 DOES INCREASING AVAILABLE NON-TAX ECONOMIC DEVELOPMENT INCENTIVES RESULT IN MORE JOBS?

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This paper examines the job creation effects of state and local non-tax incentives for capital investment, which are relatively understudied in the literature. The paper’s primary contribution is the creation of an Incentive Environment Index (IEI) from state constitutional provisions that limit and structure the ability of state and local governmental entities to aid private enterprises. Comparing estimation results across methods reveals that unobserved heterogeneity results in overstatement of policy effects. The most robust estimates indicate that increasing the ability of governments to aid private enterprise has a significant negative medium-term effect on rural county employment levels but otherwise has no effect on employment levels or growth.

Keywords: economic development, incentives, state and local policy, business subsidies, intergovernmental competition

JEL Codes: H25, H71, R58

I. INTRODUCTION

Policy-makers and elected officials are constantly under pressure to enact policies supporting local economic growth, and such pressure has only intensified in the sluggish labor market since 2007. Economic development programs aimed at job creation are the frequent response. Limited federal resources coupled with a slow return to full employment in the United States will likely increase demands for state and local officials “to do something about jobs” (Bartik 2012, p. 545). Often local job creation policies focus on increasing capital through economic development incentives, with the expectation that job creation, higher earnings, and tax revenues will follow new capital formation. Although the academic literature on economic development incentives is vast, it does not provide clear guidance on the effectiveness of these policies.

Myriad methodologies, programs, and results make it difficult for interested officials to determine best practices from the literature. Results also compete with constituent
pressures to “do something,” as well as with rent-seeking business interests asserting the need for subsidies (Greenbaum and Landers, 2009). Further, most studies focus only on tax incentives, a specific program, or a single location. Economic development incentives packages are bundles of tax and non-tax incentives, with non-tax incentives often comprising the majority of a package. A recent survey of state economic development programs finds that “the percentage of businesses receiving more than $50,000 through non-tax programs significantly exceeded that percentage for tax programs” (Council for Community and Economic Research, 2013, p.19). Research suggests these incentives may comprise three-quarters of state and local resources devoted to economic development (Bartik, Erickcek, and Eisinger, 2003).1 Further, discretionary and non-tax incentives are at the forefront of public debate because this type of incentive (cash and near-cash grants, low-interest financing, free land and buildings, etc.) looks the most like legalized bribery of companies (Bartik, 2005). Yet, the academic literature has relatively little to contribute to the debate, with the majority of research focused on taxes. This paper contributes to the debate by using heretofore untapped variation in structural constraints on state and local non-tax incentives to examine their job creation effects, a topic that is relatively understudied in the literature.

Attempts to study the effects of non-tax incentives have been hampered by limited data availability and methodological problems. Since the policy response to pressure to create jobs is often to create programs and spend more, it is difficult to separate their actual effects on jobs from the simultaneous effect that local employment conditions have on programs and expenditure. Programmatic and spending measures of economic development incentives also confound the effects of different program types. Reliance on state-level, cross-section analysis in the literature has problems as well, as state-level analysis masks certain subtleties in local effects and unobserved heterogeneity may lead to misleading results.

Constrained by these issues, the existing literature does not provide a clear answer to some important policy questions regarding economic development incentives. The research presented in this paper provides insights into one important yet unresolved question: does increasing the availability of public aid to private enterprise support local employment growth? I construct a theoretical model of local employment with incentives that guides the empirical implementation. Utilizing panel data from 1970–2002, I use random trend models to estimate U.S. county employment level and growth effects. The random trend model includes time-invariant county unobservables as well as county-specific trends and is implemented by fixed effects estimation of the first-differenced equation. This method removes county and state sources of level and trend heterogeneity, including state and county unobservables correlated with the policy of interest. To strengthen identification, I employ a natural experiment methodology by

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1 For a recent database of state and local spending on business incentives, see “The United States of Subsidies,” The New York Times, http://www.nytimes.com/interactive/2012/12/01/us/government-incentives.html. According to the database, state and local governments spent $1.75 billion on cash grants, loans, or loan guarantees for private businesses in the United States between 2010 and 2012. Unfortunately, the database does not include information on all types of non-tax incentives that are the focus of this paper.
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restricting the sample to counties that share a state border. As demonstrated by the growing literature following Holmes (1998), restricting analysis to locations on either side of a policy border further removes local sources of level and trend heterogeneity. Together, the random trend and border county methods control very well for unobserved spatial variation in levels and trends.

One of the paper’s main contributions to the literature is the creation of an Incentive Environment Index (IEI). The IEI is created from state constitutional provisions that limit and structure the ability of state and local governmental entities to aid private enterprises. Every state constitution was analyzed and scored to construct an IEI for each continental U.S. state and year 1970–2000. Most provisions originated in the 19th century as a response to state and local financial crises caused by participation in risky economic development projects. The provisions are remarkably heterogeneous. They typically act as a constraint on both the type of incentives available in a location and the nature of the policy response to changes in economic conditions. For the purposes of estimation, they provide structural variation in state and local economic development incentives policy. Identification comes from differences in constitutional provisions and the very low probability that employment expectations in a given county exert a significant influence on state level constitutional change. Hence, these constraints may limit the ability of governments to provide needed incentives — limiting growth — or allow governments to credibly argue that they cannot offer incentives, which reduces the possibility that they will offer wasteful incentives. The IEI provides a better measure of the availability of state and local non-tax incentives for capital than existing measures that do not account for the dynamics of incentives competition, do not contain data on local incentives, and include data on tax incentives and on workforce, marketing, and other activities not related to capital incentives.

Employing the IEI in annual and five-year county panels provides several interesting results for policymakers and researchers considering economic development incentives. Comparing estimation results across methods and panels reveals that unobserved heterogeneity contaminates estimates of policy effects. Further, constraining urban and rural counties to have the same employment process masks differences in policy effects in the presence of agglomeration economies and forward and backward linkages. The most robust estimates indicate that increasing the ability of governments to aid private enterprise has a significant negative medium-term effect on rural county employment levels but no significant effects otherwise. Urban counties may reap some short-term level benefits, but these effects are statistically indistinguishable from zero in the medium-term and when the sample is restricted to border counties.

The results call into question the policy of subsidizing capital in order to create jobs. The IEI measures the ability of government to use public monies, credit, and property to aid private enterprises. It is not a measure of other types of economic development programming, such as human capital, amenities, tax breaks, or direct jobs programs. However, the availability of programs like industrial revenue bonds, venture capital funds, loan guarantee program, etc., is directly governed by these constitutional provisions. It is not clear whether these incentives do not result in net new capital or whether
net new capital does not create new jobs. The paper’s findings suggest that creating more tools for governments to aid private capital is an ineffective local job creation policy.

The paper proceeds in Section II by discussing key findings in the literature. Section III sketches a theoretical model of local employment with incentives, and Section IV describes the empirical strategy. Section V explains the IEI in more detail, and the estimation results are presented in Section VI. Section VII discusses the results, and some concluding remarks are provided in Section VIII.

II. BACKGROUND

After decades of research, there is no clear consensus on the effects of economic development incentives competition (see the surveys by Thomas (2011), Glaeser (2001), and Bartik (1991)). Data and methodological issues are pervasive in the literature. Many studies focus only on tax incentives. Existing studies of the employment effects of non-tax incentives tend to restrict their attention to a specific location, one program, or establishment impacts.

Comprehensive data are not available, although some evidence suggests state and local governments devote a significant portion of their resources to non-tax incentives. For example, Morgan (2009) estimates the combined value of state and local incentives for Google’s selection of North Carolina for their new data center at $262 million. The tax incentive portions of the package (credits, exemptions, and refunds) totaled $91.6 million compared to the $170 million grant portion of the package. Analyzing the incentive packages contained in the Good Jobs First Megadeals subsidy database from 1985–2000, the reported subsidy value of the non-tax incentives were 1.7 times greater than the tax portions. It is possible that large deals include more non-tax incentives than the typical incentive package. Comparing data from state tax expenditure reports with standardized state economic development spending data, the relative magnitudes vary depending upon the definition of economic development incentives.

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2 See “Megadeals,” Good Jobs First, http://www.goodjobsfirst.org/megadeals. The total value of non-tax incentives was $2.93 billion compared to $1.75 billion for tax incentives based upon the author’s calculations. These values are exclusive of worker training incentives when possible. Another $95 million was classified as “other” incentives, which were unspecified in the source data. This analysis is available upon request from the author.

