Job Creation Tax Credits and Job Growth:

Whether, When, and Where?

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December 2010

PRELIMINARY DRAFT

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FOR PRESENTATION AT THE 2011 AMERICAN ECONOMIC ASSOCIATION MEETINGS

January 7, 2011

* **Acknowledgements:** We would like to acknowledge the excellent research assistance provided by Katherine Kuang, Charles Notzon, Tom O'Conner, and the comments and suggestions from seminar/conference participants at the CESifo, European Central Bank, Federal Reserve Bank of San Francisco, Institute for Advanced Studies (Vienna), National Tax Association, and North American Regional Science Association, especially our formal discussants, Elliott Dubin and Leo Feler. Financial support from the Federal Reserve Bank of San Francisco and the Upjohn Foundation is gratefully acknowledged. All errors and omissions remain the sole responsibility of the authors, and the conclusions, based on the preliminary analysis contained in this paper, do not necessarily reflect the views of the organizations with which they are associated.

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Abstract

This paper studies the effects of Job Creation Tax Credits (JCTCs) enacted by U.S. states over the past 20 years. First, we investigate *whether* JCTCs stimulate within-state job growth. Second, we evaluate *when* JCTCs' effects occur? In particular, we test for negative anticipation effects between JCTC enactment and when legislation goes into effect. Third, we assess from *where* any increased employment comes from – in-state or out-of-state? These questions are investigated in an event study framework applied to monthly panel data on employment, the JCTC effective and legislative dates, and various controls.

Keywords: Job creation tax credits, state business tax incentives, spatial externalities, anticipation effects, fiscal foresight, implementation lags
JEL codes: H25, H32, H71

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1. Introduction

The current U.S. recession has taken a heavy toll on nearly all aspects of the economy. Perhaps nowhere has the toll been greater than on the labor market. The unemployment rate now exceeds 10% and monthly job losses have continued even as real quarterly GDP growth has turned positive. This stubbornly high unemployment rate has generated discussions about innovative fiscal policy instruments, such as job creation tax credits (JCTCs), to help stimulate labor demand. In fact, such discussion began early in the recession as policymakers debated various ways to stimulate the economy. For example, Bartik and Bishop (2009) recently argue that a "well-designed temporary federal job creation tax credit should be an integral part of the effort to boost job growth." President Obama included a JCTC in his economic platform as a candidate for presidency, and he and his transition team listed such a credit in their economic stimulus proposal. Though a JCTC was not included in the final stimulus package (the "American Recovery and Reinvestment Act of 2009") passed by Congress in February 2009, it has resurfaced in policy discussions of late due to the persistence of net job losses in the economy. A temporary federal JCTC was adopted in early 2010.

Such a credit has been tried only once before at the U.S. federal level, with the 1977-78 "New Jobs Tax Credit" (NJTC; see Sunley, 1980). The NJTC offered corporations with taxable income a credit whose value was proportional to the increase in the corporation's net payroll employment level above 102% of its previous year's employment level. Using survey data, Perloff and Wachter (1979) found that firms which reported knowing about the credit experienced 3% higher employment growth than other firms. Bishop (1981) also studied the employment effects of the NJTC and found that it increased employment in the Construction, Trucking, Wholesale, and Retail sectors in 1977-78 by between 0.66% and 2.95%.

Although the federal government's JCTC experience is quite limited, nearly half of U.S. states have enacted JCTCs over the past twenty years. Chart 1 shows the policy diffusion process for state JCTCs, using the legislative enactment dates that we compiled for this paper.

The first JCTCs were adopted in late 1992 and, by August 2009, twenty-three states had such a credit.[1]

Chart 2 shows which states have these credits as of August 2009. The plurality of JCTC states are in the eastern United States, but there are also many in the Midwest and South. The design of these JCTCs varies among states (discussed in Section 3 below). The monetary value of the JCTCs also varies among states (see Wilson and Notzon 2009), though for this preliminary draft, we do not incorporate monetary values.

An important element in this paper is the creation of a comprehensive database on JCTCs. We compile the relevant legislative dates for all state JCTCs that have been passed in the U.S. since at least 1990 (and probably much earlier). For each JCTC, we have identified two dates: (1) the "signing" date on which the legislation is signed into law by the state's governor and (2) the "qualifying" date on or after which net new hires by an in-state employer can qualify for the credit. (These and other terms are defined in the glossary.) We combine this information with data on employment outcomes from January 1990 to August 2009 to investigate three important aspects of JCTCs: *whether*, *when, and where* they affect job growth. First, we assess *whether* JCTCs succeed in stimulating job growth within the enacting state or are merely an inframarginal transfer to employers. Second, using data for each JCTC state on the date at which the credit was signed into law and the date at which new hires may qualify for the credit, we evaluate *when* JCTCs' employment effects occur. In particular, by exploiting the variation among states in the sequence of these two dates –either having an implementation lag between signing and qualifying dates, a retroactive period between qualifying and signing dates, or simultaneous dates – we can test and control for anticipation effects. For instance, if firms have an implementation period after the legislation has been signed into law but before they can begin hiring under the credit, they may delay hiring until the qualifying date. This general phenomenon – sometimes called "Ashenfelter's Dip" (Ashenfelter 1978) in labor economics or "fiscal foresight" in

[1] As of October 2010, every JCTC that has been enacted since 1990 is still in effect except one. Colorado enacted a "performance-based" employment incentive in June 2006 that acts essentially like a refundible tax credit, but in August 2008 it was modified substantially to no longer operate like a tax credit. (Instead, it became more like a set-aside fund that the state government could dip into for luring large corporations considering moving to/from the state.) Colorado did in fact pass a job creation income tax credit in May 2009 and it went into effect in August 2009. Note, however, that Colorado's 2009 JCTC does not have any effect on the regression results in this paper because the credit occurs at the very end of our sample period and our regression specification requires 12 months of employment data before and after a credit's effective date.

macroeconomics – has become an important topic in recent debates over assessing the effects of fiscal policy (e.g., Ramey (2008), Romer and Romer (2009), and Leeper, Walker, and Yang (2009)). Third, in future work, we will explore *where* the employment effects of JCTCs are to be found. Do these credits affect only in-state job growth or do they also negatively affect job growth in "neighboring" states?

Our paper proceeds as follows. Section 2 offers a theoretical framework for understanding the effects of a JCTC and analyzes the intertemporal decisions faced by a firm. Section 3 describes the unique dataset that we have collected on state JCTCs. Section 4 discusses our preliminary empirical results. Section 5 discusses our results in terms of the thin extant literature and considers policy implications. Section 6 concludes and mentions the next steps in this research program.

2. A Theoretical Framework

This section presents a dynamic model to provide guidance as to the patterns of policy-response coefficients we might expect from a forward-looking firm facing a JCTC. There are four periods in the model, and we focus on the case where there is an implementation lag between when the credit is signed into law and when it goes into effect:

1) the period between when the credit is signed into law (the "signing date") and goes into effect (the "qualifying date"),
2) the period the tax credit goes into effect,
3) the period immediately after the tax credit goes into effect,
4) one additional period after the tax credit goes into effect.

The firm is assumed to maximize discounted profits subject to a fixed sales constraint. (We choose a profit-maximization objective because it is more natural to think of inventory stocks as increasing profits than reducing costs.) We allow for production smoothing via inventory accumulation.

The firm hires labor and accumulates inventory to maximize the following flow of discounted profits,

$$\begin{aligned} \Pi &= S - w * L_1 \\ &+ \left\{ S - w * L_2 + \tau * w * (L_2 - L_1) + \beta(L_1^{\alpha} - S + INV) \right\} / (1+\rho) \\ &+ \left\{ S - w * L_3 + \tau * w * (L_3 - L_2) + \beta(L_1^{\alpha} + L_2^{\alpha} - 2 * S + INV) \right\} / (1+\rho)^2 \\ &+ \left\{ S - w * L_4 + \tau * w * (L_4 - L_3) + \beta(L_1^{\alpha} + L_2^{\alpha} + L_3^{\alpha} - 3 * S + INV) \right\} / (1+\rho)^3 \\ &+ \lambda \left(L_1^{\alpha} + L_2^{\alpha} + L_3^{\alpha} + L_4^{\alpha} - 4 * S \right) \end{aligned} \tag{1}$$

where Π is the maximand, S is an exogenously given level of sales assumed constant over all four periods, w is the wage rate assumed constant over all four periods, L_n is the level of labor hired by the firm in period n=1,4, and τ is the JCTC. The amount of the credit depends on the current level of employment relative to the previous level. This "rolling base" is represented by the difference ($L_n - L_{n-1}$) multiplying the tax credit and wage rate in the second, third, and fourth periods. Output is produced according to the production relation, L_n^{α}, and is distributed between current sales and inventory accumulation, ($L_1^{\alpha} - S$). The production relation is concave ($\alpha < 1$), which assures that the second-order conditions for profit-maximization are satisfied. The value to the firm of the stock of inventory is determined by β, which multiplies the sum of current inventory accumulation and the existing stock of inventories (INV). The firm faces a constraint that the inventory stock is the same at the beginning and the end of the optimization problem ($L_1^{\alpha} + L_2^{\alpha} + L_3^{\alpha} + L_4^{\alpha} - 4*S$). The shadow price on this constraint is λ. Future cash flows are discounted by ρ.

