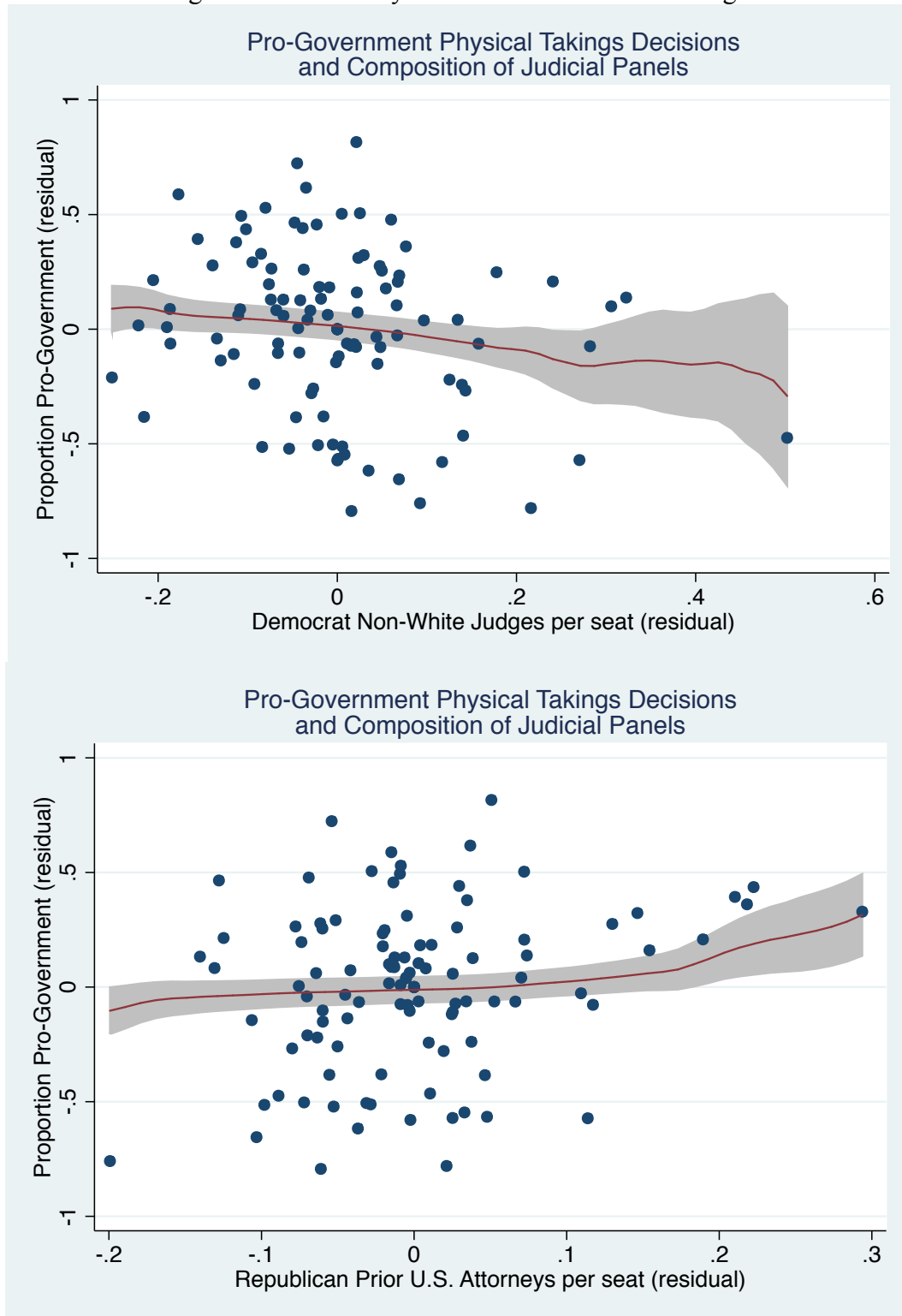
Nonparametric local polynomial estimates are computed using an Epanechnikov kernel. Rule-of-thumb bandwidth is used. Shaded area indicates 90 percent confidence bands. The residuals are calculated removing circuit and year fixed effects.