3 The tax expenditure data were compiled from state tax expenditure reports covering primarily income, sales and use, and property tax expenditures. Four states do not produce tax expenditure reports. Standardized state economic development spending data were provided by the Council for Community and Economic Research. Note that that budgetary spending does not capture the bonds, bond-financed incentives, and other non-tax and discretionary economic development incentives funded outside the general revenue budget. The debt service obligation for economic development incentives funded through general obligation bonds can also represent a substantial liability for state and local governments. For example, South Carolina’s debt service obligation for economic development bonds totaled over $69 million for FY2010–2011 (Rainwater et al., 2013). Under the narrowest definition of economic development incentive tax expenditure, budgetary spending exceeds tax expenditures. However, state tax expenditures significantly exceed budgetary spending under the broadest definition of economic development tax expenditures. See the online appendix, https://sites.google.com/site/carliannepatrick/research, for additional detail and discussion.
Although non-tax incentives are relatively understudied, the literature on tax competition and tax incentives is voluminous. Reviews by Wasyleenko (1997), Netzer (1997), Fisher (1997), Buss (2001), and Bartik (2005) highlight important methodological, causality, and data issues that must also be considered in the study of non-tax and discretionary incentives. The authors note that most research on tax incentives fails to control for the level of public services and expenditures associated with the tax levels. They indicate that the failure to account for other growth factors, such as agglomeration economies, is also a problem. Tax research tends to rely on static models without accounting for time dynamics and path dependency. These issues suggest that existing estimates of the relationship between taxes and growth are suspect.

The authors of these reviews note that the more careful studies tend to find small or insignificant effects, perhaps due to the dynamics of tax competition. Competition causes states to replicate programs in other states. As a result, each state now offers a variation of almost every possible tax incentive (Chi and Leatherby, 1997; Buss, 2001). In fact, Papke (1995) found that after-tax returns on investment were so similar in six Great Lake states that one could not be preferred. At the county level, Agrawal (2013) finds that local sales tax competition erodes state sales tax differentials at state borders.

The few studies of non-tax incentives and aggregate level employment effects focus on state level analysis and use spending or program measures of incentives. For example, de Bartolomé and Spiegel (1995) and Goss and Phillips (1997) find a positive relationship between state economic development spending and job growth. As will be argued below, spending is likely endogenous and includes programs which fall outside the capital creating jobs paradigm.

Rather than spending, Ó hUallacháin and Satterthwaite (1992) use tax rate measures, industrial revenue bond (IRB) financing, and program dummy variables. They find that only enterprise zones and university research parks have a positive statistical relationship with employment growth. Recognizing the limitations of their empirical approach, Ó hUallacháin and Satterthwaite are careful not to claim causation. Goetz et al. (2011) create measures of the share of all possible programs available in states. They find tax incentive and financial assistance programs may harm growth rather than promote it.

As critics of tax studies have also argued, non-tax incentive studies that do not control for other employment growth factors and time dynamics may yield questionable results. With a few notable exceptions, job incentive studies lack a theoretical underpinning to adequately control for other job creation mechanisms. Goetz et al. (2011) root their work in spatial equilibrium theory, which explicitly considers the locational effects of policy. However, they do not fully specify their model. de Bartolomé and Spiegel (1997) do fully develop a model of equilibrium labor change under incentives. The model is in the static neo-classical growth tradition, though, and does not consider any feedback effects. The de Bartolomé and Spiegel (1997) model does illustrate the general motivation for non-tax and discretionary incentives aimed at capital — to increase employment in a location by increasing the amount of capital.

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4 See Wilson (1999) for a thorough review of tax competition models.
Whether explicitly stated or not, the premise underlying many non-tax and discretionary incentives is that increasing the amount of capital in a location will increase employment. Neo-classical, spatial equilibrium, tax competition, and agglomeration theories for incentives share this foundation. Eisinger (1988) discusses rapid universal acceptance of this model. The fundamental rationale of inducing private capital investment to create jobs and wealth is largely unquestioned. He describes the public and private benefit models in some detail. Updating his public and private benefit relationships with more recent insights, the general concept behind capital incentives is illustrated in Figure 1.

The hope is that induced increases in capital are greater than any decreases due to factors such as capital displacement, revenue shortfalls, and the costs of providing incentives to capital that would have been located in the jurisdiction without them. As Figure 1 shows, the model implicitly assumes increasing capital will result in new jobs and payroll. However, increasing capital may have productivity effects reflected in wage increases. As discussed below, the relative slopes of the labor supply and demand curves determine which, if any, of these effects actually occur (in the absence of tax and service effects).

Figure 1 is merely a summary of the generally accepted conceptual framework for analyzing economic development incentives. It is not a theoretical model upon which an empirical strategy can be based. The next section sketches such a model.

**III. THEORETICAL MODEL OF LOCAL EMPLOYMENT WITH INCENTIVES**

This section describes the key insights from a theoretical model of employment location choice. (See the online appendix for additional details.) Equilibrium employment
results from firm and individual optimization of the expected future value of employment location decisions.

A. Spatial Equilibrium with Incentives

Consider a spatial equilibrium model in the spirit of Roback (1982), Gyourko and Tracy (1989), and Brown, Hayes, and Taylor (2003). Assume that an individual’s utility is defined over the quantity of goods consumed \( q \), the quantity of land consumed \( N \), the quantity of labor supplied \( L_{hj} \), the natural amenities found in the jurisdiction \( j \) described by a vector \( A_j \), and state and local government services \( G_j \).

Assume individuals choose \( L_{hj} \), the labor they supply in location \( j \) given the wage rate \( w_j \), taxes \( t_j \), amenities, and public services in location \( j \), to maximize utility subject to their budget constraint. Further assume that individuals are forward looking and choose a sequence \( \{L_{hjt}\}_{t=0}^{\infty} \) that maximizes

\[
E \left\{ \sum_{t=0}^{\infty} \varphi^t \left[ \tilde{V}(w_{jt}L_{hjt}(1-\tau_{wjt}), Y(1-\tau_{yjt}), P(1+\tau_{pj}), n_j(1+\tau_{nj}), A_j, G_j, \epsilon_{jt}) \right] \right\},
\]

where \( \tilde{V}(\cdot) \) is the indirect utility function, \( w_j \) is the wage rate in jurisdiction \( j \). \( Y \) is non-labor income, \( P \) is the national price of good \( q \). \( \tau_{yj}, \tau_{nj}, \tau_{wjt} \) are the sales, land rental, wage, and nonlabor income tax rates imposed by state and local governments in \( j \), respectively. \( \varphi \) is a discount rate between zero and one, and \( \epsilon_{jt} \) is a time and location specific idiosyncratic shock. In the long run, labor mobility will guarantee constant, equal expected utility across locations.

Similarly, assume firms choose \( L_{fj} \), the amount of labor they employ in location \( j \) given the wage rate, taxes, amenities, public services, regulatory environment, and incentives environment in location \( j \). Further assume that firms make forward looking production decisions when choosing labor locations (and thus the location of other factors of production). They thus choose a sequence \( \{L_{fjt}\}_{t=0}^{\infty} \) that maximizes

\[
E \left\{ \sum_{t=0}^{\infty} \varphi^t \left[ \tilde{\pi}(P, w_{jt}, r(1+\tau_{rjt}), n_j(1+\tau_{nj}), M_j, A_j, G_j, R_j, I_{fjt}, \epsilon_{jt}) \right] \right\},
\]

where \( \tilde{\pi}(\cdot) \) is the indirect profit function, \( r \) is the rate of return on capital, \( R_j \) is a vector of state and local regulation in jurisdiction \( j \), \( I_{fjt} \) is the value of incentives available to the firm in jurisdiction \( j \), and \( M_j \) represents potential agglomeration economies associated with industrial composition. In the long-run, free entry and exit of firms requires that economic profits be zero for all firms in all locations.

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5 The subscript \( h \) is used to differentiate labor supplied by individuals from the labor demanded by firms.

6 Given that \( L_{hj} \) equals one if the worker is employed and zero otherwise, the choice of \( L_{hj} \) is equivalent to choosing whether to work in location \( j \). (For details, see the online appendix, available at https://sites.google.com/site/carliannepatrick/research.)
Under assumptions similar to those in dynamic household sorting models such as Bayer et al. (2010) and the appropriate regularity conditions, the equilibrium level of employed labor in location $j$ at time $t$ is given by

$$L^*_jt = L^*_jt(S^*_jt; \gamma_jt, \epsilon_jt) = L^*_jt(\gamma_jt, S^*_jt-1, \epsilon_jt),$$

where $S^*_jt = (P, w_jt, \tau_{wjt}, \tau_{yjt}, \tau_{wjt}, \tau_{yjt}, M_{jt}, A_j, R_j, I_{jt}, G^*_jt)$ is a set of variables in location $j$ at time $t$ taken as given by households and firms as described above and $\gamma^*_jt$ is the trend growth rate of these variables in location $j$ at time $t$.