The firm maximizes Π by making four hiring decisions subject to the technological and inventory constraints. The first-order condition (FOC) of hiring labor in period 1 is as follows,

$$
\begin{aligned}
&\mathrm{FOC}_1 : 0 = \\
&-w && \text{(cost of hiring labor)} \\
&-tc * w / (1+\rho) && \text{(cost of increasing the tax credit base)} \\
&+\beta\alpha * L_1^{*\alpha-1} / (1+\rho)^1 && \text{(benefit of added inventory)} \\
&+\beta\alpha * L_1^{*\alpha-1} / (1+\rho)^2 && \text{(benefit of added inventory)} \\
&+\beta\alpha * L_1^{*\alpha-1} / (1+\rho)^3 && \text{(benefit of added inventory)} \\
&+\alpha\lambda L_1^{*\alpha-1} && \text{(marginal value of output multiplied by the shadow price)}
\end{aligned}
\tag{2}
$$

The economic interpretations of each element are provided above. This FOC can be rearranged to solve for optimal employment in period 1,

$$
\begin{aligned}
&L_1^* = \left((w * (1 + \tau / (1+\rho))) / \Phi_1\right)^{-(1/(1-\alpha))} \\
&\qquad \Phi_1 = \beta\alpha / (1+\rho)^1 + \beta\alpha / (1+\rho)^2 + \beta\alpha / (1+\rho)^3 + \alpha\lambda
\end{aligned}
\tag{3}
$$

where Φ_1 is the discounted value of an increment to the inventory stock. Equation (3) indicates that the optimal level of employment is negatively related to wages (note the expression in parentheses is inverted) and, somewhat paradoxically, negatively related to the tax credit. But recall that, in this first period, the firm is not yet eligible to receive the credit. Period 1 hiring leads to a lower base in subsequent periods, and hence reduces profits. Period 1 hiring is positively related to the benefits of holding inventory.

The comparable FOC's for the second and third periods are as follows,

$$L_2^* = \left((w*(1-\tau(\rho/(1+\rho))))/\Phi_2\right)^{-(1/(1-\alpha))} \qquad (4)$$
$$\Phi_2 = \beta\alpha/(1+\rho)^1 + \beta\alpha/(1+\rho)^2 + \alpha\lambda$$

$$L_3^* = \left((w*(1-\tau(\rho/(1+\rho))))/\Phi_3\right)^{-(1/(1-\alpha))} \qquad (5)$$
$$\Phi_3 = \beta\alpha/(1+\rho)^1 + \alpha\lambda$$

The tax credit raises employment in the second and third periods provided that there is a time value to money ($\rho > 0$). The $\rho/(1+\rho)$ reflects that a hire today benefits the firm because of the tax credit but hurts the firm because of the rolling base. Since ρ is a relatively small number, the rolling base feature substantially attenuates the impact of the tax credit.

The FOC for the fourth and final period is determined by the inventory constraint,

$$L_4^* = \left(4S - L_1^{*\alpha} - L_2^{*\alpha} - L_3^{*\alpha}\right)^{(1/\alpha)} \qquad (6)$$

We can use these FOC's to analyze the separate effects of inventory accumulation and the tax credit on employment decisions. Since $\Phi_1 > \Phi_2 > \Phi_3$ and L* increases in Φ_n, the inventory accumulation channel implies that

$$L^*_1 > L^*_2 > L^*_3$$

The effect of the tax credit in isolation (determined by setting $\beta = 0$ predicts that

$$L^*_1 < L^*_2 = L^*_3$$

If we assume that the tax credit effect dominates the inventory effect (i.e., β is relatively small), then the model has the following implications,

$$L^*_1 < L^*_2 > L^*_3 \,.$$

These relations are portrayed in Chart 3 where the Inter-Date, At Date 2, and After Date 2 correspond to periods 1, 2, and 3 in the above model.

The model illustrates the interesting dynamics associated with JCTCs. For example, for a state in an implementation regime, employment may actually fall after the credit is enacted as forward-looking firms delay hiring and draw down inventories to meet current demand, until after the qualifying date. This effect is illustrated by line segment AB. (As shown in the theoretical model, this effect may be exacerbated in states where the value of the credit depends on the level of prior employment.) When the credit goes into effect, employment rises sharply both because firms need to replenish inventories (line segment BC) and because of the lower labor costs (line segment CE). The latter effect is further divided between the "true" short-run effect (line segment CD) and the inventory accumulation effect (line segment DE) due to a need to replenish inventories. Gradually, employment falls as inventories are returned to their steady-state levels, but it remains above the original employment level because of lower labor costs (line segment AF, which is the same length as line segment CF).

In contrast, for concurrent or retroactive credits, and assuming the credits are not anticipated prior to enactment (represented by line segment ZA and which we will test empirically), employment does not drop prior to the qualifying date and increases after that date only due to lower labor costs. Thus, analyzing employment responses to concurrent or retroactive credits provide a clean read on the true effectiveness of JCTCs, whereas employment responses to credits with implementation lags may overstate their effectiveness.

3. Data

3.1 Identifying And Dating Job Creation Tax Credits

We identify states offering Job Creation Tax Credits (JCTC) in three steps. First, we use Rogers (1998) to identify state JCTCs in place as of 1997. Second, *Site Selection*'s website (www.siteselection.com) contains tables documenting various state tax incentives from 1997 onward. Third, we supplement these sources with, for each state, a general web search for "tax credits" and a more targeted search in the legal database *WestLaw*.

Having identified all 23 states that have or have had a JCTC (all of these 23 states still have a JCTC as of Oct. 2010), we then use *WestLaw* to obtain the state statute code for the legislation associated with the JCTC. The state statute code identifies the Session Law that includes the bill signed into law, officially authorizing the credit. States session laws and bills are found either in *WestLaw* or on the state's house/assembly website. These bills contain all of the relevant information on each JCTC needed for this paper. (These bills are available from the authors upon request.)

3.2 The General Design of Job Creation Tax Credits

We start with a description of the general design of state JCTCs.[2] As mentioned above, 23 states have a broad JCTC with little or no restriction on eligible industries.[3] The details of these credits vary widely, but their basic designs are quite similar.

All JCTCs currently in place are intended to subsidize net job creation by businesses. That is, only new jobs that expand a business' total payroll employment level qualify for the tax credit. With many state JCTCs, a firm can only claim the credit if the number of jobs and/or wages associated with new jobs are above specified thresholds and meet certain other requirements, such as providing health insurance. In order to target net job creation, the thresholds tend to be defined on a "rolling basis" – the initial threshold is based on previous levels of employment or wages and future thresholds are increased to reflect recent hires. Some

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[2] This description is based largely on the information provided in Wilson and Notzon (2009).

[3] Georgia is an exception because only jobs in manufacturing are eligible for the credit. Results presented in Table 4.C do not suggest any anomalous behavior. California and New Jersey have extremely narrowly targeted JCTC, and they were excluded from our dataset.

states offer multiple credit rates that increase with the number of or wages associated with the added jobs.

JCTCs are credits against a state's corporate income or franchise tax and use one of three basic structures. In most states, the credit is a percentage of the total annual wages or compensation of the new jobs. In a number of other states, the credit is a percentage of the state income tax withholdings associated with the new jobs. The credit in a few other states, as well as the federal JCTC proposed by President Obama during the transition (but not adopted), is a fixed dollar amount per new job (e.g., $1000 in Virginia). The president proposed a $3000 federal credit.

State JCTCs differ with regard to how many years a corporation can apply the credit for a given hire against taxable income. Multi-year credits are intended to encourage future job retention in addition to current job creation.

An important characteristic of JCTCs is whether the credit is valuable to firms with no current tax liability. Refundability (a firm receiving payment from the state even if there is no tax liability) and carry-back/carry-forward provisions (a firm being able to apply a current year credit against past or future tax liabilities) are important considerations in gauging a credit's fiscal cost and effectiveness. While President Obama proposed a refundable credit, very few JCTCs are refundable. However, many states allow firms to carry back or forward the credit to reduce tax liabilities in other years.

