

**Dr. P. Phillips School of Real Estate**

**Using Eminent Domain for Economic Development:  
Does it Increase Private Sector Employment?\***

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**Abstract.** Although controversial, local and state governments draw on broad interpretations of the Fifth Amendment takings clause to justify using eminent domain for economic development. Previous studies examine such uses from the perspective of property rights and the scope and size of government. This paper addresses the fundamental question: Do states that grant local governments liberal eminent domain powers actually enjoy greater economic growth? This paper estimates how liberal eminent domain laws affect private sector employment growth across states while controlling for national trend and industry mix effects. The results clearly show that allowing local governments to use eminent domain for economic development does not lead to more private sector jobs.

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[H]ousing will almost never afford a community with the economic development benefits that a commercial application will. If economic development as a sole justification for public use is decided using a rational basis test with deference to local legislative bodies, then the door is left open for local governments to abuse their eminent domain powers and take developable land...

*National Association of Home Builders and  
National Association of Realtors<sup>1</sup>*

## **1. Introduction**

According to the 2005 U.S. Supreme Court decision *Kelo v New London*, the Constitution does not prohibit governments from using eminent domain to stimulate local employment and tax bases. While the constitutional question is settled, what remains unsettled is the economic question at the root of granting governments such power. Does using eminent domain to stimulate economic development actually do so? Simply put, do states that allow local governments to use eminent domain for private economic development actually have greater job growth than states that do not allow such uses of eminent domain? This paper provides the first empirical answer to this question.

Even before the U.S. housing crisis and the ensuing tepid recovery, state and local governments' attempts to stimulate economic activity highlighted debate concerning the proper role of government power over private property. *Kelo* places the definition of public sector power to take private property squarely in the hands of the individual state governments. The Supreme Court decision led to a backlash of state legislative action in thirty-seven states as they acted to update their eminent domain laws to restrict the use of eminent domain for economic development purposes (Cohen 2006, Lopez et al. 2007, Lopez et al. 2009). Even some industry groups historically supportive of economic development policies recognize the long-term threat to an important principle

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<sup>1</sup> Pickel (2004:2), brief of amici curiae in support of the petitioners in *Kelo v. City of New London*.

underpinning economic vitality, that of secure private property rights. For example, the National Association of Home Builders (NAHB) and the National Association of Realtors (NAR) together filed a brief of amici curiae in support of homeowners defending their private property rights against the City of New London, CT, in *Kelo* (Pickel 2004). Perhaps more than any other argument, the collective stance of industry groups like the NAHB and the NAR highlights the difference between relying on government policies that actively target specific economic development projects and an institutional framework broadly conducive to long-run economic growth. While active government policies may directly create jobs and can generate significant multiplier effects on local economies, development policies relying on eminent domain to underpin public-private development partnerships run the risk of undermining broader confidence in government respect for private property, possibly adversely affecting economic development in the longer run.

So the question remains: Does exercising eminent domain on behalf of private developers to create new jobs and increase local government tax revenues serve the broader interests of the public? Anecdotal evidence suggests the answer to this question is not obvious. Pfizer Pharmaceuticals, the private developer in the *Kelo* case, never built its proposed facility in the Fort Trumbull neighborhood in New London, CT. Today the site serves as a dump for storm debris. In contrast, the GM plant in Detroit, the subject of *Poletown Neighborhood Council v City of Detroit* (1981), another landmark eminent domain case, was successfully completed and remains in production. These represent only two cases. But they are nationally recognized cases tied to landmark legal decisions that bookend the full range of outcomes. Reliable comprehensive micro-level data on

countless other eminent domain-led development efforts are difficult to assemble. Nonetheless, we can use private sector employment data to answer the fundamental question. Do states that have liberal eminent domain enjoy greater economic development than states that have not explicitly embraced this approach? Or more specifically, do these states actually enjoy greater private sector job growth as a result of these economic development efforts?

This paper uses private sector employment data across states to examine whether granting liberal eminent domain powers for economic development to state and local governments leads to greater job creation. The study initially looks at total private sector employment growth effects. This approach, however, might not get to the heart of the question. Probing more deeply, we look at not only changes in total employment, but also the local competitive effect identified in shift-share analysis which picks up both direct and multiplier or indirect effects of development. The local competitive employment effect represents the growth or decline in jobs relative to the national trend and other states with the same industry mix. It controls for the fact that some of the observed employment growth is attributable to the national trend and some is attributable to the state's mix of faster and slower growing industries during the period of analysis. The empirical model estimates how much of this unexplained state employment growth can be attributed to allowing liberal use of eminent domain for economic development purposes.

Regardless of how employment is measured, the empirical analysis provides absolutely no support for the notion that *Kelo*-type eminent domain used to promote

economic development leads to faster private sector employment growth. There is evidence that it leads to slower growth.

The next section discusses the relevant literature and empirical motivation for the present research. Section 3 discusses the data and empirical approach. Section 4 presents the empirical results and section 5 concludes.

## **2. Background**

The *Kelo* (2005) decision upholds government use of eminent domain to forcefully transfer property from one private party to another private party when it serves a broadly defined public purpose such as economic development. The decision represents the culmination of the steady widening of the public use clause in the Fifth Amendment of the U.S. Constitution, replacing “public use” with the more elastic “public purpose” requirement, a process that began in *Berman v. Parker* (1954) to justify large scale public-private partnership urban renewal programs. According to *Kelo* it is appropriate to use eminent domain to increase the local tax base if individual states so wish; there is no federal constitutional prohibition on such actions. Local governments often point to the anticipated increase in tax revenues when justifying controversial eminent domain plans to the public. But this raises questions about whether the practice actually serves the public interest beyond increased local tax revenues or short run employment effects.