If we assume that $\gamma^*_jt$ can be disentangled from the trend growth rate, then we have equations for both employment level and growth rate that depend only on the previous period’s predetermined locational characteristics

$$\gamma^*_jt, 1 = \gamma^*_jt(S^*_jt-1, \epsilon_jt), 1 = \gamma^*_jt(S^*_jt-1, \epsilon_jt),$$

B. The Effect of Incentives

Non-tax and discretionary economic development incentives (cash and near-cash grants, low-interest financing, free land and buildings, etc.) effectively subsidize capital. As argued above, the idea is to increase capital in a location and thereby increase employed labor, earnings, and tax revenues. Consider a government in location $j$ that provides firms with public services $G_j$ and economic development incentives $I_j = \Sigma_j I_{jt}$, collect taxes $T_j = w_jL_j(1 - \tau_{wj}) + Y_j(1 - \tau_{yj}) + n_j(1 - \tau_{nj})N + P(1 - \tau_{pj})q_jH$, receives other sources of income $O_j$, and issues debt $D_j$. The government is subject to the period-by-period budget constraint $G_j + I_j \leq T_j + O_j + D_j$.

Assuming the government’s budget constraint is binding, any change in the non-tax and discretionary economic development incentives provided in location $j$ can be expected to induce changes in at least one of the other budget elements. Thus, it is reasonable to think of the employment effect of incentives as being the sum of its direct and indirect effects. Taking the total derivative of equilibrium employed labor with respect to incentives gives

$$\frac{dL^*_jt}{dI_{jt-1}} = \frac{\partial L^*_jt}{\partial I_{jt-1}}, \frac{\partial L^*_jt}{\partial w_{jt-1}}, \frac{\partial L^*_jt}{\partial t_{wjt-1}}, \frac{\partial L^*_jt}{\partial t_{yjt-1}}, \frac{\partial L^*_jt}{\partial \tau_{wjt-1}}, \frac{\partial L^*_jt}{\partial \tau_{yjt-1}}.$$

The first term in (5) is the amount of additional aggregate labor employed as a result of the capital attracted by incentives to location $j$. It is important to note that this is the

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7 Under this formulation, I assume the government administers policies on incentives, taxes, and services and does not have an objective function. Elected officials, economic development professionals, and other players determine policy according to their objective functions, which may include the desire to be re-elected, receive campaign contributions, and be more employable in future economic development positions. The government, firms, and individuals take those policies as given.
effect being estimated and not the total effect. In order to understand the total effect in location \( j \) of incentives provided by location \( j \), each term would need to be investigated. It is also important to note that (5) is not the total effect across all jurisdictions.\(^8\) Figure 2 illustrates the intuition behind (5).

Suppose that equilibrium employment in location \( j \) is given by point \( A \) in Figure 2. The accepted premise of incentives is that the induced capital will shift the aggregate labor demand schedule in location \( j \), say from \( L_{\text{frms}}(\cdot) \) to \( L'_{\text{frms}}(\cdot) \). At the pre-existing wage, firms demand labor at point \( B \). The second term in (5) is expected to be non-zero for two reasons. New capital should increase the marginal productivity of labor and put upward pressure on wages. If there are no indirect effects shifting the aggregate labor supply schedule, wages would also be expected to rise to achieve the new equilibrium at point \( A' \). Thus, if the tax and service indirect effects in (5) are neutral, the increase in equilibrium employment is less than the direct effect of incentives on employment holding wages constant.

Proponents of incentives argue that the increase in employment and wages will have positive effects on government tax revenues and service provision. If they are correct, firms and/or individuals will receive tax reductions or more or better services and thus will be willing to accept a lower before-tax wage rate. Supporters thus argue better government services and lower taxes will also shift the labor demand schedule to the right.

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\(^8\) In other words, (5) does not include negative effects in other jurisdictions caused by location \( j \)'s incentives, nor does it include the negative effects in location \( j \) caused by incentives in other locations.
Critics argue that incentives cost more government resources than they generate. Revenue shortfalls result in increased taxes or reduced public services. It has also been argued that the competition for capital results in public service reductions and that mobile capital implies that tax increases are borne by residents rather than firms. If this is the case, then revenue shortfalls would cause individuals to demand higher wages in location \( j \) and shift the labor supply schedule, say from \( L_{ij}(\cdot) \) to \( L'_{ij}(\cdot) \). The new equilibrium level of employment and wages would be point \( A'' \). Equilibrium employment increases in the figure, but by much less than the direct effect of incentives.

Figure 2 demonstrates that the overall change in equilibrium employment will depend on the signs and relative magnitudes of the terms in (5). The estimating equation includes measures for wages, taxes, industrial composition, services, and outstanding debt. Adequately controlling for the indirect effects should allow the direct effect to be isolated.

IV. EMPIRICAL IMPLEMENTATION

A. Empirical Model

Allowing for unobserved heterogeneity and macroeconomic time effects, (3) and (4) imply the following estimation model:

\[ E_j = c + g_j + X'_t \beta + t + \epsilon_{jt}, \]

where \( E_j \) is either employment level or growth in county \( j \) at time \( t \), \( c \) is a county fixed effect capturing the time-invariant components of (3) and (4), \( g_j \) is the county-specific trend, and \( X'_t \) denotes the time-varying components of (3) and (4) including the policy variable of interest. In the few aggregate incentives (and other policy) studies employing panel data, the standard approach is to ignore \( g_j \) and constrain the partial effect of unobservable location characteristics to be constant over time. Fixed effects estimation is then generally used to remove the time-invariant location unobservable. Wooldridge (2002) warns that if there is additional heterogeneity in the underlying trend, the strict exogeneity assumption required for consistent fixed effects estimation is violated. The proposed theoretical model clearly suggests underlying trends should be considered.

Estimating a random trend model considers this additional source of heterogeneity and allows county level and trend unobservables to be correlated with observables. Wooldridge (2005) demonstrates that implementing a random trend model by fixed effects estimation of the first differenced equation provides consistent estimates of the average population effect. Further, he shows that consistency requires fairly weak assumptions. Burge and Ihlanfeldt (2009) employ this method in a study of policy effects on county employment. Comparing their random trend model results with those from first differenced estimation, they show that employment regressions failing to control for both level and trend heterogeneity (and thus violating strict exogeneity) incorrectly estimate policy effects.

9 Specifically, \[ X_{jt, -1} \equiv (w_{jt-1}, \tau_{jt-1}, \tau_{jt-1}, \tau_{jt-1}, \tau_{jt-1}, \tau_{jt-1}, M_{jt-1}, I_{jt-1}, G_{jt-1}). \]
Building upon Holmes (1998), the growing border methodology literature also demonstrates that failure to control for level and trend heterogeneity can lead to biased estimates of policy effects (Dube, Lester, and Reich 2010; Huang 2008; Kahn and Mansur 2013; Thompson and Rohlin, 2012). Variations of the border methodology exploit spatial correlation between counties across policy borders to solve the identification problem. As Rohlin (2011) notes, Holmes’ border method controls for natural advantages, access to labor pools, and other sources of unobserved heterogeneity. Dube, Lester, and Reich (2010) demonstrate that traditional estimates are contaminated by spatially, heterogeneous pre-existing trends. However, reducing the sample to border counties and allowing for spatially-varying trends controls for this source of contamination. As discussed below, the incentives measure for each county is derived from state constitutions. Thus, there is a distinct jump in policy at state borders. Restricting the sample to only those counties adjacent to a shared state border should significantly reduce any remaining identification concerns.\footnote{Although the border approach removes identifications concerns associated with unobservable heterogeneity, it might introduce concerns about local versus global effects. The paper’s main finding of significant negative, medium-term effects on rural county employment is qualitatively unchanged when the sample is restricted to rural border counties.}

The estimation proceeds in steps. First, the first difference model \( \Delta E_{jt} = \Delta X'_{jt} \beta + \Delta d_t + \Delta \epsilon_{jt} \) is estimated with the full county panels described below. Note that first differencing removes county and state fixed effects. State policies that have a common effect across all counties in the state, such as favorable regulatory environments, are differenced out. The first difference model is then estimated with border county panels.

Next, a random trend model is implemented by fixed effects estimation of the first difference model. Full and border county panel first difference estimates are compared to full and border county random trend estimates to determine the extent of unobserved heterogeneity bias. Again, the random trend method removes county and state sources of level and trend heterogeneity. This procedure should remove any state and county unobservables correlated with the policy of interest. The border restriction removes any remaining unobservable heterogeneity. Finally, separate random trend estimates for urban and rural counties provide the paper’s primary findings.\footnote{County rural/urban status is based upon the USDA Economic Research Service Rural-Urban Continuum Codes 1974 classification, with urban defined as codes 1–3.}

**B. Data**

The panel data for counties in the lower 48 U.S. states were obtained from the U.S. Bureau of Economic Analysis (BEA) and the U.S. Census Bureau, as described below. Two base panels capture potential differences in short and medium-term effects. The covered periods are determined by availability of data. The annual panel consists of yearly data from 1970–2000. The five-year panel covers 1972–2002.
Using counties as the unit of analysis makes institutional sense and provides several advantages. Counties better approximate local economies than states. They also avoid the selection and endogeneity issues associated with using cities or metropolitan areas (Beeson, DeJong, and Troesken, 2001). Counties are defined for all areas, with relatively stable boundaries over time independent of pre-existing economic activity. County-level analysis includes potentially affected areas outside city and metropolitan area boundaries. The political, financial, and service relationship between counties and lower levels of government also suggests counties are the appropriate unit of analysis. For example, a firm locating in City X in County Y utilizes public services, pays taxes, and receives incentives from City X and County Y. Economic development incentive packages, in particular, are typically joint packages, involving the city, county, and state. Evaluating state constitutional changes with county-level data retains this important link.