3.3 The Timing of Job Creation Tax Credits

In any event study, the accurate timing of the event is crucial to properly identifying the relationship between the effective date for the tax credit and the employment outcome of interest. The "effective date," referred to as t_i^E above, is the earliest moment at which firms both know with reasonable certainty that the credit will be enacted (which we assume occurs on the signing date) and can act on that information to receive the credit (which we assume occurs on the qualifying date). The legislative bills that we obtain for every JCTC state indicate both the qualifying date and the signing date of the credits.

There is considerable variation among states in whether the signing date comes before, after, or at the same time as the qualifying date. This variation, based on daily data, is shown in

Chart 4. We classify the states into three regimes. There are eight **“implementation states”** for which the signing date precedes the qualifying date. This implementation interval ranges from one to ten months. In these states, any increase in employment caused by the credit should occur after the qualifying date. However, since the qualifying date should be perfectly anticipated during the implementation interval, firms might delay hiring that would otherwise have occurred until after the qualifying date. Such an adverse fiscal policy channel has been referred to as “Ashenfelter dips” in the labor literature or “fiscal foresight” in the macroeconomics literature. We will refer to this channel as “perfect fiscal foresight (to distinguish it from the possibility of “imperfect fiscal foresight” described below).

There are nine **“concurrent states”** for which the signing and qualifying dates are either the exact same day or the difference between these two dates is less than 15 days. For this latter class of states, we assign the signing date/qualifying date to the calendar month of the qualifying date.

There are six **“retroactive states”** for which the qualifying date precedes the signing date. For each retroactive state, the qualifying date is January 1 of the year in which the credit was signed into law. In all cases, only net employment increases made after January 1 can qualify for a credit. If a retroactive credit stimulates employment, the increase should occur at the time of signing.

For each regime, we recognize the possibility that, in some instances, firms may have some anticipation of passage of a JCTC prior to the effective date. We refer to this channel as imperfect fiscal foresight – imperfect in the sense that firms see a positive, but less than 1, probability of future passage of a JCTC. For the implementation and concurrent regimes, imperfect fiscal foresight may occur before the signing date. For the retroactive regime, imperfect fiscal foresight may occur before the qualifying date. Our empirical tests will examine the empirical importance of this channel during the “pre-date 1” interval. Note that the policy discussions and legislative history (especially the critical role of the House/Senate Conference Committee) surrounding the Carter tax credit (as documented by Sunley, 1980) suggest that the probability of passage was well below one until the final moment of passage and hence that, at least for this particular tax credit, imperfect fiscal foresight was not likely of much empirical importance.

3.4 Employment Data

The empirical work reported here is based on monthly, seasonally adjusted, private, non-farm employment data for the period January 1990 to September 2009. The earlier date is the first month in which these data are published. The latter date is chosen because it is the latest month that reflects information from state administrative records (based on unemployment insurance) and is no longer subject to revisions by the Bureau of Labor Statistics.

4. Empirical Results

4.1. Properties of the Monthly Employment Data

Before analyzing the impact of JCTCs, we first determine the statistical properties of $L_{i,t}$,. We estimate the following two models with an eye toward examining the persistence properties of the series:

$$L_{i,t} = \tilde{\alpha}_i + \beta_t + \sum_{j=-12}^{12} \gamma_j D_{i,t-j} + \tilde{\varphi}_i t + \tilde{\phi}_i t^2 + \sum_{j=1}^{24} \lambda_j L_{i,t-j} + \varepsilon_{i,t} \ , \tag{20}$$

$$\Delta L_{i,t} = \alpha_i + \beta_t + \sum_{j=-12}^{12} \gamma_j D_{i,t-j} + \phi_i t + \sum_{j=1}^{24} \lambda_j \Delta L_{i,t-j} + \varepsilon_{i,t} \ , \tag{21}$$

$$\Lambda_J \equiv \sum_{j=1}^{J} \lambda_j \tag{22a}$$

$$\varepsilon_{i,t} = \rho_i \ \varepsilon_{i,t-1} + \nu_{i,t} \ , \tag{22b}$$

where $L_{i,t}$, is the logarithm of employment data for state i in time period t, $\Delta L_{i,t}$, is the growth rate in employment, α_i is a state fixed effect, β_t is a time fixed effect, $\tilde{\varphi}_i$ and $\tilde{\phi}_i$ are state-specific time trends, and ε_t is a white-noise error term.[4] (In this version of the paper, we have not estimated $\tilde{\varphi}_i$ and $\tilde{\phi}_i$.) $D_{i,t}$ is a dummy variable taking a value of 1 when the JCTC becomes effective, which we define to be the latter of the signing and qualifying months. The coefficient on this dummy identifies the increase or decrease in employment growth during the first month in which businesses BOTH know that a credit has been enacted (i.e., signed into law) and can start making qualifying hires (i.e., after the qualifying date). Twelve leads and lags of the JCTC dummy are included, in addition to its contemporaneous value. Lagged dependent variables are entered for up to J = 24 months and are parameterized by the individual λs and their sum, Λ_J. In principle, it is important to include the lagged dependent variable to account for the persistence in employment.

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[4] The parameters in equations (20) and (21) are the same save the α_i and ϕ_i Notice that $\alpha_i = \tilde{\varphi}_i - \tilde{\phi}_i$ and $\phi_i = 2\tilde{\phi}_i$

The results of estimating equations (20) and (21) for various values of J are presented in Tables 1 and 2, respectively. For the log levels equation, when J = 1, the value of $\Lambda = \lambda$ (column 2) is close to 1.0. For larger values of J (additional lags of the dependent variable), Λ is greater than one, and the log levels equation would not appear to be a suitable specification for $L_{i,t}$. Table 2 contains estimates of the growth rate equation. Even for J = 1, serial correlation is absent (columns 6 and 7) and the coefficients on the lagged dependent variables (column 2are close to zero for small values of J. For larger values of J, the coefficient on the additional lagged dependent variable becomes statistically significant, but the near constancy of the R^2 suggests that there is little additional explanatory power provided by these additional lags. Taken together, these results strongly suggest that $L_{i,t}$ is best modeled as a simple growth rate.

This initial suggestion is confirmed by a formal unit root test. To assess stationarity, we use the panel unit root test recently proposed by Pesaran (2007) that extends the standard augmented Dickey-Fuller test to allow for cross-sectional dependence. For $L_{i,t}$, we estimate the following auxiliary equations,

$$\Delta L_{i,t} = a_i + b_i L_{i,t-1} + \bar{b}_i \bar{L}_{t-1} + \sum_{j=1}^{J'} d_{i,j} \Delta L_{i,t-j} + \sum_{j=0}^{J'} \bar{d}_{i,j} \overline{\Delta L}_{t-j} + g_i t + e_{i,t} , \tag{23a}$$

$$\varepsilon_{i,t} = \rho_i \varepsilon_{i,t-1} + \nu_{i,t} , \tag{23b}$$

$$\mu_b = \sum_{i=1}^{48} b_i / 48 , \tag{23c}$$

where the critical values for μ_b are provided in Pesaran's Tables II.b and II.c for tests without and with a time trend (g_i t in the above equation), respectively. The lag length for the lagged dependent variable (J') is determined by the need to absorb any serial correlation in the errors. The estimated values of μ_b are well below these critical values and serial correlation is absent. These tests indicate that there is a unit root in the monthly series for $L_{i,t}$ that is best modeled as follows,

$$\Delta L_{i,t} = \alpha_i + \beta_t + \sum_{j=-12}^{12} \gamma_j D_{i,t-j} + \varepsilon_{i,t} . \tag{24}$$

Though equation (24) controls for state-specific and aggregate fixed effects, it is also useful to control for any employment changes in a state driven by the state's exposure to particularly fast-growing or slow-growing industries. For example, even in absence of any employment-inducing fiscal policies, a state with a large IT industry during the late 1990s was likely to experience rapid employment growth during that period. One way to control for industry-driven employment changes is to first predict a state's year-over-year employment growth rate by calculating a weighted-average across industries of the national (excluding own-state) employment growth rates (year-over-year), where the weights are the state's employment shares in each industry. Multiplying this predicted annual growth rate by the level of employment in period t - 12 yields a predicted level of employment in period t.[5] Given that our empirical model is stated in terms of monthly growth rates (based on the unit root tests above), we therefore add the monthly growth rate of this predicted employment variable, $\Delta L^P_{i,t}$, to our baseline specification,

$$\Delta L_{i,t} = \alpha_i + \beta_t + \zeta \Delta L^P_{i,t} + \sum_{j=-12}^{12} \gamma_j D_{i,t-j} + \varepsilon_{i,t} \ . \tag{25}$$

Somewhat surprisingly, we find that the estimated coefficient on $\Delta L^P_{i,t}$ is very small ($\zeta = 0.02$) and not statistically different from zero (p-value of 0.157). The $\Delta L^P_{i,t}$ variable is strongly correlated with $\Delta L_{i,t}$; the correlations across states range from 0.1 to 0.7 with most correlations near 0.5 and with all but two of the correlations statistically different from zero. However, $\Delta L^P_{i,t}$ is also strongly correlated with the month fixed effects that capture the impact of aggregate factors. A regression of $\Delta L^P_{i,t}$ on state and time fixed effects yields an R^2 of 0.37; when the state fixed effects are omitted, the R^2 falls only slightly to 0.32. Thus, the information from the $\Delta L^P_{i,t}$ is largely captured by the time fixed effect. Nonetheless, we continue to include $\Delta L^P_{i,t}$ as a control variable in the results reported below.