Figure 5 – Number of Decisions

Appellate Physical Takings Decisions, 1975-2008

- Number of Pro-Property Owner Takings Decisions
- Number of Pro-Government Takings Decisions

Appellate Physical Takings Decisions by Circuit, 1975-2008

- Number of Pro-Property Owner Takings Decisions
- Number of Pro-Government Takings Decisions

Figure 6: Dynamic Reponse to Takings Predecent

Figure 7: Perceived Takings Risk in Response to Eminent Domain Decisions
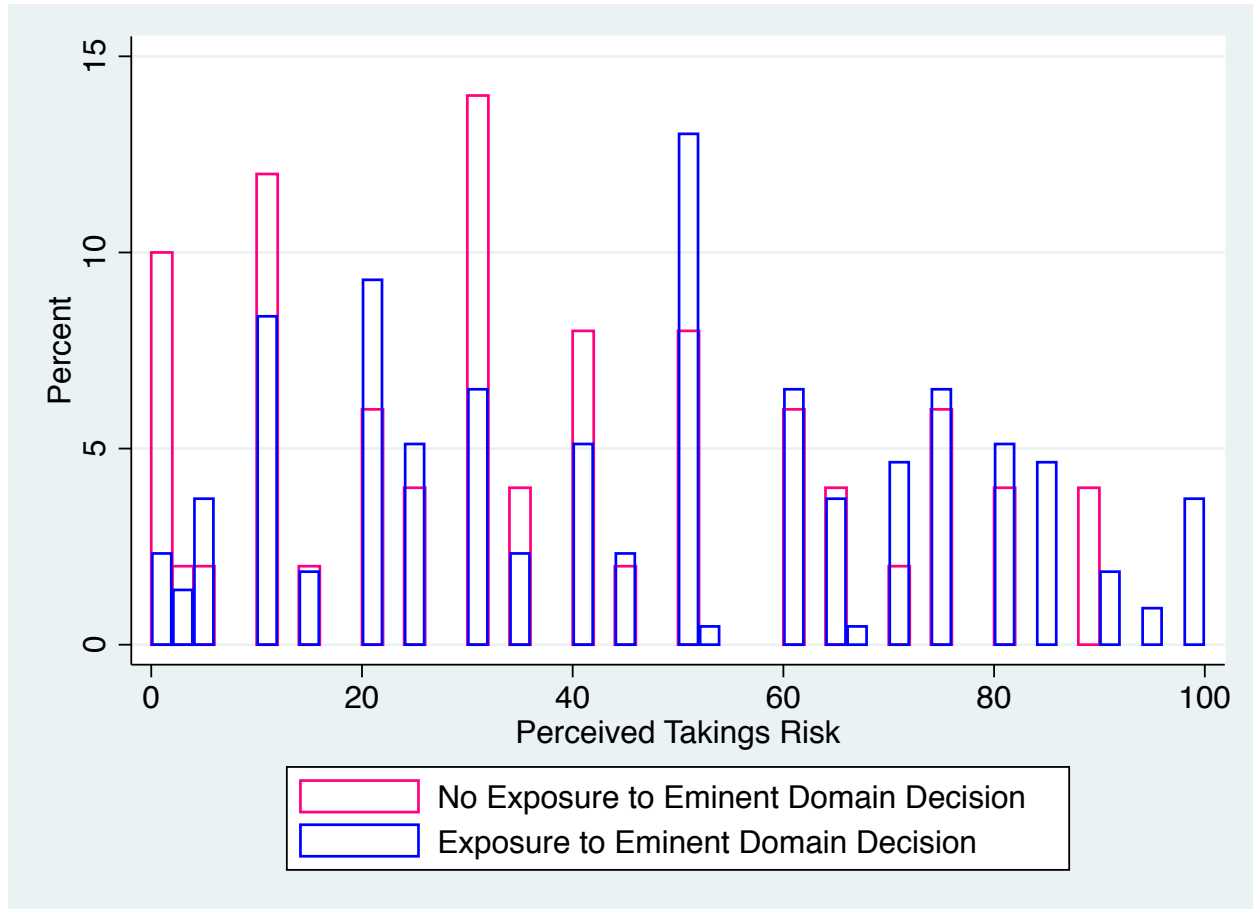
Appendix Table 1: List of Physical Takings Appellate Precedent