BEA annual wage and salary employment is the dependent variable measuring location \( j \)'s equilibrium employment level in both panels.\(^1\) Employment growth is the percent change in wage and salary employment from time \( t-1 \) to time \( t \). For the five-year panel, the difference between time \( t-1 \) and time \( t \) is five years.

The independent variables for \( w \) and \( M \) are also constructed from BEA annual data. Wage and salary earnings per employed person measure wages. As noted above, \( M \) reflects potential agglomeration economies associated with industrial composition. Manufacturing, farm, service, finance, insurance, and real estate, and military sector shares are calculated by dividing the total number of employees in each Standard Industrial Classification (SIC) industry by total county employment.

Local government taxes and services are clearly important in the theoretical context of spatial equilibrium with incentives. In practice, the tax rates and services in a location are determined by state government policy as well as local government policy. Thus, state and local measures are created for the tax and service variables. State variables are calculated from the U.S. Census Bureau State Tax Collections and State Government Finance data, as well as BEA State Gross Domestic Product estimates. County variables are calculated using data on all government units within the county from the U.S. Census Bureau’s Annual Survey of Governments (ASG) and Census of Governments (CG), which induces variation in the county coverage between the annual and five-year panels.\(^1\)

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1. BEA local area income and employment data are available at http://www.bea.gov/regional/downloadzip.cfm. Note that BEA does not provide data by SIC industry past 2000. However, the five-year panel estimation is not affected by the lack of post-2000 SIC data. The last period in the five-year panel is \( T=2002 \). The dependent variable is not affected by industrial classification. The independent variables for the final time period in the five-year panel are from \( T-1=1997 \). Thus, SIC data is available from the BEA for all time periods.

13. The CG data are available at http://www2.census.gov. The data cover all governmental units in the United States. The 1972–1997 CG data are aggregated to the county level and used to construct the five-year panel. The 1967–2000 ASG dataset compiled by Greenstone was obtained from the National Bureau of Economic Research (NBER), available at http://www.nber.org/asg/. The ASG is an annual non-random sample of government units. The procedure samples with certainty governmental units serving larger populations or deemed financially significant. Small units (in terms of population or financial activity) are then randomly sampled. To avoid noise created by variation in the ASG sample from year to year, only counties with governmental units continuously appearing in the sample from 1970–2000 are included in the annual panel. U.S. Census Bureau State Tax Collections and State Government Finance data are available at http://www.census.gov/govs/.
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Finally, the annual and five-year panels are restricted to include only border counties. Border counties are defined as counties located on the border of their state with another state. Figure A1 in the online appendix illustrates the counties classified as border counties for the purposes of this paper, and Table A1 in the appendix provides a summary of the variables and their means across panels.

C. Measuring Incentives

The above described data covers all the variables in $\Delta X_{ij,t-1}$ except the key variable of interest, $\Delta I_{ij,t-1}$. The lack of data on incentives is a well known issue. Thomas (2011) concludes that creating an ideal measure of U.S. state and local aid to private industry is an impossible task.\textsuperscript{14} Eisinger (1988) and Buss (2001) reach a similar conclusion. Thus, the typical approach to studying incentives is to investigate one program and/or one location. As discussed above, the few aggregate studies available rely on state measures of economic development expenditure or programs.

While studies using spending and programs provide useful insights, the measures are problematic for our purposes. First, economic development expenditure and program measures often include data on workforce training, research partnerships, marketing, and other activities outside the scope of interest. Clearly, theory suggests the impact of programs targeting capital will be different than programs targeting labor or public perception. Goetz et al. (2011) improves program measures by characterizing programs in terms of race-to-the-top (RTT) and race-to-the-bottom (RTB) policies. However, even their classification does not distinguish between the jobs effect of capital subsidization and other policies. For example, RTT policies include capital subsidy programs targeted at innovative firms, while RTB policies include traditional capital subsidy programs.

Endogeneity is likely to be at least as problematic as the confounding effects of different policies included in expenditure and program measures. For example, one can imagine a location that is already attractive for business and is experiencing significant job growth. Holding the level and types of economic development incentive programs constant, the level of economic development expenditure would increase as companies already locating in the area take advantage of available incentives. Estimates based on this measure, particularly those that do not adequately control for job growth fundamentals, will indicate incentives cause job growth. The reality, however, is that program availability caused existing firms or firms who would have located there otherwise to take advantage of them; thus, it is job growth that caused the increase in economic development expenditure rather than vice versa.

\textsuperscript{14} Thomas (2011) estimates 2005 total U.S. state and local corporate subsidies at $64.8 billion, of which he classifies $46.6 billion as investment incentives. He obtains this estimate through extrapolation based on data from the few consistent state and local sources available. State subsidy levels are estimated based upon share of economic activity. Most local subsidy levels are based upon the assumption that local subsidies equal state subsidies. While providing a reasonable total estimate, the procedure is not ideal for uncovering how state and local variation affects economic activity. It may also significantly underestimate (overstate) local incentives, depending upon whether local subsidies are greater (less) than state subsidies. For example, Thomas reports Missouri has a 7:1 local/state subsidy ratio.
One can also imagine a community suffering from economic distress. It would be reasonable to expect concerned leadership to increase marketing activities, create new programs, and engage in more aggressive economic development. In this case, spending and program measures are endogenously related to expectations about poor future economic performance. If incentives do not succeed in correcting the underlying cause of economic distress, then the net result may simply be providing incentives for jobs that would have existed without them.

In fact, the policy literature indicates that economic development policy does react and evolve based upon economic conditions (Greenbaum, Russell, and Petras, 2010). Empirical evidence is, however, inconclusive with regard to the direction of bias. There is no clear pattern that distressed communities offer more incentives than growing communities (Fisher and Peters, 1998).

Attempts to measure the availability of incentives with business climate measures and surveys of economic development organizations do not overcome the data problem either. Business climate measures often include both tax and subsidy economic development programs, measures of regulatory environments, labor force characteristics, quality of life indicators, and economic growth outcomes. Using data on incentives proposals from economic development organizations to determine differences across states fails to account for the nature of incentives competition. The hypothetical firm method uncovers the first offer in the incentive negotiation process, which may differ substantially from the final incentive package and thus does not necessarily measure the available incentives. For example, the competition for BMW’s first U.S. automotive manufacturing facility illustrates that a location’s initial incentives bid is unlikely to be the final incentive bid. In 1992, BMW announced it had decided to locate in Greenville, SC. The decision was the culmination of a public bidding war between competing locations, which resulted in an increase of Greenville’s initial incentive bid of $35 million to the $150 million final bid (Kurylko, 1992, a, b, c; Patrick, 2012).

Interjurisdictional competition causes rapid policy convergence and innovation. Eisinger (1988) describes the proliferation of programs in the 1960s. He notes that aggressive incentives were first used by economically underdeveloped Southern and Western states. However, by 1966 Northeastern and Midwestern states had responded by not only matching the available Southern and Western programs, but also creating programs unavailable in those areas. According to Site Selection magazine, financial assistance programs were available in all 50 states by 1985 (the first year the magazine published listings) and the average state offered nine of the 15 types of financial assistance reported (International Development Council, 1985). In a more recent survey of U.S. local governments, 95.1 percent of all respondents report offering business incentives (International City/County Management Association, 2009).

Theoretical models of incentives competition predict jurisdictions will match bids until they have exhausted (or exceeded) potential benefits (Guisinger, 1985; Ellis and

15 The IEI reflects the fact that Northeastern and Midwestern states could respond with additional programs. The average 1970 IEI is higher in the Northeast and Midwest than in the South and West.
Does Increasing Development Incentives Result in More Jobs?

Rogers, 2000; Patrick, 2011). If there are no constraints on jurisdictions, they may create new incentives to match competing offers. The Mercedes Benz 1993 selection of Vance, Alabama as the location for its first U.S. manufacturing facility provides an example. In this case, the economic development incentives package included a number of programs developed specifically to induce Mercedes to locate in Alabama. The incentives package was designed to replicate the incentives provided to Toyota when they made their decision to locate in Kentucky (White, 1993).