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[5] This variable was introduced by Bartik (1991) and is frequently referred to as the "Bartik mix variable."

The object of our analysis is the estimation of the γ_i coefficients. However, we do not have sufficient information in our dataset to identify each of the individual coefficients. Instead, as discussed in Section 2 and shown in Chart 3, we divide our period of interest into the following four mutually exhaustive and exclusive intervals for a given state:

1) Pre-Date 1: the interval from 12 months before the "effective" month (when $D_{i,t} = 1$, which occurs in the later of the signing and qualifying months) to the earlier of the signing and qualifying months. The average lengths of the Pre-Date 1 intervals are 6.76, 12. and 8 months for the implementation, concurrent, and retroactive regimes, respectively;

2) Inter-Date: the interval between the signing and qualifying months. The length of the Inter-Date interval varies by state. The average lengths of the Inter-Date intervals are 5.24, 0, and 4 months for the implementation, concurrent, and retroactive regimes, respectively;

3) At Date 2: the "effective" month;

4) Post-Date 2: the 12 month interval after the "effective" month.

and estimate the following equation for all 48 states,

$$\Delta L_{i,t} = \alpha_i + \beta_t + \zeta \Delta L^P_{i,t} + \varepsilon_{i,t} \tag{25}$$

[8 Implementation Regime States, $t_i^S < t_i^Q$]

$$\sum_{i=1}^{8} \left\{ \sum_{j=t_i^Q-12}^{t_i^S} \gamma^{I,\text{Pre-Date}1} D_{i,t-j} + \sum_{j=t_i^S}^{t_i^Q} \gamma^{I,\text{Inter-Date}} D_{i,t-j} + \gamma^{I,\text{At Date}} D_{i,t_i^Q} + \sum_{j=t_i^Q}^{t_i^Q+12} \gamma^{I,\text{After-Date }2} D_{i,t-j} \right\}$$

[9 Concurrent Regime States, $t_i^S = t_i^Q$]

$$\sum_{i=1}^{9} \left\{ \sum_{j=t_i^Q-12}^{t_i^Q} \gamma^{C,\text{Pre-Date}1} D_{i,t-j} + \gamma^{C,\text{At Date}} D_{i,t_i^Q} + \sum_{j=t_i^Q}^{t_i^Q+12} \gamma^{C,\text{After-Date }2} D_{i,t-j} \right\}$$

[6 Retroactive Regime States, $t_i^Q < t_i^S$]

$$\sum_{i=1}^{6} \left\{ \sum_{j=t_i^S-12}^{t_i^Q} \gamma^{R,\text{Pre-Date}1} D_{i,t-j} + \sum_{j=t_i^Q}^{t_i^S} \gamma^{R,\text{Inter-Date}} D_{i,t-j} + \gamma^{R,\text{At Date}} D_{i,t_i^S} + \sum_{j=t_i^S}^{t_i^S+12} \gamma^{R,\text{After-Date }2} D_{i,t-j} \right\}$$

where the first superscript on the γ coefficients refer to the implementation (I), concurrent (C), or retroactive (R) regime and the second superscript on the γ coefficients to one of the four intervals listed immediately above. Note that the γ coefficients are fixed over the time periods constituting each interval and across the states constituting a regime. In this specification, the 25 non-JCTC states serve as the control group.

2. Preliminary Estimates – By Regime

The preliminary results from estimating equation (25) are presented in Table 4 and are strikingly consistent with the theoretical predictions summarized in Table 3. Panel A reports the γ coefficients and the associated standard errors and p-values when the γ's are constrained to generate a constant monthly effect. These coefficients represent the average effect over the interval. It proves more convenient to interpret these coefficients in terms of the total effect over the interval. We thus multiply the estimated coefficient by the average interval length for each regime.

The results for the implementation regime indicated a positive impact on employment growth of 0.23% in the month (Date 2) that firms can qualify for the JCTC. The remaining entries are negative. Employment growth falls immediately after the qualifying date as firms adjust to a lower credit (due to the rolling base) and the absence of an inventory restocking motivation. Employment growth also falls in the Inter-Date interval between the qualifying and signing dates. This decrease is nearly twice as large as the Date 2 increase and documents the quantitative importance of fiscal foresight/Ashenfelter dips. Lastly, employment growth also falls before the signing date, and thus suggests that firms are also exercising imperfect fiscal foresight. All of these entries are statistically significant at the 10% level.

The sum of the four cell entries is -1.43%, indicating that the JCTC lowered employment growth. This counterintuitive result needs further exploration. One contributing factor is the long lags that are used in the Pre-Date 1 and Post-Date 2 intervals. The length of these lags is determined by assumption, not tested in terms of a statistical criterion. Such testing will be undertaken in the next draft of this paper.

The results for the concurrent period provide a better "experiment" for assessing the impact of the JCTCs because they are not affected by a perfect fiscal foresight channel. The increase in employment growth at the qualifying date is 0.08%. Comparing this estimate to the comparable figure for the implementation regime of 0.23% indicates that about two-thirds of the employment stimulus for implementation regime states is due to a catch-up effect due to perfect fiscal foresight and inventory restocking. In contrast to the results for the implementation regime, the coefficients for the Pre-Date 1 and Post-Date 2 intervals are not distinct from zero. These results suggest that a JCTC with an implementation lag contributes to volatility in employment growth.

The third row of Panel A contains results for the retroactive regime. The signs of the responses are the same as for the implementation regime and, consistent with theory, are all lower in absolute value. All of the coefficients are now statistically close to zero, including the coefficient for the At Date 2 interval.

A parallel set of results is presented in Panel B under the restriction that the total effect (as opposed to the average effect) over the interval is constant across states. The estimates are very similar to those reported in panel A, with the exception that the coefficients for the implementation regime are estimated less precisely.

3. Preliminary Estimates – By State

The results in the above sub-section are based on grouping JCTC states into one of three regimes and estimating coefficients that are the same for all states within a regime. In this sub-section, we relax this restriction by allowing the γ coefficients appearing in equation (25) to vary by state.[6]

The results for the three regimes are presented in Table 5. For states in the implementation and concurrent regimes, the results are displayed in panels A and B, respectively, and are similar to those in Table 4, where all implementation or concurrent states had the same coefficients. The coefficients At Date 2 are generally positive and their average is close to that of the constrained estimates. The Post-Date 2 estimates continue to be large and negative for implementation states. (Future work will relate the statistical significance of the state-specific γ coefficients to the detailed characteristics of the JCTCs.)

The results for states in the retroactive regime differ from those in Table 4. For the At Date 2 coefficients, five of the six state-specific coefficients are statistically significant in panel C of Table 5, and two of these estimates are negative. This heterogeneity may be partly response for the imprecisely estimated constrained coefficient in Table 4.

In sum, the preliminary results displayed in Tables 4 and 5 suggest a positive answer to the *whether* question. Regarding *when*, there is some evidence of anticipation effects when firms face an implementation period. Our overall assessment is that an unanticipated JCTC leads to an increase in employment growth of 0.10% during the first month after the credit goes into effect. This figure may be biased upward if firms "game" the tax credit program by "artful" hiring/firing decisions. There is some evidence of this behavior. Lastly, assessing the *where* question requires the inclusion of additional regressors that capture the effects of JCTCs enacted in neighboring states. This extension will be pursued in future work.