*Kelo* brought into the public eye the pros and cons of traditional economic development practices, particularly those underpinned by the exercise of eminent domain. Successful economic development efforts increase economic activity in one location and, while it is usually at the expense of lost activity in another location, it need not result in a

zero-sum game from the broader regional or national perspective (Braid 1996). Nonetheless, unsuccessful development efforts can cost taxpayer dollars and some property owners their homes or businesses while generating no increased economic activity in return. This political type of economic development also places existing, usually smaller, firms at odds with the new, usually larger, entrant when tax incentives or discounted property not enjoyed by the existing firms are a significant part of the package to lure the new entrant. Then of course there is the risk that the footloose entrant will be lured to yet another location upon expiration of the incentives. All of this assumes that the new project actually represents a potential net economic gain to the community. Many examples across the country suggest that too often public-private partnerships favor politically-connected parties at the expense of those less connected (Berliner 2003).

The public's response has not been one-sided against the Court's majority decision. Ranis (2007) argues for greater constraints on capital mobility and free trade as a path to local economic opportunity, envisioning the outcome as mixed-use development with high-paying jobs and a strong American company, Pfizer, at the anchor. He cites reports from project consultants and vested interests: the New London Development Corporation, RKG Associates, Inc., Durham, New Hampshire Economic Development Corporation, the University of Connecticut Center for Economic Analysis, and the Frank E. Downes Construction Company. The project was to provide the following:

a harbor front hotel and conference center, a residential housing complex, a Coast Guard Museum, a wellness center and office facilities, a bioscience installation, a public Riverwalk, waterfront and marina with commercial stores, cafes and kiosks, and a "village green" park. This development contemplates new investments of \$170 million and the creation of over 6,000 new jobs and almost an additional 3,000 indirect jobs by way of the development projects as well as an expansion of

Pfizer's Global Development facility. Moreover, the entire development process is slated to provide an additional 1,600 construction jobs...[to a city] suffering from deep economic and social disadvantage, steep economic decline, high unemployment and fewer residents today than in 1920.<sup>2</sup>

The homes were taken and cleared, but the development never occurred; the site remains vacant except for vegetative debris dumped on the site after Hurricane Irene in 2011.

The Supreme Court majority's economic and fiscal arguments rationalizing its *Kelo* decision do not take into account the inability of many municipality or development corporations to realize projected economic gains. Pfizer is not the first company to fail to follow through with an investment after incentives have been approved and homeowners have been forced from their properties. The underlying current of the market is blind to political whims and sometimes a project turns out to be inferior even for a heavily subsidized participant when compared with other options. In other cases, the project may begin as planned, but the ultimate potential may prove less than estimated, or the project may fail entirely. Private markets always confront the possibility of such failures, but when government-forced property takings are part of the equation, the liquidation or reorganization costs can be great or even insurmountable.

Academic research in the wake of *Kelo* has examined various aspects of *Kelo*-type eminent domain and property rights institutions. Lopez, Jewell, and Campbell (2009) study states' legislative reactions to the decision. At the time of their research thirty-seven states updated their eminent domain laws in response to *Kelo*. The study examines to what extent the backlash in each state motivated legislative action. Using data from Berliner (2003) on the number of threatened and filed takings in each state from 1998 through 2002, they find significant evidence for the propensity to update

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<sup>2</sup> From Ranis (2007: 196) and footnote 5 of same.

eminent domain laws, but not necessarily laws with teeth. Loopholes abound, so the study probes deeper to find that twenty-three of the thirty-seven states' updated laws do in fact place effective constraints on eminent domain powers. Their econometric tests suggest that a history of economic freedom in a state is positively correlated with the likelihood of enacting a law that actually narrows eminent domain powers. (See Karabegovic and McMahon (2006) for more on the economic freedom index.) Likewise their measure for housing values is positively correlated with stronger laws, a result consistent with the disciplining effect envisioned in the Tiebout or homevoter hypothesis (Fischel 2001).

DeGenarro and Li (2012) investigate the potential effect of broad eminent domain powers on business formation. Here again, the rationale relates to risk of ownership in the face of more liberal eminent domain powers. They argue that entrepreneurs may forego plans to follow through on business formation depending on the probability of government takings and the expected compensation in the event of such a taking. They find no empirical support for this notion.

Turnbull and Salvino (2009) examine the economic effects of broad eminent domain power on the relative size of the government sector across states, finding evidence consistent with Brennan and Buchanan's (1980) leviathan hypothesis. The study's motivation stems from the language in the *Kelo* ruling that sanctions local governments using eminent domain to redistribute resources in hopes of increasing tax revenue in exchange for lower valued, usually residential, properties. The study tests whether a state whose constitution or statutes explicitly empowers local governments to use eminent domain for private development projects tends to have a larger government

sector in proportion to its aggregate personal income. Surveying state approaches to *Kelo*-type eminent domain in 2000 to construct their measure of eminent domain, they find that the handful of states expressly allowing eminent domain for economic development purposes have larger state and local public sectors than states that do not give their local governments similar eminent domain powers. It appears that, to the extent such development policies are successful in generating greater tax capacity, additional tax capacity is used primarily to support greater public spending rather than tax relief for residents. The empirical results are reminiscent of the flypaper effect in that, once pulled into the public sector, resources tend to stay in the public sector (Mueller 2003, Turnbull 1998).