Many firms also employ site selection consultants (or in-house personnel) with intimate knowledge of the incentives provided in other locations. When negotiating for incentives from a particular location, the firm/consultant will provide information about the incentives offered by other jurisdictions in an attempt to get a matching bid. Suppose a location does not currently offer a particular incentive. If the rules governing incentives allow them to create a similar incentive, then the location may feel more pressure to do so; otherwise, state legal limitations may credibly constrain the jurisdiction from offering this incentive. In this case, both programmatic measures and economic development spending will be a response to local economic conditions. Further, neither adequately captures the actual incentive environment in the location.

The dynamics of innovation and response caused by competition may be the most significant element in determining the overall effect of incentives, but it is largely ignored in most empirical research. An exception is Ó hUallacháin and Satterthwaite (1992), who conjecture that the ease with which industrial revenue bond (IRB) programs can be duplicated in competing jurisdictions likely explains their estimated negative impact of IRB subsidies. While contrasting the incentive packages received by Mercedes for their 1992 plant locations in Alabama and Germany, Thomas (2000) points to differences in the policies on aid to private business. He notes that the incentives given in Alabama could not be provided in Germany. Arguing that European Union constraints restrict incentive levels, Thomas (2000) suggests the lack of incentive constraints in the United States allows the dynamics of interjurisdictional competition to prevail.

It is clear that a lack of federal level constraints on lower level jurisdictions characterizes the U.S. incentive environment. However, unique circumstances in the nineteenth century caused states to impose constraints on state and local governments’ ability to aid private enterprise. As will be argued below, state constitutional provisions enacted in response to historic events are relevant to today’s competitive environment. These state constitutional provisions limit and structure the ability of a jurisdiction to match and innovate in response to economic circumstances. Creating a measure of incentives from these provisions avoids aforementioned endogeneity problems and exploits the dynamics of incentives competition.

In particular, an index based on the provisions of state constitutions measures the ability of locations to provide a certain level of incentives rather than what they actually provide at a given moment in time. The issue is whether increasing the availability of incentives in an area increases job growth beyond that which would have occurred otherwise. The types of programs available in locations across the United States are a direct reflection of the limits placed by these constitutional provisions. Thus, they
provide both a measure of available programs and the limitations placed on the nature of the response to competing offers.

The number and type of incentives available is also an important factor determining the value of incentive bids. Although one type of incentive may be substituted for another, compensating provisions are generally less valuable. Further, the marginal value to the firm of compensating provisions decreases, compared to the more desirable incentives. For example, consider the cash incentives provided by Alabama to Mercedes. The incentive package included a $100 million payment to the company. Most states prohibit direct transfers of public funds to private entities. In order to match Alabama’s cash offer, such a state can offer another type of incentive. Compensating incentives could come in the form of an additional $100 million in loans or property. The present value of a $100 million loan must be discounted by the (usually reduced) interest rate, yielding a present value lower than the $100 million cash transfer. Increasing the loan amount to achieve the same present value as the cash transfer will also increase liabilities on the firm’s balance sheet, which may have broad consequences for the firm and further decrease the marginal value of additional loan funds. However, the public entity’s marginal cost of providing the loan is not decreasing as the loan amount increases. While cash is liquid and fungible, the same cannot be said for property. Cash may be used for a variety of purposes, whereas property cannot and there are diminishing marginal returns to additional property for any given project — land and machinery beyond a certain point add little to no value to firm output at the site. The cost of providing additional property, however, is not decreasing. If additional property must be financed (as opposed to funded out of current revenue), then the costs may actually increase. Further, many states that prohibit cash transfers also prohibit property donations. Instead, the public entity owns the property and leases it to the private company at a reduced rate. Clearly, any type of compensating incentive will have to be larger than the more desirable cash transfer to achieve the same present value to the company. Additionally, the compensating provisions represent less risk for the public entities. Thus, both present value to the firm and public risk are increasing in the type and number of incentives available.

V. THE INCENTIVES ENVIRONMENT INDEX (IEI)

Business recruitment activity takes place within the context of state constitutional provisions limiting and structuring the freedom of local and state governments to use public credit, money, and property for the benefit of private enterprises (Pinsky, 1963; Green, 1990; Schaefer, 1998). Analyzing the various incentives available, Gray and Spina (1980) assert constitutional prohibitions as the major reason gifts of land and money are the least used incentive. Similarly, Anderson and Wassmer (2000) find that most states do not use general obligation debt for economic development because their constitutions prohibit it. IRBs provide another example. Their use became widespread during the 1960’s and 1970’s. The few states whose constitutions prohibited their use were disadvantaged from competing for new firms and amended their constitutions
Does Increasing Development Incentives Result in More Jobs?

16 It is generally accepted that there are three state constitutional provisions governing public aid to private enterprises: (1) credit clause(s), (2) current appropriations clause(s), and (3) stock clause(s) (Pinsky, 1963; Roy, 1969; Gray and Spina, 1980; Gelfand and Amdusky, 1986; Marks and Cooper, 1988; Green, 1990; Schaefer, 1998; Rubin, 1999). I create the Incentives Environment Index (IEI) for every state and year 1970–2000 from these three clauses. The clause scoring methodology is similar to that used by Ameil, Deller, and Stallman (2009) in their Tax and Expenditure Limitation Index. Each clause is scored based on sub-categories and the sub-category scores are then summed. A higher score means a jurisdiction has more freedom to use the incentives governed by that clause. The IEI is calculated by adding the individual state and local clause scores to reflect the possibility of substitution of one type of incentive for another.\(^{17,18}\)

The clauses originated in the mid- and late-19th century in response to state and local government financial crises caused by participation in economic development projects (e.g., railroads, canals, ferries, etc.) and should provide an exogenous measure of the incentives environment in a given location (Roy, 1969; Rubin, 1999; Tarr, 2000). Some amendments to these clauses occur during the study period. Some amendments reflected political movements focused on the role of government, while others allowed a location to provide new relevant baseline incentives, but they were not endogenously motivated by local economic conditions (Tarr, 2000). For example, Idaho’s 1982 amendment was specifically designed to allow IRBs; prior to the amendment, Idaho had the most restrictive local credit clause in the country.\(^{19}\) The amendment allowed city and county authorities to issue IRBs, and also enabled revenue-secured debt in general. Tax Increment Financing (TIF) enabling legislation followed. The largest changes over the period occur in Louisiana and Montana due to the adoption of entirely new constitutions. During the sample period, over one-third of the states changed their constitutions at least once. Although some states did not change their constitutions during the sample period, approximately 30 percent changed multiple times. The vast majority of these changes increased the ability of public entities to aid private entities; however, there

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17 Summing implicitly assumes clauses have equal marginal effects. Disaggregated results are available in the online appendix at https://sites.google.com/site/carliannepatrick/research. F-tests for equality of coefficients suggest this assumption is valid for some samples, but may be too strong for others. Exploring other weighting schemes may be a fruitful area of future research.

18 In the composite index, a value of zero indicates the most restrictive combination of clauses possible, and values of the index should be interpreted relative to that benchmark. A state with no state or local restrictions on the use of public funds to aid private enterprise would achieve the highest possible value, indicating that it has the ability to provide economic development incentives in any manner.

19 Idaho’s local credit clause score in 1982 was 3, which was well below the next lowest score of 11.
were isolated instances of states that moved in the opposite direction. For example, Montana’s new 1973 constitution imposed new restrictions on the use of state current appropriations to aid private entities.

State constitutional change occurred less frequently in the 1990’s than in the 1970’s and 1980’s, with 13.6 percent of the changes occurring in the 1990’s compared to 40.9 percent and 45.5 percent in the 1970’s and 1980’s, respectively. States in the South and West census regions were more likely to change their state constitutions than Midwestern and Northeastern states, reflecting the generally less restrictive constitutions in the Midwest and Northeast at the start of the sample period. Approximately 33.3 percent of continental states are located in the South Census region, but 43.8 percent of the states that changed their constitutions are located in the South. Similarly, Western states accounted for 31.3 percent of the states that changed their constitutions and 22.92 percent of all states. Relative economic activity, as measured by Gross State Product (GSP) per capita, also varies among the states that changed their constitutions, with 59 percent of changes occurring in states with lower GSP per capita than the average state in the year of the change and 41 percent in states with higher than average GSP per capita.

Counties are the unit of analysis. County panels vary substantially in the number of county-year observations by construction. Thus, within state variation translates into different numbers (and percentages) of observations with a change within each sample. For each panel, the post-hoc achieved power is determined by the combination of IEI variation, sample size, and the chosen significance levels. Analyzing the relationship between post-hoc achieved power for the IEI and the proportion of observations with a change in each specification reveals no systematic relationship. Achieved power is increasing in the estimated economic effect. As expected, it is easier to distinguish large estimated effects from zero than small estimated effects.