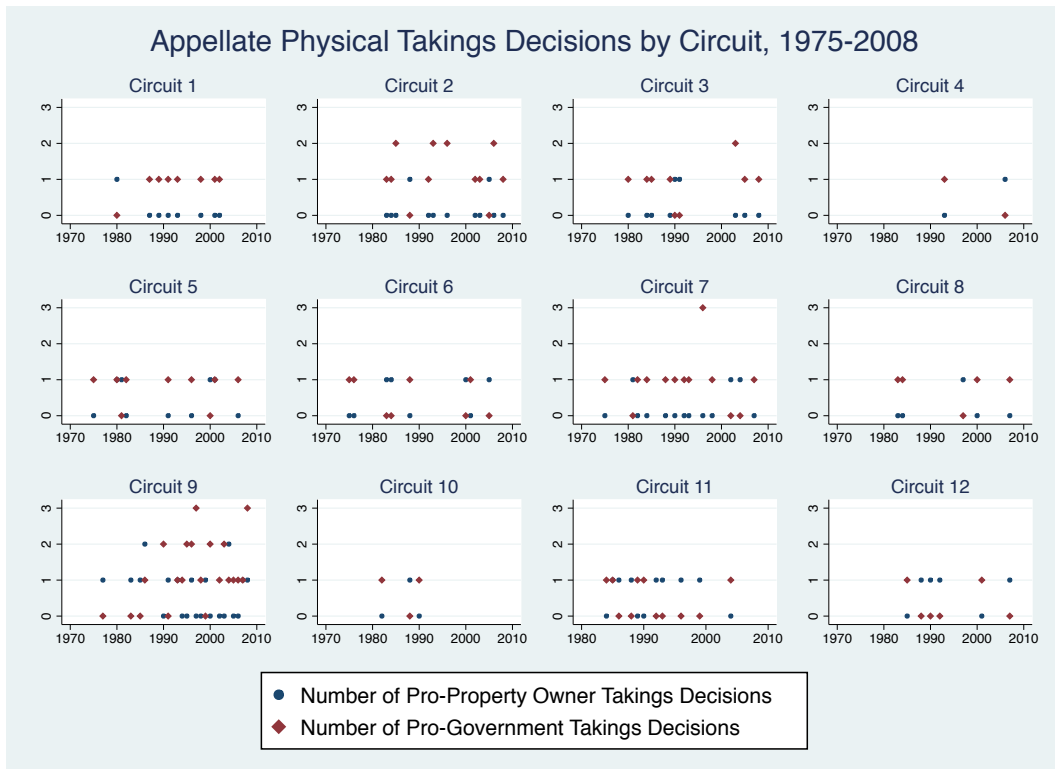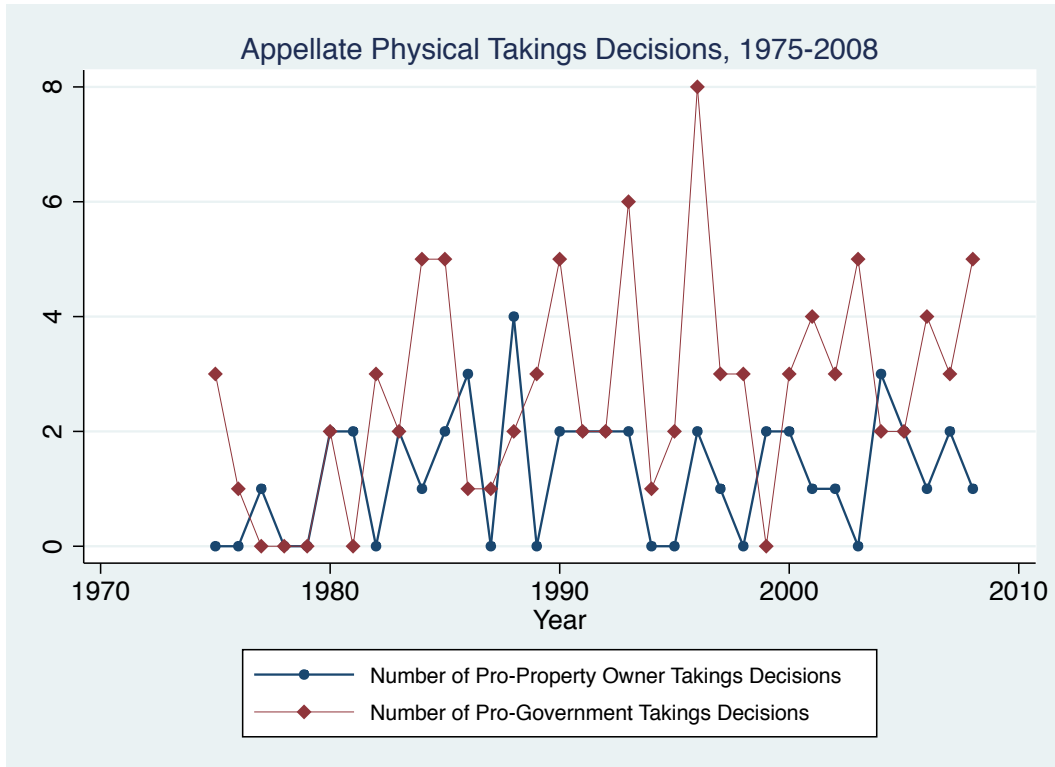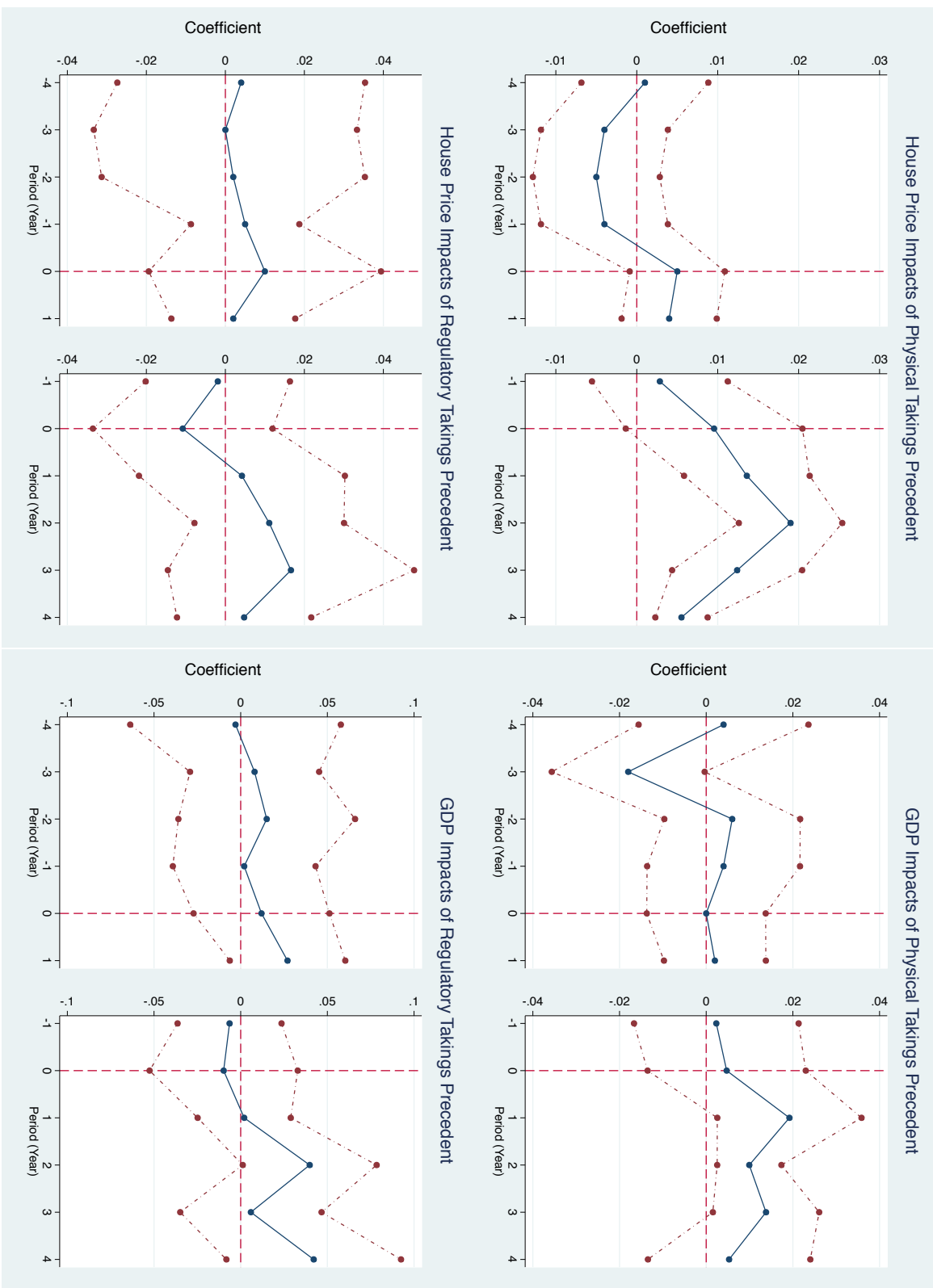| Citation	Case Name	Circuit	Year	Pro-Property Owner
228 F.2d 280	U.S. v. Certain Parcels of Land in Fairfax County, of Va.	4	1955	1
229 F.2d 675	Anderson v. U.S.	5	1956	0
235 F.2d 864	District of Columbia Redevelopment Land Agency v. 61 Parcels of Land in Squares	12	1956	0
249 F.2d 811	Richmond Inv.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