These studies address a range of important questions, but do not address whether or not allowing eminent domain for private economic development actually yields more economic development than would otherwise occur. While Turnbull and Salvino (2009) find larger public sectors, relatively larger government sectors do not imply absolutely larger private sectors or greater employment growth.<sup>3</sup>

This study is motivated by the unanswered question about whether allowing eminent domain for economic development actually leads to more economic development. We start from the simple premise that institutions (can) matter. The institutional literature suggests that legal regimes, tax regimes, and regulatory environments affect economic growth (Baumol 1990, North 1991). This perspective

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<sup>3</sup> The interpretive review of the large body of international empirical studies by Bergh and Henrekson (2011) concludes that greater national government spending leads to lower economic growth. Comparable state level empirical evidence is not available for the U.S., but the international evidence is suggestive for states or regions as well.

implies that states using *Kelo*-type eminent domain are less likely to be growth states – a testable hypothesis.

### **3. Empirical Models**

We construct two of our state private sector employment growth measures using shift-share analysis. Shift-share analysis has been around for many years and has shown up in different applications in many economic fields. Graham and Spence (1998) provide a rigorous discussion of the economic theory underlying the empirical method. Understanding the effect of policy decisions on employment growth requires that we isolate the employment growth in the region that would occur naturally (or based on national trends) from employment growth that might occur as a result of existing institutions or policies undertaken in the region. Shift-share analysis provides a simple way to separate these effects and, in our case, to specifically answer whether the use of broader eminent domain powers in a state actually leads to greater than expected employment growth. The Supreme Court’s conclusion underlying the *Kelo* decision was that greater economic development of an area meets the broadly-defined public purpose doctrine; therefore, we use increased private sector employment growth as our indicator of whether or not economic development is successful.

Using two consecutive periods (in our case, census years), the objective of shift-share analysis is to decompose the total change in employment by source. Define the following notation:

$N_i$  = national employment in industry  $i$  in the first period;

$n_i$  = regional employment in industry  $i$  in the first period;

$G_i$  = national growth rate in employment in industry  $i$  over the time period examined.

$g_i$  = regional growth rate in employment in industry  $i$  over the time period examined.

Total growth rate and total employment for the nation and region use subscript  $T$ , or  $G_T$  and  $g_T$  and  $N_T$  and  $n_T$ , respectively

Shift-share decomposition begins with the fact that job growth can be determined by multiplying the rate of growth by the starting level of employment in an individual industry ( $n_i g_i$ ) and summing across all industries in the region to obtain total regional employment growth  $\sum_i n_i g_i$ . Using the simple identity to start,

$$\sum_i n_i g_i = \sum_i n_i g_i \quad (1)$$

If employment in the region grows at the same rate of total employment for the nation as a whole then total employment growth is simply  $G_T \sum_i n_i$ . On the other hand, if regional employment in each industry grows at the same rate as each industry in the nation then total regional growth is  $\sum_i n_i G_i$ . Add and subtract both summation terms to the right hand side of (1) and rearrange to find the standard *shift-share equation*

$$\sum_i n_i g_i = G_T \sum_i n_i + \sum_i n_i (G_i - G_T) + \sum_i n_i (g_i - G_i) \quad (2)$$

Shift-share analysis arithmetically separates the changes of a region's employment into three categories. The first r.h.s. term is the portion of employment change in a region

based on national growth rates in employment across all industries, which is basically the employment growth that all regions in the country experience if they grow at the national average rate.

The second and third terms in (2) are the employment mix and local competitive effects, respectively. The second term, the employment mix effect  $\sum_i n_i(G_i - G_T)$ , measures the amount of growth in total employment that is a result of one region having a different industry mix than other regions or the nation as a whole. This part of the shift-share equation accounts for higher or lower employment growth that is solely due to the fact that the state may comprise high or low growth industries.

The third r.h.s. term,  $\sum_i n_i(g_i - G_i)$ , is the local competitive effect. This term is the focus of additional attention in the regression analysis, since it explains the growth or decline in employment that is unique to an individual state after controlling for national trends and industry mix effects on employment growth or decline. The local competitive effect differs across states due to state-specific advantages or disadvantages. These advantages or disadvantages can be the result of differences in agglomeration economies (localization economies and urbanization economies), tax policies, or economic development initiatives across states. A positive local competitive effect indicates that the state is growing faster or declining slower than other states in the nation that have similar industry mixes – as a result of that state’s specific factors. A negative local competitive effect indicates that the state is growing slower or declining faster than other states with similar industry mixes.

We construct the dependent variables used in some of the regression models using dynamic shift share analysis on 47 of the lower 48 continental states. Virginia is not

included because the unique jurisdictional structure of independent cities and counties in the state (Turnbull and Tasto 2008) may have its own effects on economic development and employment. The data are drawn from the pre-*Kelo* period in order to focus on the effects of long running differences in state property rights. We have 94 observations measuring the change in employment by SIC sector from 1980-1990 and 1990-2000. This employment data allows us to complete the shift-share analysis and construct the three different dependent variables used in the analysis.

The first dependent variable is the change in total state private sector employment, which provides both a basic intuitively appealing measure of successful economic development as well as a baseline for comparing other models.