5. Prior Literature and Policy Implications

A. Prior Literature

A job tax credit has been tried only once before at the U.S. federal level, the 1977-1978 "New Jobs Tax Credit" (NJTC). Sunley (1980) offers a detailed description of the convoluted policy discussions and legislative history surrounding the eventual enactment of the NJTC. It is

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[6] We implement this less restricted model by interacting the γ coefficients with α_s for JCTC states.

particularly important to note that crucial details of the NJTC were not determined until the end of the process in the House/Senate Conference Committee and thus would have been difficult to anticipate. The NJTC offered corporations with taxable income a credit whose value was proportional to the increase in the corporation's net payroll employment level above 102% of its previous year's employment level.

The effectiveness of the NJTC has been discussed in three studies. Using survey data in a cross-section regression, Perloff and Wachter (1979) find that firms that reported knowing about the credit experienced 3% higher employment growth than other firms. Bishop (1981) also studies the employment effects of the NJTC but with time series data for several industries likely to be responsive to the NJTC. He reports that the NJTC increased employment in the Construction, Trucking, Wholesale, and Retail sectors in 1977-1978 by between 0.66% and 2.95%. As in the Wachter study, the effects of the NJTC are measured by a variable reflecting the percentage of firms aware of the tax credit. By contrast, Sunley (1980, p. 408) concludes that the effects of the NJTC were "slight" because of the complexity of the law and delays between hiring decisions by firms and eligibility determination by regulators.

There are two other studies that have quantified the effects of marginal tax credits. Kesselman, Williamson, and Berndt (1977) estimate a translog cost function and report that, for equal revenue costs and hypothetical policies, the percentage increase in employment from a marginal employment tax credit is about twice as great as the comparable increase from a uniform employment tax credit. Faulk (2002) examines an incremental job tax credit in Georgia. With cross-section data, she estimates separate employment equations for eligible firms that are participating or non-participating in the Georgia program and a probit selection equation to determine participation. For those eligible firms participating in the program, employment rose by between 23 to 28 percent. The cost was between \$2280 and \$2680 per job created.

B. Policy Implications

Owing to the different empirical approaches and data, it is difficult to compare the estimates in the current paper to the prior literature. An alternative perspective on our results can be obtained with the following thought experiment. If a federal JCTC were to be passed today and it had the same impact on employment that we estimate for states (0.10%, based on those states whose JCTCs do not have a gap between legislative and qualifying dates), the credit would create 140,000 net new jobs. This would have a very modest impact on the unemployment rate and is small compared with the 640,000 new jobs attributed to the Stimulus Plan.

A second computation assumes that all of the 0.10% increase in employment growth is drawn from the ranks of the unemployed. In this case, an unemployment rate of 9.6% would fall to 9.4%. At the other extreme, if all of the employment growth is met by an increase in labor force participation, the fall in the unemployment rate would be only 0.1%.

These calculations could be affected by at least two biases. All of the state JCTCs investigated in this paper are permanent. If the hypothetical national JCTC was expressly temporary, the expected expiration date of the job credit would enhance hiring incentives. A perennial challenge with JCTCs that reward marginal hiring decisions is that the unobservability of the counterfactual path that would have occurred in the absence of tax credits. Policymakers are aware of this difficulty and the temptation for firms to "game" the tax credit program by "artful" hiring/firing decisions.[7] Insofar as firms are successful, these gaming activities will lead to a small net increase in employment evaluated over several months.

6. Future Work

This draft is very preliminary. In further drafts, we plan to undertake the following work:

- Add an additional control for employment, state-specific time trends:
- Choose the length of the Pre-Date 1 and Post-Date 2 intervals with a formal statistical criterion.
- Estimate the Post-Date 2 lead with the PDL specification sketched in the Appendix.
- Assess the endogeneity of JCTC adoption decisions in probit and logit frameworks with economic, political, and labor market variables. A significant role for the latter variables will require additional controls in the estimating equation.
- Compute, for each state, the approximate value of its JCTC to a representative firm and use this measure to test whether more valuable credits have larger employment impacts. While

[7] Bartik (2001) discusses design issues aimed at attenuating gaming behavior.

this measure is theoretically preferred to the qualitative variables used in the current draft, the value of the credits is difficult to estimate and may lead to measurement error.

- Assess the "bang for the buck" of each state's credit using the data mentioned in the above point;
- Estimate the effect of out-of-state JCTCs on in-state employment by adding an additional regressor to the model. This regressor is a spatial-weighted average of JCTC's in the 47 other states. The coefficients on this variable will allow us to assess our "where" question regarding JCTCs.

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Glossary:

Term	Definition
Concurrent Regime (C)	A JCTC in which $t_i^S = t_i^Q$.
Effective date/month (t_i^E)	Later of the signing and qualifying dates/months in state i.
Employment (L_{it})	Logarithm of the level of total nonfarm employment in state i and month t.
Imperfect fiscal foresight	The potential phenomenon whereby economic agents (especially employers) believe there is a positive, but less than 1, probability that a JCTC will be passed in the near future (prior to signing). Also known as pre-signing anticipation. See Line segment ZA in Chart 4.
Implementation Interval	Interval between signing date/month and qualifying date/month, if $t_i^S < t_i^Q$.
Implementation Regime (I)	A JCTC with an implementation period.
Inventory overshooting effect	JCTC-induced response of employment that occurs on the effective date and reflects the accumulation of inventory that compensates for prior draw downs and/or reflects intertemporal substitution in the face of temporarily lower labor costs. Line segment DE in Chart 3.
JCTC-event dummy (D_{it})	Dummy variable equal to 1 if $t = t_i^E$; 0 otherwise.
Long-run effect ("True")	JCTC-induced response of employment between the time the tax credit becomes effective to one year later. Line segment CF (equal to line segment AF) in Chart 3.
Perfect fiscal foresight	The phenomenon whereby economic agents know with probability 1 that a JCTC will go into effect on a known date in the future. This situation only occurs during the period between the signing date and the qualifying date for credits with implementation periods. Also known as "Ashenfelter Dips." See line segment AB in Chart 4.
Qualifying date/month (t_i^Q)	Earliest date/month at which time a new hire may qualify for a JCTC in state i.
Rebound effect	JCTC-induced response of employment that occurs on the effective date and compensates for the anticipation effects. Line segment BC in Chart 3. Note that Point C has the same value as Point A.
Retroactivity Period	Period between qualifying date/month and signing date/month, if $t_i^S > t_i^Q$.
Retroactivity Regime (R)	A JCTC with a retroactivity period.

Short-run effect ("True")	JCTC-induced response of employment that occurs on the effective date, net of the rebound effect. Line segment CD in Chart 3.
Signing date/month (t_i^S)	Date/month at which the governor in state i officially signs or enacts JCTC legislation into law.

Appendix: Constraining the γ Coefficients With Polynomial Distributed Lags

In order to aid in the interpretation of the estimated γ's and to conserve degrees of freedom, we can estimate an alternative model containing the following polynomial distributed lag (PDL) structure (Almon, 1965),

$$\gamma_j = \pi_0 + \pi_1 j + \pi_2 j^2 + \pi_3 j^3 \tag{A-1}$$

and (25) becomes,

$$\Delta L_{i,t} = \alpha_i + \beta_t + \zeta \Delta L^P_{i,t} + \sum_{j=-12}^{12} (\pi_0 + \pi_1 j + \pi_2 j^2 + \pi_3 j^3)\, D_{i,t-j} + \varepsilon_{i,t} \tag{A-2}$$

→

$$\begin{aligned} \Delta L_{i,t} &= \alpha_i + \beta_t + \zeta \Delta L^P_{i,t} + \varepsilon_{i,t} \\ &\quad + \pi_0 \sum_{j=-12}^{12} D_{i,t-j} + \pi_1 \sum_{j=-12}^{12} j * D_{i,t-j} + \pi_2 \sum_{j=-12}^{12} j^2 * D_{i,t-j} + \pi_3 \sum_{j=-12}^{12} j^3 * D_{i,t-j} \end{aligned} \tag{A-3}$$

Note that the sums in the bottom row are just new, transformed model variables. Thus the PDL model is still linear.