A. The Clauses

It is useful to briefly consider each clause and the types of economic development incentives that it structures and limits. The scoring system for each clause captures variation in the type of activity covered, the scope or entities restricted, explicit exemptions, and the approval process. The online appendix provides additional detail on the clauses and scoring.

The credit clause governs the use of public credit to aid private enterprises. It is primarily a financing clause and covers activities such as IRBs, bond-financed grants provided to private industries, borrowing-financed industrial park land, etc. The way the incentive is financed determines whether the clause applies to a particular incentive. Credit may or may not be backed by the full taxing power of the entity. The latter type of credit is referred to as revenue-secured debt, but it includes moral obligation bonds and debt secured by a dedicated special tax. The project or other revenue stream secures revenue debt rather than the full faith and credit of the issuing entity; hence, it represents much less valuable aid to the beneficiary project than general obligation credit secured by the entity’s taxing power.

Depending upon the type of debt covered by the credit clause, there is further variation in scope, exemptions, and required approvals. For example, a state may restrict
the use of state credit to aid private enterprises but allow the state to extend its credit to state authorities and political subdivisions to use in financing economic development incentives. If the local governments are prohibited from using those funds in a particular way, then that prohibition is reflected in the local score. Similarly, a state may require electoral approval before using its credit in aid of private entities.

The current appropriations clause covers funds appropriated for donation as well as funds appropriated to provide loans. Donations of cash, property, neither, or both may be restricted. The clause can also restrict the appropriation of money to a state agency for grants to private enterprises. If current revenue may be appropriated to loan funds to private entities, then the current appropriations clause is invoked rather than the credit clause (Pinsky, 1963). Variation in current appropriations clause scope, exemptions, and approvals is similar to that described for the credit clause.

Finally, the stock clause governs the financial relationship between public and private entities. Some stock clauses only mention stock. Others are more inclusive with restrictions on direct or indirect ownership of any interest in a private company, association, or non-profit organization. The stock clause is relevant for economic development activities such as public-private partnerships, investment in seed capital funds, and other types of stock or ownership. The scoring methodology is similar to that used for the credit and current appropriations clauses.

### B. Understanding the IEI

Table 1 provides descriptive statistics for the IEI as well as for the state and local clause scores. The mean clause scores illustrate the mean IEI in terms of specific public mechanisms for aiding private enterprise. For example, the mean local credit clause (LCC) score is 17.1 with a standard deviation of 4.9. In a location with an LCC score

<table>
<thead>
<tr>
<th>Table 1</th>
<th>Annual State Index Summary Descriptive Statistics</th>
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<tbody>
<tr>
<td></td>
<td>Mean</td>
</tr>
<tr>
<td>Incentives environment index</td>
<td>96.16</td>
</tr>
<tr>
<td>Local credit clause</td>
<td>17.14</td>
</tr>
<tr>
<td>Local current appropriations</td>
<td>18.95</td>
</tr>
<tr>
<td>Local stock clause</td>
<td>13.23</td>
</tr>
<tr>
<td>State credit clause</td>
<td>15.30</td>
</tr>
<tr>
<td>State current appropriations</td>
<td>19.00</td>
</tr>
<tr>
<td>State stock clause</td>
<td>12.46</td>
</tr>
</tbody>
</table>

Note: The panel includes 48 states over 31 years giving a total of 1,488 observations.
of 17, any local government may issue revenue-backed debt for economic development. Cities, but not other political subdivisions, authorities, etc., are restricted from using general obligation debt to aid private enterprise. A five point increase in the LCC implies that any local government can use general obligation financing for any purpose, including working capital aid. A five point decrease in the LCC implies that all local governmental units can issue revenue debt in aid of firms; however, general obligation debt is almost completely restricted.

The mean local current appropriations clause (LCA) score is 19.0 with a standard deviation of 8.0. An LCA score of 19 indicates that locations may appropriate funds to use for loans to private entities. They may also use a finance authority to provide grants for land purchases. An increase in the LCA by 1 standard deviation allows unrestricted use of current funds; decreasing the LCA to 12 implies that all grants funded by tax collections are prohibited, but loans and loan guarantees are allowed.

The local stock clause (LSC) score has a mean value of 13.2 over the period and a standard deviation of 6.5. This indicates that all governmental entities in the location are prohibited from stock or direct ownership in private entities, but are allowed to have indirect equity through a donation to a nonprofit, for example, which then invests to aid private companies. Increasing the LSC by 1 standard deviation implies that only stock ownership is prohibited. A decrease of 1 standard deviation indicates indirect ownership prohibitions, which would cover activities like donations to public venture capital funds.

The interpretations of the state clause scores are similar to the local interpretations presented above and are omitted for brevity. The mean IEI can be interpreted as characterizing a location that may aid private firms with the mean local and state mechanisms. The mean IEI over the period is 96, with a standard deviation of 24. The average change in a State’s score is 13 with a standard deviation of 22. As discussed above, these changes generally occurred due to political movements focused on the role of government and/or amendments that allowed the location to provide new baseline incentives. The enactment of constitutional amendments to allow additional incentives provides evidence that these constraints are binding. When these constraints are lifted, policy-makers respond with statutory and policy changes.

It would be ideal to have similar measures of economic development incentives to compare to the IEI. However, as discussed, no such measures exist. Reviewing the available incentives data and indices for the period, only two constructs could be expected to have any real correlation with the IEI. The Corporation for Enterprise Development produces an annual Development Report Card that includes a policy index. In 1987, the mobilizing capital subindex of the policy index contained yes/no scores for risk reduction (loan guarantee) programs and assistance institutions. The correlation between the sum of those yes/no scores and IEI groups is 0.42. Site Selection magazine produced yes/no listings of available state financial assistance programs from 1985–2000 (International

State IEI scores were placed into groups to match the Development Report Card scaling and then lagged to years. The data used to create the yes/no listings was from two years prior to the publication of the indices.
Development Council, 1985–2000). I create a state financial assistance measure from these yes/no listings. The IEI appears positively and significantly correlated with this policy measure over the period, with a correlation coefficient of 0.3, significant at the 1 percent level. Using only those portions of the Site Selection listing that are most related to credit and relating them to the IEI credit scores yields a significant 0.4 correlation coefficient. The yes/no measures are blunt measures and do not account for allowable sources and uses of funds. Considering these points, the correlations are relatively high.

The IEI will not reflect changes in tax policies, changes in the target industries, or other statutory innovations unless they require constitutional change. For example, the IEI for states with post-1970 constitutions that allow public entities to provide aid through revenue-secured debt does not change when they first issue an IRB or enact TIF enabling legislation — both of which are potentially endogenous to local economic conditions. The tax/expenditure/debt changes induced by the statutory change are captured in the control variables. However, the IEI does change for those states whose post-1970 constitutions constrained revenue-backed debt. For example, Montana adopted an entirely new constitution in 1973. Although not motivated by local economic development, the new constitution removed many credit restrictions. The legislature responded by enacting TIF enabling legislation in 1974. The IEI measures the type of incentives that can be used in response to local economic conditions. When these constraints are lifted, as in the Montana and Idaho cases discussed above, policy-makers typically respond with statutory and policy changes. The 1987 Texas constitutional amendment provides another example. The amendment authorized grants and loans to private entities for purposes of economic development and allowed political subdivisions to finance loans or grants through bonds. The Site Selection magazine listings reflect the policy responses to this change. The post-amendment listings include new city and state incentives for investing in high unemployment areas, new state matching funds, state loan guarantees, city or county loans, and city and county general obligation debt financing — all of which were unavailable prior to the 1987 amendment.

Thus, the IEI isolates the types of incentives available while avoiding endogenous policy responses to local economic conditions. The Mercedes Benz 1993 selection of Vance, Alabama discussed above, where legislative action lagged the commitment to the company, provides an example of such an endogenous policy response. The IEI reflects the ability of states to make commitments that were later institutionalized through statutory change.

Finally, a few additional features of the IEI are worth noting before proceeding to the estimation results. The IEI is not measuring actual incentives given, but rather the incentives environment. Detailed tax, expenditure, and debt variables capture any fiscal capacity constraints. The IEI is not measuring tax exemptions and credits. The data include variables that reflect corporate, property, and other effective tax rates. The IEI is also not simply a measure of regional variation in state policies. Figures A2 and A3 in the online appendix show the 1970 and 2000 state distributions of the IEI and clearly demonstrate intraregional variation in the index.
VI. ESTIMATION RESULTS

Tables 2–4 contain estimates based upon the empirical strategy outlined in Section IV.\textsuperscript{21} IEI levels are used for $\Delta \text{IEI}_{j,t-1}$ and demographic control variables for each county are added to $\Delta \text{X}_{j,t-1}$. All standard errors and inference are robust to heteroskedasticity and serial correlation.