Our primary interest is to determine whether different governmental policy initiatives affect economic development; specifically, whether states that allow broad uses of eminent domain powers actually help foster economic development. Therefore, our attention focuses on the local competitive effect term in (2). Recall that the local competitive effect captures the effects of state differences – controlling for national trends and industry mixes – on the growth or decline in state employment. Our empirical model examines the effect of eminent domain on private sector employment growth that can be attributed to the local competitive effect. Therefore, we define our second dependent variable as the local competitive effect scaled by the percent change in total employment.

The third dependent variable, the adjusted local competitive effect, is a refinement of the second. It measures the local competitive effect only for industries with location quotients ( $LQ$ ) greater than one. The rationale for this alternative dependent variable

arises from the notion that urban and regional economies can be dichotomized into export base and local consumption sectors. Export base or basic industries produce goods and services for export outside the regional economy and typically have an  $LQ > 1$ ; non-basic or local consumption industries produce goods and services that are consumed within the regional economy. Expanding export base employment to meet growing demand from outside the region is the source of indirect or multiplier effects on employment in the region. As such, growth in the export base sector drives overall economic growth in the region. A large part of the multiplier effects result in greater employment in non-basic or local consumption industries. Industries with location quotients greater than one represent part of the export base of the regional economy. Using this export base local competitive effect as an additional dependent variable allows us to test whether the use of eminent domain affects the export base sector differently than the non-basic or local service sector.

Our first set of tests use OLS regressions for the three dependent variables with two separate alternative measures of fiscal decentralization from the literature included (own source revenue and expenditure). We also recognize, however, that there may be an endogenous relationship between our key variable, eminent domain, and employment growth. States that have lower than average employment growth may choose to allow local governments liberal eminent domain powers in order to make targeted economic development policies easier to implement to help improve their economic performance. On the other hand, it is possible that low growth states instead may want to remove any potential barrier that could inhibit growth and choose to restrict local government eminent domain powers.

Therefore, the second set of tests is based on instrumental variable (IV) regressions to remove possible endogeneity. In order to identify eminent domain in the IV estimation we include our set of control variables discussed below, the number of lawyers per 1,000 population, the percent of state land area owned by the state government, and a measure of the income skewness. The number of lawyers per 1,000 population follows the rationale offered by Baker, et al. (2001) in their study of adverse possession statutes across U.S. states. Higher proportions of lawyers may indicate preference for non-market based solutions to problems, which is expected to be correlated with the willingness to allow the use of eminent domain to end disputes or develop areas not supported by the market.

The percent of state land area owned by the state government is included as an indicator of the degree to which certain states have an interest in owning or developing land in the future. States that own more land may be inclined to favor broader uses of eminent domain to make future land acquisitions less arduous. Alternatively, states that own more land may find further acquisition unappealing. A measure of income skewness (ratio of state median to average household income) is included to capture income distribution effects on the choice of institutional restrictions on local authorities.

The key variable for our analysis indicates whether a state's constitution or legislation expressly allows the use of eminent domain for economic development. This is a dummy variable that takes a value of one for states expressly allowing such eminent domain. In the sample, Connecticut, Kansas, Maryland, Michigan, Minnesota, North Dakota and New York expressly allow the use of eminent domain for economic development during the sample period. All other states either expressly prohibit its use or

have no constitutionally or legislatively established policy. As noted earlier, Michigan's inclusion in the liberal eminent domain group is due to the fact that the Michigan Supreme Court *Poletown* decision allowing eminent domain for economic development purposes was in effect in 2000; the Court's *Wayne v Hathcock* decision reversing *Poletown* occurred later in 2004 and therefore could not have any effect on development outcomes during the sample period.

It is worth noting at this point that there are other feasible measures of eminent domain powers. Conceptually, one could construct a measure based on the number of eminent domain cases initiated for the purposes of economic development or the number of cases mentioned in the press. Each of these measures has its uses, but they do not necessarily measure the existence of an affirmative power of local governments to exercise eminent domain. Many, if not most, potential eminent domain cases are conducted as voluntary transactions without the local government going through the formal process of condemnation. As a practical matter, the *threat* of condemnation is often sufficient to compel property owners to settle with the authorities. After all, the owner will lose his or her property; all that can be legally contested is the amount of compensation. Similarly, only the most egregious or heart-rending eminent domain cases with greatest human-interest appeal will likely be reported in the press. Therefore, we adopt what we believe is the most straightforward measure of the threat of such eminent domain to property owners; whether such power is explicitly granted to local governments or public agencies by the state.

Employment growth in a state is influenced by many exogenous factors. Under *Kelo*, the exercise of eminent domain can be justified as public use if the government's

intent is to promote economic development. This suggests a simple empirical test, whether or not states that expressly allow this opportunity experience greater than normal employment growth rates while controlling for other factors that could also affect employment growth. One of the approaches taken here focuses on the local competitive employment growth effect. A significantly positive coefficient on the eminent domain variable indicates that states expressly allowing the use of eminent domain for economic development experience faster than average growth or decline slower than average – after removing the national trend and industry mix effect on state employment growth. On the other hand, a negative coefficient indicates that states allowing broad eminent domain grow slower or decline faster than other states on average.

Following previous research on the fiscal effects of institutions, the empirical models also include controls for a variety of state and local government structure and socio-economic factors (Campbell 2004, Oates 1985, Nelson 1987, Zax 1989, Turnbull and Salvino 2009). Tables 1 and 2 provide variable definitions and sources for the dependent and independent variables used in the regression analysis. Table 3 reports summary statistics.