→

$$\begin{aligned} \Delta L_{i,t} &= \alpha_i + \beta_t + \zeta \Delta L^P_{i,t} + \varepsilon_{i,t} \\ &\quad + \pi_0 P_{0,i,t} + \pi_1 P_{1,i,t} + \pi_2 P_{2,i,t} + \pi_3 P_{3,i,t} \end{aligned} \tag{A-4}$$

$$P_{0,i,t} = \sum_{j=-12}^{12} D_{i,t-j},\quad P_{1,i,t} = \sum_{j=-12}^{12} j D_{i,t-j},\quad P_{2,i,t} = \sum_{j=-12}^{12} j^2 D_{i,t-j},\quad P_{3,i,t} = \sum_{j=-12}^{12} j^3 D_{i,t-j},$$

Separate polynomial lags can be estimated before, at, and after the event date,

$$
\begin{aligned}
\Delta L_{i,t} &= \alpha_i + \beta_t + \zeta\,\Delta L^{P}_{i,t} + \varepsilon_{i,t} \\
&\quad + \pi^B_0 P^B_{0,i,t} + \pi^B_1 P^B_{1,i,t} + \pi^B_2 P^B_{2,i,t} + \pi^B_3 P^B_{3,i,t} \\
&\quad + \gamma_0 D_{i,t} \\
&\quad + \pi^A_0 P^A_{0,i,t} + \pi^A_1 P^A_{1,i,t} + \pi^A_2 P^A_{2,i,t} + \pi^A_3 P^A_{3,i,t} \qquad \text{(A-5)}
\end{aligned}
$$

$$
P^B_{0,i,t} = \sum_{j=-12}^{1} D_{i,t-j},\quad P^B_{1,i,t} = \sum_{j=-12}^{1} j\,D_{i,t-j},\ P^B_{2,i,t} = \sum_{j=-12}^{1} j^2\,D_{i,t-j},\ P^B_{3,i,t} = \sum_{j=-12}^{1} j^3\,D_{i,t-j},
$$

$$
P^A_{0,i,t} = \sum_{j=1}^{12} D_{i,t-j},\quad P^A_{1,i,t} = \sum_{j=1}^{12} j\,D_{i,t-j},\quad P^A_{2,i,t} = \sum_{j=1}^{12} j^2\,D_{i,t-j},\quad P^A_{3,i,t} = \sum_{j=1}^{12} j^3\,D_{i,t-j},
$$

Models of the growth rate of employment will not be estimated with this polynomial distributed lag structure in this draft.

**Chart 1: Number Of States With A JCTC In Effect
January 1990 To August 2009**

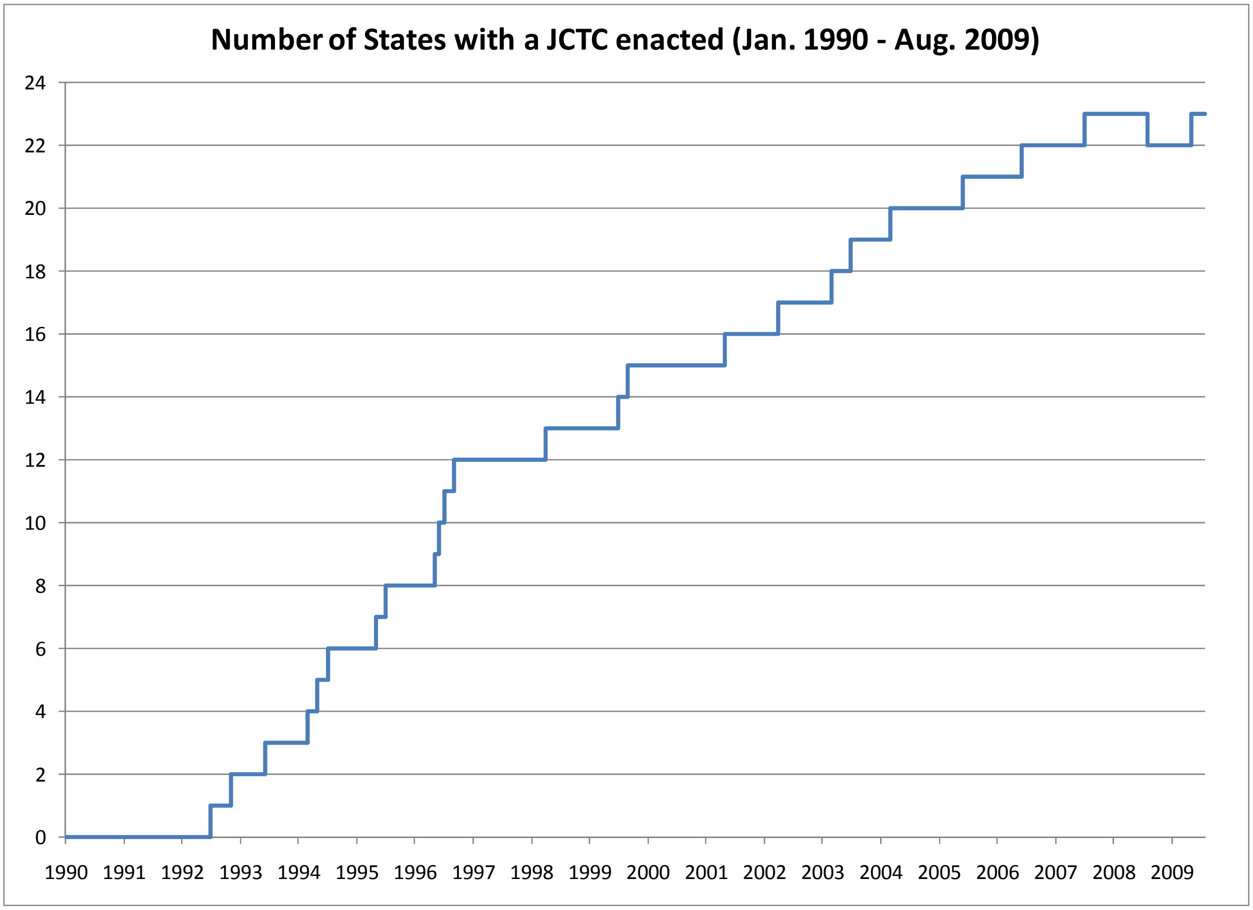

Chart 2: Map Showing States With JCTCs As Of August 2009

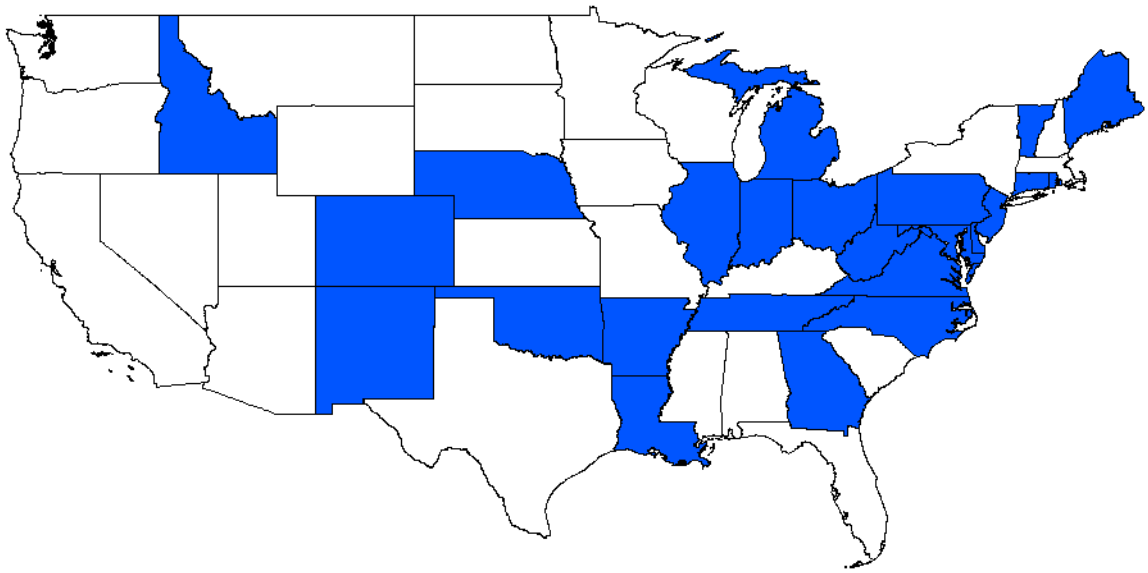

Chart 3: Theoretical Predictions of the Path of Employment around a JCTC "event"