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

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

Appendix Table 2 - Randomization Check

Panel A: P-Values from Kolmogorov Smirnov Test for Deviations from Uniform CDF					
Physical Takings (Racial Minority Democrat Appointees)					
	distance	size	90%	95%	99%
Autocorrelation	0.180666667	9	0.3392	0.3874	0.4795
Mean Reversion	0.318	8	0.3583	0.4097	0.5068
Longest Run	0.200888889	9	0.3392	0.3874	0.4795
Regulatory Takings (Judges with ABA scores of well-qualified or better)					
	distance	size	90%	95%	99%
Autocorrelation	0.218333333	12	0.2958	0.3382	0.4192
Mean Reversion	0.215333333	12	0.2958	0.3382	0.4192
Longest Run	0.179	11	0.3083	0.3524	0.4367

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

Circuit-Year Level	Outcome: Proportion of Pro-Government Physical Takings Decisions$_t$			
	(1)	(2)	(3)	(4)
Racial Minority Democratic Appointees per Seat$_t$	-0.701	-0.645	-0.694	-0.684
	(0.220)	(0.240)	(0.211)	(0.225)
Racial Minority Democratic Appointees per Seat$_{t-1}$	0.554	0.535		
	(0.370)	(0.362)		
Racial Minority Democratic Appointees per Seat$_{t-2}$		-0.525		
		(0.276)		
Racial Minority Democratic Appointees per Seat$_{t+1}$			-0.0788	-0.0824
			(0.398)	(0.394)
Racial Minority Democratic Appointees per Seat$_{t+2}$				0.306
				(0.581)
N	104	103	104	100
R-sq	0.077	0.089	0.064	0.057