The set of independent variables includes two alternative measures of fiscal decentralization. Own source revenue decentralization is defined as local own source revenues as a share of total state and local own source revenue. The second measure is expenditure decentralization, which is defined as local general expenditures as a share of total state and local general expenditures. The evidence on the relationship between governmental decentralization and economic growth in the U.S. is mixed. Stansel (2005) finds a direct relationship between employment growth and fiscal decentralization at the

local level while Hammond and Tosun (2009) do not for their sample of municipalities and counties.

The government fragmentation variable is defined as the number of county and municipal governments in the state. This variable is included to control for the effects of horizontal competition among general purpose local governments (Campbell 2004, Sjoquist 1982, Turnbull and Niho 1986) as well as possible local jurisdiction scale effects (Zax 1989). One of the conclusions from Zax (1989) is that by including both a decentralization measure and fragmentation measure implies that the fragmentation variable will be isolating the effect of smaller local governments. A positive relationship here is consistent with economies of scale, while a negative relationship indicates diseconomies of scale. This could be one of those unique local factors influencing the local competitive effect employment growth and so is included in the model.

The models also include other common variables, such as the share of intergovernmental grants to total state expenditures (Intergovernmental Grants) and socio-economic characteristics for the state which include state population (Population), median household income and percent of population living in MSA's (Percent Urban Population).

All models include year fixed-effects and a South dummy variable for Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, North Carolina, South Carolina, Tennessee, Texas, and West Virginia. With respect to the latter variable, the constitutions and bodies of property law of these southern states have historical roots that differ from other regions in the US, as reflected in their participation in the Confederacy or as border states (Holcombe 1992) and the subsequent experiences

of reconstruction and resettlement. In addition, many parts of the region experienced Federal supervision of political and legal institutions for various periods. The inclusion of South fixed effects in the empirical models allows for the possibility that these events may have legacy effects on employment growth through a lasting influence on legal and political institutions.

#### **4. Empirical Results**

The empirical procedure begins with the application of shift-share analysis to isolate the composition of long run employment growth in states for the pre-*Kelo* periods 1980-1990 and 1990-2000. Table 4 reports the OLS results for the three models examining whether allowing the broad use of power for eminent domain actually increases economic development in states. Table 5 reports the results for the IV approach adopted to purge the effects of possible endogeneity in the eminent domain variable.

The two separate measures of fiscal decentralization used in this study (Own Source Revenue and Expenditures) are positive and significant in both Tables 4 and 5 for model A (in which the dependent variable is change in total private employment). This suggests that states with greater decentralization in their government structure experience higher levels of private sector employment growth than states that are more centralized. This is consistent with Oates's (1985) rationale that increased decentralization leads to greater economic efficiency and responsiveness to local demands. In models B and C (in which the dependent variables are the local competitive employment effect and the export base local competitive effect, respectively) reported in Tables 4 and 5, the effect of fiscal

decentralization is negative, but insignificant. Thus, the effect of fiscal decentralization on the local competitive effect is undetermined.

Local public sector fragmentation has a negative and significant impact on the total employment change (model A) as reported in Tables 4 and 5. As Zax (1989) points out, by including measures of fiscal decentralization along with fragmentation, the fragmentation variable will capture the effect of smaller local governments. Following this argument, a negative coefficient estimate is consistent with diseconomies of scale at the local level. Interestingly, the sign changes to positive for model B with the local competitive effect as the dependent variable – which is intuitively appealing, as greater fragmentation may represent economies of scale at the local level and function as a source of a location-specific advantage to industries in that state. The effect however, is below the 10% level of significance in the models B (local competitive effect) and C (export base local competitive effect) in Tables 4 and 5.

Intergovernmental grants exhibit a positive and significant coefficient in both regression models, which is consistent with the public choice rationale for fiscal decentralization; assigning more government spending decisions to lower level governments increases the efficiency of the political market and increases private sector employment growth. The South variable is positive in models B and C for tables 4 and 5, indicating that there may be some location specific advantage for Southern states, perhaps weaker labor laws or other legacy effects of the unique historical development of their property laws and institutions. This variable, however, struggles to remain at the 10% level of significance across all of the models. The other variables: percent urban

population, population, and median household income are not significant in the regressions.

Turning to the key variable of interest, previous research shows that allowing eminent domain for economic development leads to public sector growth (Turnbull and Salvino 2009). The OLS estimates in Table 4 indicate that this is not the case for the private sector. The eminent domain effect is negative and significant at the 5% level for both model A specifications. It appears that states that expressly allow the use of eminent domain for economic development tend to experience slower growth or a more rapid decline in private sector employment than states that do not expressly allow eminent domain for economic development.

Recall that models B and C in the table control for the national trend and industry mix effect on employment growth by focusing just on the local competitive employment effect derived from the shift-share model. The magnitude of the coefficient estimates here are much higher than in model A, but remember that the dependent variables in these models are defined as a proportion of total employment change and so are not directly comparable to the corresponding coefficient in model A. Nonetheless, the eminent domain coefficient estimates are significantly negative for all versions of these models. Eminent domain power appears to be a significant regional factor adversely affecting private employment growth when controlling for national trends and the state industry mix.