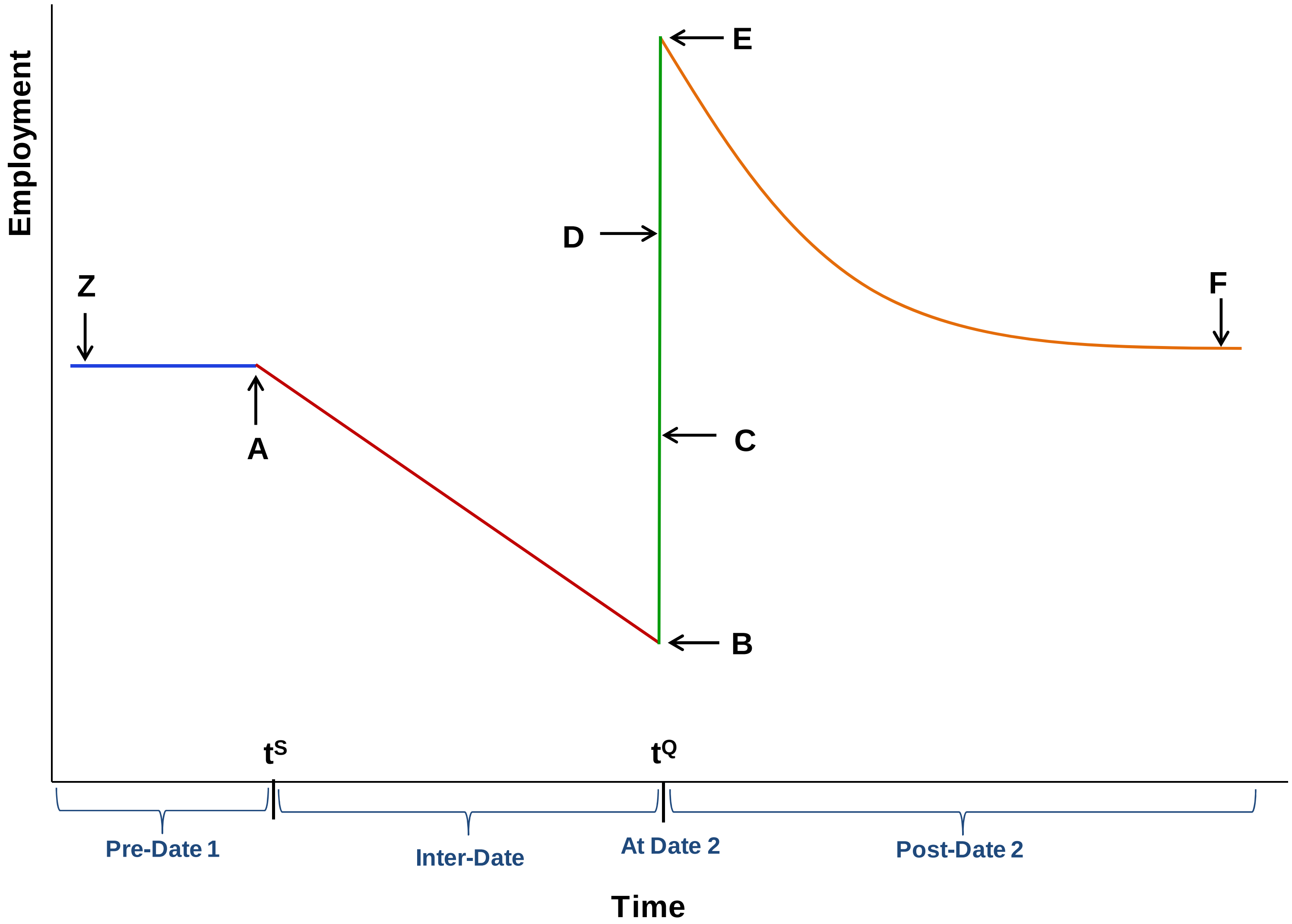

Chart 4: Time From Signing Date (t^S) To Qualifying Date (t^Q) For JCTCs

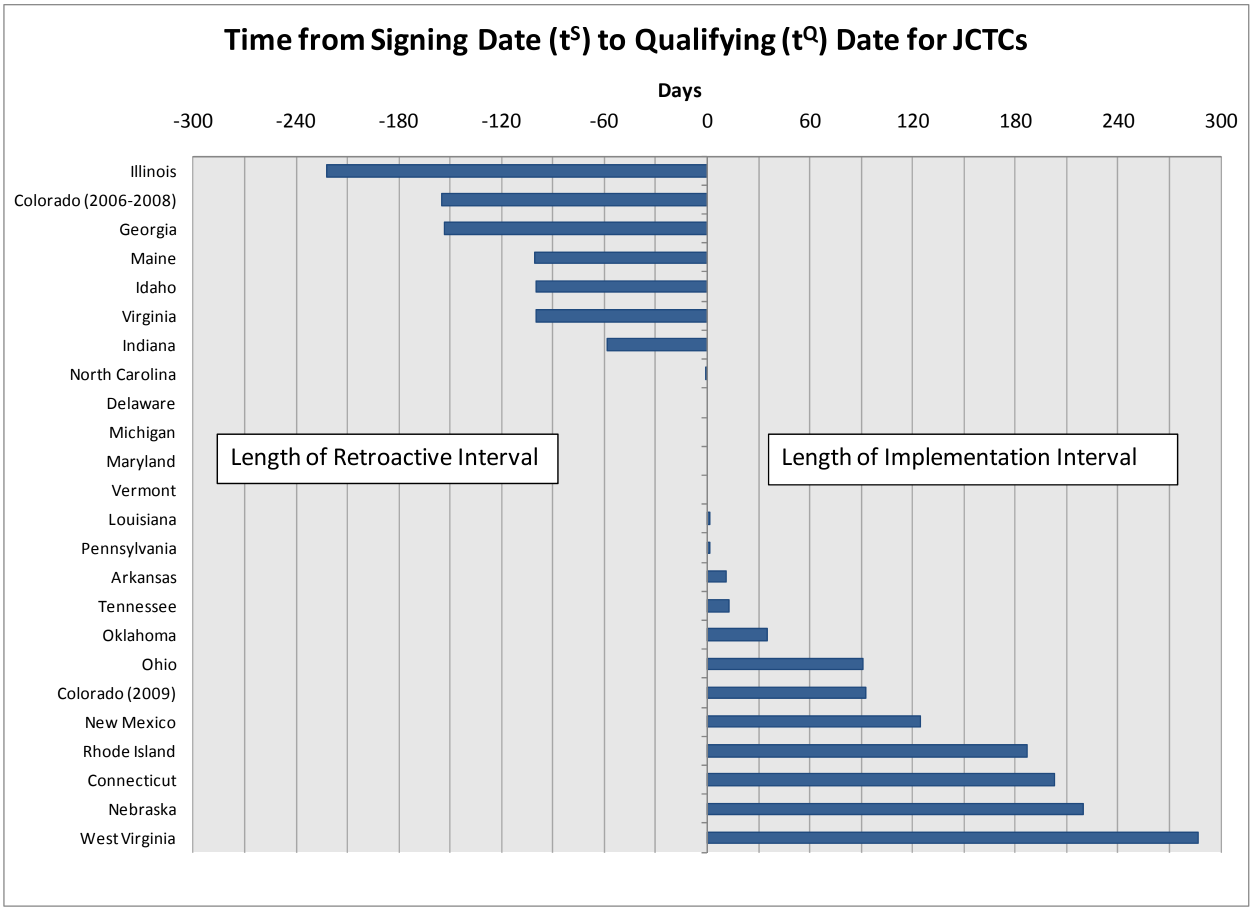

Table 1: Estimates Of Log Levels Model, Equations (20) And (22), For Various Values of J

J (1)	Λ (2)	σ_Λ (3)	R^2 (4)	t-test, Jth Lag (5)	ρ (6)	σ_ρ (7)
1	0.9734	0.0026	0.9799	368.3510	-0.0200	0.0104
2	0.9733	0.0026	0.9798	0.7466	-0.0001	0.0105
3	1.0236	0.0290	0.9796	-2.2351	-0.0026	0.0105
4	1.1120	0.0580	0.9796	-3.7660	-0.0031	0.0105
5	1.1790	0.0887	0.9794	-2.4392	-0.0011	0.0105
6	1.2290	0.1174	0.9792	-1.6640	-0.0014	0.0106
7	1.3095	0.1491	0.9792	-3.8106	-0.0058	0.0106
8	1.3269	0.1524	0.9791	-5.0839	-0.0027	0.0106
9	1.3285	0.1525	0.9791	-3.4147	-0.0027	0.0107
10	1.3256	0.1561	0.9790	-3.3215	-0.0050	0.0107
11	1.3408	0.1595	0.9790	-3.8486	-0.0019	0.0107
12	1.3442	0.1666	0.9787	-3.7749	-0.0031	0.0107
13	1.3708	0.1735	0.9791	-5.8905	-0.0050	0.0108
14	1.3842	0.1826	0.9791	-3.1304	-0.0025	0.0108
15	1.3920	0.1841	0.9791	-1.8741	0.0019	0.0108
16	1.3651	0.1819	0.9792	-1.3978	0.0050	0.0109
17	1.3349	0.1814	0.9792	-0.6700	0.0004	0.0109
18	1.3493	0.1807	0.9792	-2.2196	0.0014	0.0109
19	1.3218	0.1837	0.9791	0.8036	-0.0002	0.0109
20	1.3139	0.1876	0.9791	0.2351	0.0010	0.0109
21	1.3127	0.1907	0.9791	-1.9911	-0.0057	0.0110
22	1.3550	0.1937	0.9791	-3.0220	-0.0005	0.0110
23	1.3538	0.1949	0.9791	-0.8346	-0.0024	0.0110
24	1.3543	0.1959	0.9788	-1.5869	-0.0005	0.0111