Circuit-Year Level	Outcome: Proportion of Pro-Government Regulatory Takings Decisions$_t$			
	(5)	(6)	(7)	(8)
Judges with ABA scores of well-qualified or better per Seat$_t$	0.335	0.328	0.261	0.262
	(0.139)	(0.153)	(0.154)	(0.157)
Judges with ABA scores of well-qualified or better per Seat$_{t-1}$	-0.164	-0.159		
	(0.0912)	(0.0913)		
Judges with ABA scores of well-qualified or better per Seat$_{t-2}$		0.0287		
		(0.137)		
Judges with ABA scores of well-qualified or better per Seat$_{t+1}$			0.0115	0.00911
			(0.101)	(0.106)
Judges with ABA scores of well-qualified or better per Seat$_{t+2}$				-0.00270
				(0.111)
N	142	137	137	135
R-sq	0.217	0.208	0.209	0.207

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

Appendix Table 3 -- Wild Bootstrap

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

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

Appendix Table 4 - 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 Appellate Decisions$_{t+1}$	0.00402	0.00285	0.00166	-0.00212	-0.00258	0.00647	0.000831	0.00616	0.00379
	(0.00230)	(0.00428)	(0.00408)	(0.00703)	(0.0100)	(0.00492)	(0.00437)	(0.00387)	(0.00482)
Proportion Pro-Taking Appellate Decisions$_t$	0.00499*	0.00955+	0.0121**	0.0139*	-0.00000577	0.00860	0.0106+	0.0140**	0.0100*
	(0.00193)	(0.00557)	(0.00445)	(0.00647)	(0.00552)	(0.00583)	(0.00549)	(0.00447)	(0.00436)
Proportion Pro-Taking Appellate Decisions$_{t-1}$	0.00296*	0.0136**	0.0112**	0.00147	0.00353	0.0124*	0.0118**	0.0141**	0.00869*
	(0.00166)	(0.00410)	(0.00547)	(0.00749)	(0.00401)	(0.00617)	(0.00719)	(0.00444)	(0.00594)
Proportion Pro-Taking Appellate Decisions$_{t-2}$	0.00330*	0.0190**	0.00872	0.00478	0.00507	0.0211**	0.0105	0.00985**	0.00567
	(0.00133)	(0.00396)	(0.00364)	(0.00679)	(0.00490)	(0.00506)	(0.00399)	(0.00513)	(0.00428)
Proportion Pro-Taking Appellate Decisions$_{t-3}$	0.00159	0.0124**	0.00652	-0.00393	-0.000501	0.0196**	0.00906	0.00367	0.00256
	(0.00133)	(0.00326)	(0.00566)	(0.00390)	(0.00804)	(0.00427)	(0.00681)	(0.00363)	(0.00539)
Proportion Pro-Taking Appellate Decisions$_{t-4}$	-0.000393	0.00552**	-0.00342	0.000573	0.000291	0.00478	-0.00633	-0.001000	-0.00302
	(0.00129)	(0.00165)	(0.00443)	(0.0107)	(0.00903)	(0.00420)	(0.00596)	(0.00280)	(0.00431)
Aggregation Level	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Zip-Year	Circuit-Year	Circuit-Year	Circuit-Year	Circuit-Year
District IV	N	N	Lasso IV	Lasso IV	Lasso IV	N	Lasso IV	Lasso IV	Lasso IV
Appellate IV	N	Y	Y	N	N	Y	Y	N	N
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	0.000	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 presence	-0.003	-0.01	-0.006	-0.004	-0.003	-0.01	-0.005	-0.006	-0.003
P-value of presence lags	0.094	0.000	0.153	0.286	0.002	0.000	0.000	0.208	0.532
P-value of unconditional $(\text{Law}_{ct} + 1[M_{ct} > 0])$ 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 minority appointees per seat and Republican prior U.S. Attorneys per seat assigned to physical takings cases in a circuit-year.