Table 2 presents standardized beta coefficients for the IEI from first-difference and random trend estimation. First difference estimation of the annual and five-year panels suggests that a 1 standard deviation increase in the IEI decreases county employment levels by 0.088 and 0.097 standard deviations (16,087 and 13,661 jobs), respectively, and has no statistically significant effect on growth. The estimated growth effects are economically insignificant as well, suggesting, for example, that annual growth rates increase from 2.08 to 2.09 percent. In the border samples, the only significant effect is on five-year county employment levels. The impact remains negative, but is much smaller in magnitude.

Estimating the random trend model with the annual sample, the IEI has a significant positive effect on employment level and a significant negative effect on growth. A 1 standard deviation increase in the ability of governments to provide aid to private enterprises is associated with a 0.029 standard deviation increase in county employment levels (5,301 jobs). Only a standard deviation increase in earnings per employee has a larger positive impact on the county employment level.\textsuperscript{22} Other coefficients are of reasonable sign and magnitude. The random trend estimated level effect is the opposite of the highly significant estimates obtained using the first-difference model. Also, unlike the first-differenced result, a 1 standard deviation increase in the IEI is associated with a 0.033 standard deviation decrease in county employment growth. The growth effect is small in an economic sense, though, suggesting annual county growth rates fall from 2.08 to 1.95 percent.

While the signs of the coefficients are the same as with annual samples, five year random trend model estimates suggest the incentives environment has no significant effect on county employment level or growth rates. Recall that the five-year panel contains more rural counties than the annual panel. Thus, the difference between the annual and five-year estimates may be driven by differences in the economic sizes of counties in the samples or by differences in the short- and medium-term effects.

The changes in the signs and significance of the coefficients estimated with the random trend model relative to those estimated with the first-differenced model imply unobserved heterogeneity in county trends, which may bias the first-differenced estimates. When the annual and five-year border county panels are used, random trend estimates indicate the incentives environment has no significant impact on either county employment level or growth. Given that the incentives measure is constructed from state constitutions, one would expect the effect of the policy to be most pronounced for border counties.

\textsuperscript{21} Results for the complete set of independent variables are available from the author upon request.

\textsuperscript{22} Complete sets of estimates are available upon request from the author.
### Table 2
First-Difference and Random Trend Estimated Incentives Environment Index (IEI) Coefficients

<table>
<thead>
<tr>
<th></th>
<th>Full Annual and 5-Year Samples</th>
<th></th>
<th>Border Annual and 5-Year Samples</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Employment Level</td>
<td>Employment Growth</td>
<td>Employment Level</td>
<td>Employment Growth</td>
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<tr>
<td></td>
<td>Annual</td>
<td>5 Year</td>
<td>Annual</td>
<td>5 Year</td>
</tr>
<tr>
<td>Observations</td>
<td>41,514</td>
<td>13,798</td>
<td>41,514</td>
<td>13,798</td>
</tr>
<tr>
<td></td>
<td>15,979</td>
<td>5,038</td>
<td>15,979</td>
<td>5,038</td>
</tr>
<tr>
<td>Panel A: First-Difference Estimated IEI Coefficients</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IEI</td>
<td>–0.088***</td>
<td>–0.097***</td>
<td>0.003</td>
<td>–0.001</td>
</tr>
<tr>
<td></td>
<td>(–16.76)</td>
<td>(–9.28)</td>
<td>(0.79)</td>
<td>(–0.09)</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.088</td>
<td>0.123</td>
<td>0.262</td>
<td>0.258</td>
</tr>
<tr>
<td>Panel B: Random Trend Model Results</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IEI</td>
<td>0.029**</td>
<td>0.019</td>
<td>–0.033*</td>
<td>–0.007</td>
</tr>
<tr>
<td></td>
<td>(2.19)</td>
<td>(0.40)</td>
<td>(–1.76)</td>
<td>(–0.14)</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.367</td>
<td>0.523</td>
<td>0.245</td>
<td>0.164</td>
</tr>
</tbody>
</table>

Notes: The coefficients are standardized beta coefficients and t statistics are in parentheses. Asterisks denote significance at the 1% (***(1%)), 5% (**), and 10% (*) levels.
In other words, border county estimates should be an upper bound estimate of the effects of state policy differences because resources can easily move across the border to gain access to the more favorable incentives environment. Such estimates should also significantly reduce any remaining unobservable heterogeneity. Thus, it is curious that the IEI coefficient estimated using the annual random trend approach is no longer significant when the sample is restricted to border counties.

There are two plausible explanations for the insignificance of the border county random trend estimates. First, it is possible that unobservable heterogeneity is driving the results in the previous estimates. Once all potential sources of unobservable heterogeneity are removed through first-differencing, using fixed effects, and restricting the sample to border counties, it is possible that the incentives simply do not have an effect. However, incentives are often justified on the basis of agglomeration economies. If, indeed, the capital attracted to the jurisdiction offering the incentive generates agglomeration externalities, then it may be reasonable to think that changes in the government’s ability to aid firms affects urban and rural areas differently. Although the estimating equations should control for these effects, they might not if the underlying employment process is different in rural and urban areas.

Hammond and Tosun (2011) assert that employment factors have different effects in rural and urban areas. Partridge, Rickman, and Li (2009) also suggest rural and urban areas respond differently to job creation stimuli. As noted before, the lack of significance in the five-year panel estimates might arise because the sample is more rural than the annual panel. The border county restriction thus might emphasize the local rather than the global response. If so, using the border county restriction on pooled counties may increase the extent to which opposite effects in rural and urban areas are offsetting.

Table 3 presents results from estimating the random trend models separately for urban and rural counties. It appears that the positive effects of the incentives environment on annual county employment are driven by positive impacts for urban counties. A 1 standard deviation increase in a government’s ability to aid private enterprise increases urban county employment levels by 0.042 standard deviations (11,983 jobs). The effect is significant and large compared to a 1 standard deviation change in other factors. However, the effect for rural counties is negative but insignificant. The annual effect on employment growth is still negative, although imprecise and economically small for rural and urban counties. For example, the estimates for rural counties indicate that annual growth rates decrease from 1.8 to 1.7 percent.

In the medium term, the positive effects in urban areas disappear. However, there is a significant negative effect on rural county employment levels. A 1 standard deviation increase in the IEI is associated with a 0.103 standard deviation decrease in five-year rural county employment (1,308 jobs). These results suggest differences in urban and rural responses to employment stimuli account for some, but not all, of the differences across panels noted above. Although changes between annual and five-year panels were partially caused by urban counties being overrepresented in the annual panels, differences in short and medium term effects persist.
Table 3
Rural and Urban Random Trend Model Incentives Environment Index (IEI) Results

<table>
<thead>
<tr>
<th></th>
<th>Rural Counties</th>
<th></th>
<th>Urban Counties</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Employment Level</td>
<td>Employment Growth</td>
<td>Employment Level</td>
<td>Employment Growth</td>
</tr>
<tr>
<td></td>
<td>Annual</td>
<td>5 year</td>
<td>Annual</td>
<td>5 year</td>
</tr>
<tr>
<td>IEI</td>
<td>-0.019</td>
<td>-0.103***</td>
<td>-0.036</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(-1.08)</td>
<td>(-2.89)</td>
<td>(-1.50)</td>
<td>(-0.26)</td>
</tr>
<tr>
<td>Observations</td>
<td>26,277</td>
<td>10,740</td>
<td>26,277</td>
<td>10,740</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.379</td>
<td>0.556</td>
<td>0.231</td>
<td>0.175</td>
</tr>
</tbody>
</table>

Notes: The coefficients are standardized beta coefficients and t statistics are in parentheses. Asterisks denote significance at the 1% (**), 5% (**), and 10% (*) levels.
### Table 4

**Rural and Urban Border County Random Trend Model Incentives Environment Index (IEI) Results**

<table>
<thead>
<tr>
<th></th>
<th>Rural Border Counties</th>
<th>Urban Border Counties</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Employment Level</td>
<td>Employment Growth</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Annual</td>
<td>5 Year</td>
<td>Annual</td>
</tr>
<tr>
<td><strong>IEI</strong></td>
<td>0.012</td>
<td>–0.105**</td>
<td>–0.035</td>
</tr>
<tr>
<td></td>
<td>(0.43)</td>
<td>(–2.07)</td>
<td>(–1.05)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>10,619</td>
<td>3,969</td>
<td>10,619</td>
</tr>
<tr>
<td><strong>Adjusted R-squared</strong></td>
<td>0.361</td>
<td>0.559</td>
<td>0.219</td>
</tr>
</tbody>
</table>

Notes: The coefficients are standardized beta coefficients and t statistics are in parentheses. Asterisks denote significance at the 1% (***)**, 5% (**), and 10% (*) levels.
The estimates from separate urban and rural border county regressions are presented in Table 4. The only significant effect is on five-year rural employment level. A 1 standard deviation increase in incentives is associated with a 0.105 standard deviation decrease in five-year county employment (1,390 jobs). Only a 1 standard deviation increase in individual income taxes has a larger negative impact on county employment levels. There is no significant effect on urban levels, rural growth, or urban growth.23

The negative medium term rural employment effect is consistent between the full county and border county panels. Thus, it is unlikely that the border estimates are accounting for local rather than global effects. Instead, the border county methodology appears to serve the desired purpose of improving identification by removing unobserved heterogeneity.