Model C focuses more narrowly on the local competitive employment effect in only those industries that are in the state export base. The significant negative eminent domain effect found here indicates that even those industries that we expect to be primary

targets of state and local economic development policies are adversely affected by the property rights regime. Given the multiplier effects normally associated with export base industries, slower growth or more rapid decline in the export base sector has magnified effects on total employment. Thus, the negative effect of this policy on total employment is greater than indicated by the coefficient in model C.

Similar point estimates for eminent domain are reported for the IV estimates in Table 5. The total employment effect in model A remains significant. The local competitive employment effect in model B and the export base LCE in model C, however, fail to attain even the 10% level of significance.

Recall that models B and C reported in tables 4 and 5 measure the local competitive effect and export base local competitive effect as a proportion of the total employment change. Tables 6 and 7 report the results from re-estimating models B and C as models D and E using the unit change in employment measured by the local competitive effect and export base local competitive effect as dependent variables, respectively. The point estimates are all negative—as when the LCE and export base LCE are deflated by total job growth. The significance pattern, however, is opposite that reported in tables 4 and 5. In these models, the OLS estimates are negative but insignificant but the IV estimates are now negative and significant. The adjusted  $R^2$ 's range from 24-29% for the re-estimated OLS models and 36-43% for re-estimated IV models. The significance pattern is consistent with the notion that states with larger LCE and export base LCE may be more inclined to expressly allow local governments to use eminent domain for economic development purposes. The resultant positive relationship running from job growth to eminent domain policy offsets the negative direct effect of

the eminent domain policy on job growth (as measured by the IV coefficients in the LCE and export base LCE models), leading to the insignificant coefficients in the OLS models.

Nonetheless, while the statistical significance may vary across models and estimation methods, all of the results are consistent in that they provide absolutely no evidence that expressly allowing local governments to use eminent domain for economic development accomplishes that goal. Allowing eminent domain for economic development does not increase private employment as measured by the local competitive effect or the export base local competitive effect, a conclusion consistent with that for total private sector employment where the OLS and IV evidence clearly shows that allowing eminent domain for economic development leads to slower private sector job growth.

## **5. Conclusion**

State and local governments are finally recovering from the great recession, dealing with decreases in spending, finding new revenue sources and trying new ways to promote economic growth and increase employment. The *Kelo* decision upholds the use of eminent domain for the purpose of economic development in those states choosing to so empower their local governments. This study examined the relationship between eminent domain and private sector employment growth, a comprehensive measure of successful economic development.

The analysis examined both state total private sector employment growth and a decomposition of state employment growth using shift-share analysis. This approach

allowed us to conveniently separate employment growth arising from national trends and industry mix from employment growth arising from state-specific factors, including fiscal structure and private property rights. OLS and IV models examined how expressly allowing *Kelo*-type eminent domain influences total private sector employment and the local competitive employment effect.

The results reveal a negative and significant relationship between the use of eminent domain for private economic development and the growth in private sector employment across alternative models and OLS and IV approaches. States allowing local governments liberal use of eminent domain do not experience stronger private sector employment growth; in fact, it appears they experience weaker growth overall. The OLS analysis of local competitive employment effects as a proportion of total job growth reveals significantly negative eminent domain effects on employment; the IV estimates are negative, but not significant. Looking at the local competitive employment effects not deflated by total job growth, the OLS results yield insignificant negative eminent domain effects while the IV estimates yield highly significant negative eminent domain effects on private sector employment. Across all models the point estimates also indicate stronger eminent domain effects for export-based industries, the types of industries heavily favored in traditional economic development recruitment. Perhaps as a consequence of weakening property rights and passing more control over privately owned resources to state and local governments, it turns out that one of the fundamental concerns of state and local governments—promoting job creation—does not appear to be well served by broad eminent domain powers.

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Table 1. Independent variable definitions and sources

<b>Variable Name</b>	<b>Variable Definition</b>	<b>Source</b>
<i>Eminent Domain</i>	state expressly allows eminent domain for economic development	Compiled by the authors
<i>Expenditure Decentralization</i>	share of local expenditures in total state and local spending	Computed from <i>Census of Governments</i>
<i>Own Source Revenue Decentralization</i>	share of local own source revenue in total state and local own source revenue	Computed from <i>Census of Governments</i>
<i>Intergovernmental Grants</i>	dollar value of grants to local governments as a share of state spending	Computed from <i>Census of Governments</i>
<i>Fragmentation</i>	total number of all local government units in a state	<i>Census of Governments: Vol.1, No. 2, Individual State Descriptions</i>
<i>Population</i>	state population	<i>Census of Population</i>
<i>Percent Urban Population</i>	share of population in MSAs	<i>Census Tiger Database</i>
<i>Median Household Income</i>	median household income (1989 \$)	<i>Census SF3</i>
<i>South</i>	state is a former Confederate or border state: AL, AR, FL, GA, KY, LA, MD, MS, MO, NC, SC, TN, TX, WV	Compiled by the authors

Table 2. Dependent and instrumental variables definitions and sources

<b>Variable Name</b>	<b>Variable Definition</b>	<b>Source</b>
<b>Dependent Variables</b>		
<i>Employment Change</i>	Percent growth in private sector employment from 1980-1990 and 1990-2000	<i>BEA SA25</i>
<i>Local Competitive Effect (LCE)</i>	$\sum_i n_i (g_i - G_i)$ as a share of total employment growth	Computed from shift-share
<i>Export Base LCE</i>	For $LQ > 1$ , LCE as a share of total private employment growth	Computed from shift-share
<b>Instrumental Variables</b>		
<i>Lawyers per 1,000</i>	Lawyers per 1,000 population in state	<i>Census and Census Equal Employment Opportunity File</i>
<i>Percent State-Owned</i>	Percent of state land area owned by state government	<i>National Wilderness Institute, 1995</i>
<i>Incomeskew</i>	State median household income divided by average household income	<i>Census SF3</i>