Appendix Table 5 - Impact of Physical Takings Precedent on Economic Growth

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ΔLog GDP								
Proportion Pro-Taking$_{t+1}$	0.000911	0.00233	0.00506	0.0251	0.00760	0.00202	-0.00482	0.0897	-0.00486
Appellate Decisions$_{t+1}$	(0.000641)	(0.00969)	(0.00674)	(0.0187)	(0.0115)	(0.00769)	(0.115)	(0.115)	(0.00631)
Proportion Pro-Taking$_t$	0.00410	0.00472	0.0114+	0.0241	0.0219	0.00274	0.0771	-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$_{t-1}$	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$_{t-2}$	0.00297	0.00994**	0.00836	0.0110	-0.00451	-0.000318	0.0167	0.0735	0.00659
Appellate Decisions$_{t-2}$	(0.00377)	(0.00378)	(0.00659)	(0.0196)	(0.0377)	(0.0120)	(0.0740)	(0.0845)	(0.00900)
Proportion Pro-Taking$_{t-3}$	-0.00288	0.00528	0.00677	-0.0103	-0.0229	0.0114	0.00693	0.0443	-0.00736
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$_{t-4}$	0.000282	0.0138*	0.0112	0.0157	0.00122	0.0194*	0.0128	-0.0468	0.0139
Appellate Decisions$_{t-4}$	(0.000342)	(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	Lasso IV	Lasso IV	Lasso IV
District IV	N	N	Y	Lasso IV	Lasso IV	N	Lasso IV	Lasso IV	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 presence	-0.002	-0.009	-0.01	-0.011	-0.011	-0.009	-0.004	-0.007	-0.003
P-value of presence lags	0.040	0.000	0.000	0.002	0.400	0.019	0.035	0.202	0.086
P-value of unconditional	0.040	0.000	0.000	0.002	0.400	0.019	0.035	0.202	0.086
$(Law_{ct} + 1[M_{ct} > 0])$ 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	0.0012
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 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.

Appendix Table 6 -- 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
F. 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. Instruments for appellate physical takings are Democratic minority Appointees per seat and Republican Prior U.S. 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 7 -- 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. 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 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 8 - Impact of Physical Takings Precedent on Growth

Robustness to Alternative Definition of Appellate and District IV, Judge Sample, LASSO penalty, and Definition of Law

	Law_{ct} is % Pro-Government Taking Precedent			Law_{ct} is Mean of Pro-Government (+1) and Pro-Owner (-1) Precedent	
	Appellate IV	Appellate and District IV	LASSO IV	Appellate IV	LASSO IV
Average lag effect	0.00679	0.00280	0.00317	0.00296	0.000317
P-value of lags	0.00350	0.0330	0.0000233	0.0547	0.00000171
Average lead effect	-0.00117	-0.00313	0.00439	-0.00250	0.00281
P-value of leads	0.954	0.889	0.387	0.758	0.251
Average lag of presence	-0.00610	-0.00295	-0.00221		
P-value of presence lags	0.117	0.0402	0.00307		
Average lead of presence	0.00239	0.00612	-0.00239		
P-value of presence leads	0.875	0.850	0.596		
Anderson-Rubin test statistic	49.29	684.0	888.7	13.10	35.23
N	2295	2295	2295	2295	2295
R-Sq	0.403	0.378	0.425	0.393	0.421

Appendix Table 9: List of Regulatory Takings Appellate Precedent

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

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

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

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

Appendix Table 10 - LASSO Instruments for Regulatory Takings Panels, 1979-2004

Panel A

Appellate-level LASSO Instruments

Outcome: Pro-Takings Precedent

Fiserv (Zip-Year)	Secular * ABA score of well-qualified or better
GDP (State-Year)	Secular * ABA score of well-qualified or better
CPS (Individual-Year)	Secular * ABA score of well-qualified or better
FHWA (State-Year)	Secular and had prior government experience

Panel B

District-level LASSO Instruments

Outcome: Presence of Appellate Case

Fiserv (Zip-Year)	Born in 1940s * Born in 1940s, Protestant Judge whose BA is from appointment state
GDP (State-Year)	Born in 1920s * Mainline Protestant who attended baccalaureate in state of appointment
CPS (Individual-Year)	Born in 1910s * Catholic, BA from appointment state * BA from appointment state
FHWA (State-Year)	Born in 1940s * Born in 1940s

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