Taken together, the above results indicate that the failure to control for potential sources of endogeneity results in biased estimates of incentive effects. Using the random trend method with the border county sample eliminates unobserved level and trend heterogeneity that could result in such bias. Therefore, the border county random trend estimates are preferred to the other reported estimated impacts. Constraining urban and rural areas to the same employment process also masks important differences and can lead to misleading interpretations. Estimated effects from separate regressions are thus preferred to pooled results.

With this in mind, the most robust result suggests that increasing the ability of governments to aid private enterprise has a negative effect on five-year rural county employment and no effect otherwise. At best, the availability of non-tax incentives may have a positive annual effect on urban employment levels, causing overall positive estimated annual effects when urban and rural counties are pooled. Short-term positive urban impacts do not affect county employment growth rates or five-year levels. The short-term positive effects also disappear when the sample is restricted to urban border counties, lending credence to the idea that the estimated positive annual effect is the result of unobserved heterogeneity. The next section discusses these findings in more detail.

VII. DISCUSSION

These results contrast with the findings of de Bartolomé and Spiegel (1995) and Goss and Phillips (1997) but are consistent with Ó hUallacháin and Satterthwaite (1992), Gabe and Kraybill (2002), and Goetz et al. (2011). The former group of authors uses spending as the measure of economic development incentives. The latter group of authors, however, uses program measures and is more careful with regards to potential sources of endogeneity.

23 It should be noted that the five-year urban estimates are not very powerful due to small sample size. The estimated coefficients imply that urban county employment levels increase by 11 percent — double the estimated five-year level effect from the full urban county sample. The annual urban level effect is both statistically and economically insignificant, indicating an increase of 232 jobs (0.18 percent of the sample mean).
Recall the total derivative of equilibrium employment with respect to non-tax incentives presented in Section III.B. The estimated coefficient is for the direct effect, holding wages and the government service tax-price constant. Thus, indirect incentive-induced changes in these elements are not causing the estimated negative effect. In determining the overall effect of incentives, the signs and magnitudes of the indirect effects should be considered. If the direct effects are negative or insignificant, though, it seems unlikely incentives would indirectly induce a virtuous cycle of employment increase and growth.

Referring back to Figures 1 and 2, there are two possible reasons for the negative and insignificant direct effects: either increasing the availability of incentives in an area does not result in a net capital increase, or the net new capital does not result in new jobs (perhaps because capital is substituted for labor). The present analysis is not suited to determining which is occurring. I am unaware of any studies on the aggregate capital effects of incentives or research that assesses the possible underlying mechanisms at work. Future research on this topic could provide further insights.

Why might non-tax incentives induce a net negative (or no) change in the capital stock in a state? The most obvious explanation is that incentives do not affect firm location at the margin. Not all firms in a location receive economic development incentives. If other firms bear the opportunity costs associated with greater incentive availability (not captured by taxes and services) and firm location is unaffected at the margin, then increasing incentives may harm employment (Goetz et al., 2011; Rodriguez-Pose and Arbix, 2001).

Studies of firm location decisions suggest the process occurs in two stages (Schmenner, Huber, and Cook, 1987; Addy, 1997; Bartik, 2005). In the first stage, firms determine a set of profit-maximizing locations. Incentives become relevant only in the second stage as a factor that differentiates similar locations. Incentives are thus used to induce firms that are already attracted to the area as a profit-maximizing location, but do not fundamentally change whether the area is a profit-maximizing location. In other words, incentives do not make a bad place to do business a good place to do business. They make a good place to do business a marginally better place to do business. If this is true, then the opportunity costs of the incentives may be the dominant effect.

There are other possible reasons for incentives to induce a negative (or insignificant) net increase in capital. Induced capital might be replacing, displacing, or supplementing outdated existing capital. In addition to attracting firms, economic development incentives may be granted to existing firms. Existing firms may use the incentives to replace existing capital, in which case the net effect could be neutral or negative. The existing firm might also be using outdated production technology and would be forced out of the market without incentives, in which case incentives may allow a firm to continue operation. If incentives are given to a new firm and outdated firms are in the area, then

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24 For example, Cerqua and Pelligrini (2014) find positive effects of Italian L488 capital subsidies on recipient firms’ employment, investment, and turnover. However, De Castris and Pelligrini (2012) and Cerqua and Pelligrini (2013) find evidence that the L488 subsidies caused employment reductions in unsubsidized firms.
the increase in competition may also force existing firms to close. The net result could be neutral or negative, particularly if the new firm is less capital intensive than the old firm.

As previously stated, it is also possible that increasing the availability of public aid to private industries may increase capital but not create jobs. Crotty (2003) suggests that attracting capital with incentives creates overcapacity, particularly in the core industries. The induced capital could be redirecting capital from productive activities into overcapacity and thus result in negative (or neutral) employment effects. Capital subsidy studies generally do not consider (existing or induced) excess capacity.

Given the estimated negative effect of incentives on medium-term rural employment, the substitution and rent-seeking explanations seem particularly plausible reasons for “jobless capital.” In the medium and long term, managers may adjust to public aid by substituting capital or public inputs for labor (Fisher, 2007). If this substitution effect dominates, then future employment levels will be lower than predicted. In addition, rather than substituting away from labor, managers may engage in additional rent-seeking as a result of the increased availability of incentives. Managers may view incentives as windfall profits. As managers spend more time rent-seeking than increasing productivity and developing new products/markets, they may manage the firm less efficiently and move away from their production frontier. The net result is lower future employment (Gabe and Kraybill, 2002).

Any of the aforementioned mechanisms would be consistent with the estimated effects. The fact that the impacts vary systematically between rural and urban areas points to several additional areas for future research. Are there systematic differences in the types of capital attracted to rural and urban areas? Is rent-seeking more costly in rural areas than urban areas? Or is there a simple urban economics explanation? Do agglomeration economies generate enough positive spillovers to compensate for any negative effects?

VIII. CONCLUSIONS

As policy-makers search for ways to increase employment in their jurisdictions, economic development incentives are often the primary tool. Most economic development programs and incentives aim to increase capital investment. The presumption is that increasing capital increases employment and wealth in the jurisdiction. From a policy perspective, it would be useful to know if the economic development incentives aimed at increasing capital create more jobs. Existing research provides mixed conclusions on the impact of economic development incentives. Further, available measures often include economic development programs with a wide variety of goals beyond increasing firm capital. Ultimately, data and methodological problems hamper the ability of existing research to determine whether making more incentives available in an area

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25 Similarly, capital-directed incentives could induce industrial churning in the medium term. As labor-intensive firms are replaced by capital-intensive firms, employment falls. Hanson and Rohlin (2011) and Patrick (2013) provide evidence of differential response to economic development incentives in labor- and capital-intensive sectors.
increases employment beyond that which would have occurred otherwise. In order to shed some light on this question, I create an Incentives Environment Index (IEI) and use it to estimate the effects of increased availability of incentives on county employment level and growth.

The results suggest that increasing the availability of non-tax incentives negatively affects medium-term rural employment. There appears to be no effect otherwise, although it is possible there may be small short-term benefits to urban county employment levels. The finding of negative or no effect calls into question the generally accepted practitioner view that providing more economic development incentives to attract capital will result in more jobs. The paper discusses a number of possible mechanisms for the results.

Burstein and Rolnick (1995) and Thomas (2011) call for federal intervention to restrict competition in economic development incentives in the United States. Since the analysis suggests that easing constraints on non-tax and discretionary incentives does not support local job growth, the results could be taken as support for increased restrictions. The gains from increased restrictions, however, rely critically on the mechanism(s) underlying the results. If the mechanism is capital-labor substitution, then the incentives may result in productivity gains. Capital subsidies may therefore be an ineffective jobs tool, but an effective productivity growth policy. On the other hand, mechanisms such as rent-seeking and overcapacity suggest restrictions will reduce the possibility that state and local governments will offer wasteful incentives.

Several areas of research would significantly contribute to our understanding of economic development incentives and jobs. Study of aggregate county capital effects would provide insight into whether incentives are resulting in less capital or “jobless capital.” Examination of the systematic differences between the types of subsidy-induced capital in rural and urban areas would also shed light on the issue. Similarly, the rent-seeking costs associated with increasing the availability of incentives should be explored. It would also be helpful to further investigate the role of agglomeration economies in determining the effect of non-tax incentives on employment.

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Does Increasing Development Incentives Result in More Jobs?

DISCLOSURES

The author declares that she has no relevant or material financial interests that relate to the research described in this paper.

REFERENCES


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