Table 3: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Total Employment Change (A)	94	0.24	0.14	-0.07	0.69
Local Competitive Effect/Total Employment Change (B)	94	-0.27	3.53	-27.90	17.67
Local Competitive Effect (Loq. Quotients > 1)/Total Employment Change (C)	94	-0.19	2.95	-23.96	14.14
Eminent Domain	94	0.15	0.36	0.00	1.00
Own Source Revenue Decent.	94	0.41	0.08	0.20	0.55
Expenditure Decent.	94	0.51	0.08	0.32	0.65
Fragmentation	94	1,829	1,500	119	6,835
Intergovernmental Grants	94	0.25	0.06	0.09	0.41
Percent Urban Population	94	0.69	0.21	0.24	1.00
Population	94	5,448,832	5,921,957	453,588	33,900,000
Median Household Income	94	\$ 29,465	\$ 4,939	\$ 20,136	\$ 41,721
South	94	0.30	0.46	0.00	1.00
Year 2000	94	0.50	0.50	0.00	1.00
Lawyers per 1,000	94	2.63	0.86	1.48	5.46
Percent State-Owned	94	1.16	7.88	0.02	76.69
Incomeskew	94	0.78	0.03	0.70	0.86

Table 4: OLS estimates for models with dependent variables Percent Total Private Employment Change (Models A), Local Competitive Effect (Models B), and Export Base Competitive Effect (Models C).

	Models A		Models B		Models C	
<i>Eminent Domain</i>	-0.08 (2.31)	-0.07 (2.07)	-1.19 (2.16)	-1.25 (2.27)	-0.94 (2.08)	-0.99 (2.19)
<i>Own Source Revenue Decentralization</i>	0.41 (2.32)	---	-4.80 (1.04)	---	-3.99 (1.06)	---
<i>Expenditure Decentralization</i>	---	0.72 (3.11)	---	-5.57 (0.72)	---	-4.06 (0.72)
<i>Fragmentation(x10<sup>-4</sup>)</i>	-0.22 (2.40)	-0.27 (3.14)	1.80 (0.72)	1.99 (0.87)	1.45 (0.68)	1.59 (0.81)
<i>Intergovernmental Grants</i>	0.69 (2.26)	0.10 (0.26)	9.24 (2.29)	13.45 (1.62)	7.14 (2.12)	10.53 (1.57)
<i>% Urban Population</i>	0.21 (1.73)	0.19 (1.67)	-4.53 (1.38)	-4.45 (1.35)	-3.78 (1.36)	-3.72 (1.33)
<i>Population (x10<sup>-8</sup>)</i>	-0.49 (1.34)	-0.37 (0.96)	-4.14 (1.26)	-5.39 (1.68)	-3.04 (1.09)	-4.07 (1.50)
<i>Median Household Income (x10<sup>-4</sup>)</i>	-0.09 (0.18)	-0.13 (0.26)	2.13 (1.13)	2.11 (1.15)	17.60 (1.10)	17.50 (1.12)
<i>South</i>	< 0.01 (<0.01)	-0.01 (0.22)	0.97 (1.50)	0.98 (1.32)	0.71 (1.36)	0.71 (1.20)
<i>Year 2000</i>	0.03 (1.03)	0.04 (1.50)	0.07 (0.13)	-0.03 (0.05)	0.08 (0.18)	<-0.01 (<0.01)
<i>Constant</i>	-0.16 (1.16)	-0.20 (1.51)	-3.91 (0.85)	-3.99 (0.75)	-3.07 (0.79)	-3.16 (0.71)
Adj. R <sup>2</sup>	0.28	0.31	0.06	0.06	0.06	0.06
Observations	94	94	94	94	94	94

Note: Absolute value of t-statistics in parentheses, using White's heteroscedastic robust standard errors.

Table 5: IV estimates for eminent domain for models with dependent variables Percent Total Private Employment Change (Models A), Local Competitive Effect (Models B), and Export Base Competitive Effect (Models C).

	Models A		Models B		Models C	
<i>Eminent Domain</i>	-0.18 (3.23)	-0.16 (2.82)	-2.27 (1.55)	-2.43 (1.70)	-1.79 (1.43)	-1.91 (1.56)
<i>Own Source Revenue Decentralization</i>	0.41 (2.29)	---	-4.89 (1.05)	---	-4.07 (1.07)	---
<i>Expenditure Decentralization</i>	---	0.69 (3.07)	---	-5.99 (0.77)	---	-4.84 (0.77)
<i>Fragmentation(x10<sup>-4</sup>)</i>	-0.14 (1.42)	-0.19 (2.12)	2.71 (0.83)	3.03 (1.00)	2.16 (0.78)	2.41 (0.93)
<i>Intergovernmental Grants</i>	0.74 (2.43)	0.17 (0.45)	9.77 (2.26)	14.40 (1.70)	7.56 (2.09)	11.27 (1.64)
<i>% Urban Population</i>	0.19 (1.55)	0.17 (1.51)	-4.78 (1.38)	-4.70 (1.35)	-3.98 (1.35)	-3.92 (1.32)
<i>Population (x10<sup>-8</sup>)</i>	-0.61 (1.79)	-0.48 (1.34)	-5.48 (1.26)	-6.90 (1.59)	-4.09 (1.11)	-5.25 (1.42)
<i>Median Household Income (x10<sup>-4</sup>)</i>	-0.03 (0.53)	-0.02 (0.40)	2.52 (1.13)	2.55 (1.17)	2.07 (1.10)	2.09 (1.12)
<i>South</i>	0.01 (0.32)	<0.01 (0.09)	1.08 (1.77)	1.10 (1.57)	0.79 (1.61)	0.81 (1.43)
<i>Year 2000</i>	0.02 (0.83)	0.04 (1.32)	<0.01 (0.01)	-0.11 (0.18)	0.03 (0.07)	-0.06 (0.13)
<i>Constant</i>	-0.25 (1.77)	-0.28 (1.98)	-4.93 (0.90)	-5.05 (0.83)	-3.87 (0.84)	-3.99 (0.78)
Adj. R <sup>2</sup>	0.31	0.33	0.06	0.06	0.06	0.06
Observations	94	94	94	94	94	94

Note: Absolute value of t-statistics in parentheses, using White's heteroscedastic robust standard errors.

Table 6: OLS estimates for models with dependent variables Local Competitive Effect (Models D) and Export Base Competitive Effect (Models E) – both unscaled by the change in total employment.

	Models D		Models E	
<i>Eminent Domain</i> (x10 <sup>3</sup> )	-226.2 (1.57)	-214.6 (1.44)	-169.8 (1.51)	-160.0 (1.38)
<i>Own Source Revenue Decentralization</i> (x10 <sup>3</sup> )	482.8 (1.12)	--- (1.12)	396.6 (1.12)	--- (1.12)
<i>Expenditure Decentralization</i> (x10 <sup>3</sup> )	---	1,092 (1.88)	---	920.2 (1.92)
<i>Fragmentation</i>	-24.41 (0.70)	-33.86 (0.97)	-26.62 (0.91)	-34.71 (1.20)
<i>Intergovernmental Grants</i> (x10 <sup>3</sup> )	1,131 (2.10)	203.7 (0.23)	826.6 (1.83)	42.74 (0.06)
<i>% Urban Population</i> (x10 <sup>3</sup> )	49.43 (0.27)	22.06 (0.12)	21.44 (0.14)	-1.89 (0.01)
<i>Population</i>	-0.02 (1.05)	-0.02 (0.94)	-0.01 (0.64)	-0.01 (0.54)
<i>Median Household Income</i>	3.09 (0.25)	2.25 (0.18)	2.28 (0.23)	1.54 (0.16)
<i>South</i> (x10 <sup>3</sup> )	221.7 (2.43)	207.2 (2.27)	162.2 (2.25)	149.7 (2.08)
<i>Year 2000</i> (x10 <sup>3</sup> )	40.53 (0.63)	62.51 (0.89)	37.03 (0.71)	55.62 (0.96)
<i>Constant</i> (x10 <sup>3</sup> )	-530.9 (1.86)	-621.4 (2.22)	-392.9 (1.72)	-471.6 (2.11)
Adj. R <sup>2</sup>	0.28	0.29	0.24	0.26
Observations	94	94	94	94

Note: Absolute value of t-statistics in parentheses, using White's heteroscedastic robust standard errors.

Table 7: IV estimates for models with dependent variables Local Competitive Effect (Models D) and Export Base Competitive Effect (Models E) – both unscaled by the change in total employment.

	Models D		Models E	
<i>Eminent Domain</i> (x10 <sup>3</sup> )	-783.8 (3.14)	-759.5 (2.90)	-573.3 (2.82)	-551.9 (2.58)
<i>Own Source Revenue Decentralization</i> (x10 <sup>3</sup> )	449.1 (1.18)	--- (---)	371.9 (1.16)	--- (---)
<i>Expenditure Decentralization</i> (x10 <sup>3</sup> )	---	877.8 (1.66)	---	766.7 (1.71)
<i>Fragmentation</i>	21.30 (0.61)	13.22 (0.37)	6.49 (0.22)	-0.83 (0.03)
<i>Intergovernmental Grants</i> (x10 <sup>3</sup> )	1,365 (2.47)	626.4 (0.74)	996.6 (2.16)	347.0 (0.48)
<i>% Urban Population</i> (x10 <sup>3</sup> )	-88.2 (0.45)	-105.1 (0.55)	-78.0 (0.46)	-93.2 (0.57)
<i>Population</i>	-0.02 (1.67)	-0.02 (1.54)	-0.01 (1.10)	-0.01 (0.99)
<i>Median Household Income</i>	23.86 (1.76)	22.77 (1.62)	17.31 (1.54)	16.30 (1.40)
<i>South</i> (x10 <sup>3</sup> )	279.4 (3.59)	267.9 (3.39)	204.0 (3.17)	193.4 (2.96)
<i>Year 2000</i> (x10 <sup>3</sup> )	7.62 (0.13)	25.9 (0.40)	13.2 (0.27)	29.2 (0.53)
<i>Constant</i> (x10 <sup>3</sup> )	-1,053 (3.38)	-1,097 (3.55)	-770.6 (3.02)	-813.9 (3.22)
Adj. R <sup>2</sup>	0.42	0.43	0.36	0.37
Observations	94	94	94	94

Note: Absolute value of t-statistics in parentheses, using White's heteroscedastic robust standard errors.