Notes To Table 1: Empirical results based on OLS estimation of equations (20) and (22) for the period 1990.1 to 2009.9. (The ψ_i and φ_i coefficients are constrained to zero in the runs reported here.) The γ_i coefficients are unconstrained. Fixed state and time effects are included. The states in the three regimes are listed in Chart 4 and Table 5. Column 1 is the lag length. Column 2 contains the summation of the lagged coefficients (Λ , equation (22a)). Column 3 contains the standard error associated with Λ. Column 4 contains the R^2. Column 5 contains the t-test for the null hypothesis that the coefficient on the J^{th} lag equals zero. Column 6 contains the first-order serial correlation coefficient (ρ, equation (22b)) for the residuals. Column 7 contains the standard error associated with ρ. Standard errors are heteroscedastic-consistent.

Table 2: Estimates Of Log Levels Model, Equations (21) and (22), For Various Values Of J

J (1)	Λ (2)	σ_Λ (3)	R^2 (4)	t-test, Jth Lag (5)	ρ (6)	σ_ρ (7)
1	-0.0301	0.0262	0.9219	-1.1464	0.0001	0.0104
2	-0.0080	0.0320	0.9224	1.4407	-0.0027	0.0105
3	0.0675	0.0479	0.9231	2.9601	-0.0032	0.0105
4	0.1661	0.0712	0.9234	1.6288	-0.0014	0.0105
5	0.2650	0.0981	0.9234	0.8529	-0.0010	0.0106
6	0.4059	0.1273	0.9240	2.9518	-0.0046	0.0106
7	0.5691	0.1563	0.9246	4.1367	-0.0019	0.0106
8	0.5619	0.1553	0.9251	2.2281	-0.0022	0.0106
9	0.5526	0.1552	0.9256	2.2609	-0.0046	0.0107
10	0.5801	0.1552	0.9260	2.4793	-0.0010	0.0107
11	0.6465	0.1565	0.9245	2.5308	-0.0015	0.0106
12	0.7110	0.1575	0.9241	4.7450	-0.0032	0.0108
13	0.8479	0.1660	0.9245	1.7417	-0.0018	0.0108
14	0.9625	0.1778	0.9252	0.3478	0.0020	0.0108
15	0.9338	0.1798	0.9261	-0.3202	0.0047	0.0109
16	0.9049	0.1802	0.9264	-0.8696	0.0006	0.0109
17	0.8939	0.1832	0.9266	0.7298	0.0009	0.0109
18	0.8775	0.1888	0.9270	-2.4762	-0.0012	0.0109
19	0.8552	0.1972	0.9275	-1.6053	0.0014	0.0109
20	0.7870	0.2107	0.9283	0.5502	-0.0039	0.0110
21	0.7676	0.2333	0.9286	1.6152	0.0009	0.0110
22	0.7863	0.2335	0.9291	-0.8505	-0.0011	0.0110
23	0.8016	0.2343	0.9279	-0.0158	0.0020	0.0111
24	0.7946	0.2403	0.9267	2.4864	0.0257	0.0111

Notes To Table 2: Empirical results based on OLS estimation of equations (21) and (22) for the period 1990.1 to 2009.9. (The φ_i coefficients are constrained to zero in the runs reported here.) The γ_i coefficients are unconstrained. Fixed state and time effects are included. The states in the three regimes are listed in Chart 4 and Table 5. Column 1 is the lag length. Column 2 contains the summation of the lagged coefficients (Λ , equation (22a)). Column 3 contains the standard error associated with Λ. Column 4 contains the R^2. Column 5 contains the t-test for the null hypothesis that the coefficient on the J^{th} lag equals zero. Column 6 contains the first-order serial correlation coefficient (ρ, equation (22b)) for the residuals. Column 7 contains the standard error associated with ρ. Standard errors are heteroscedastic-consistent.

Table 3: Expected Impacts Of JCTCs On Employment Growth By Regime, by Interval Theoretical Predictions

	Interval			
Regime	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Implementation	0 or -	-	++	-
Concurrent	0 or -	N/A	+	0 or -
Retroactive	0 or -	0 or +	+	0 or -

Notes To Table 3: Theoretical predictions based on the analysis in Section 2. N/A: not applicable.

Table 4: Expected Impacts Of JCTCs On Employment Growth
By Regime, by Interval
Equation (25)

Panel A. Constant Monthly Effect Over Each Interval

	Interval			
Regime	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Implementation	-.0046246 (.0022771) [0.042]	-.0043959 (.0023333) [0.060]	.0022626 (.0013405) [0.091]	-.0075519 (.0032932) [0.022]
Concurrent	-.0000652 (.0035291) [0.985]	N/A	.0007812 (.0005084) [0.124]	.0009547 (.0024688) [0.699]
Retroactive	-.0022731 (.0028154) [0.419]	-.0016925 (.0023422) [0.470]	.0002319 (.0004641) [0.617]	-.0017331 (.0027477) [0.528]

Panel B. Constant Total Effect Over Each Interval

	Interval			
Regime	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Implementation	-.0034289 (.0022763) [0.132]	-.0013527 (.0020024) [0.499]	.0022857 (.0013406) [0.088]	-.0072839 (.0032911) [0.027]
Concurrent	-.000068 (.0035296) [0.985]	N/A	.000779 (.0005082) [0.125]	.0009168 (.0024694) [0.710]
Retroactive	-.0019856 (.002592) [0.444]	-.0001344 (.0022179) [0.952]	.0002389 (.0004646) [0.607]	-.0016368 (.002748) [0.551]

Notes to Table 4: Empirical results based on OLS estimation of equation (25) for the period 1990.1 to 2009.9. The γ coefficients vary by interval and by regime. The coefficients in Panel A are multiplied by the average interval length, and thus can be interpreted as the total effect over the interval. Fixed state and time effects are included. The states constituting the three regimes are listed in Chart 4 and Table 5. The cells contain the point estimate, the standard error in parentheses, and the p-value in brackets for the null hypothesis that the coefficient equals zero. Standard errors are heteroscedastic-consistent. N/A: not applicable.

Table 5: Estimated Impacts of JCTCs on Employment Growth, By State, by Interval, for a Given Regime Equation (25)

Panel A: Implementation Regime

	Interval			
State	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Colorado	.0020212 (.0045913) [0.660]	-.003366 (.0023343) [0.117]	-.0007596 (.000519) [0.143]	-.0005503 (.0044315) [0.901]
Connecticut	-.0135378 (.0015748) [0.000]	-.0093792 (.0051869) [0.071]	.0092816 (.0005784) [0.000]	-.0317871 (.0065035) [0.000]
Nebraska	-.0024983 (.0051872) [0.630]	-.0049067 (.0047234) [0.229]	-.0014277 (.0005053) [0.005]	-.003717 (0.0061032) [0.543]
New Mexico	-.0021682 (.0044731) [0.628]	-.002811 (.0017824) [0.115]	-.0001389 (.0003648) [0.703]	-.0024841 (.006682) [0.710]
Ohio	-.0001341 (.0080147) [0.987]	.0005049 (.0035922) [0.888]	.0019727 (.0005723) [0.001]	.0044862 (.0086217) [0.603]
Oklahoma	-.0024357 (.0077705) [0.754]	.0019404 (.0003237) [0.000]	.0021262 (.0004424) [0.000]	-.0053917 (.0065938) [0.414]
Rhode Island	-.0127532 (.0056565) [0.024]	-.0110332 (.0115504) [0.339]	.0049643 (.0005851) [0.000]	-.017475 (.010093) [0.083]
West Virginia	.0009426 (.0013635) [0.489]	-.0004921 (.0007302) [0.500]	-.0007562 (.0004179) [0.070]	.0002721 (.00087) [0.754]

Table 5: Estimated Impacts of JCTCs on Employment Growth, By State, by Interval, for a Given Regime Equation (25) (continued)

Panel B: Concurrent Regime

	Interval			
State	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Arkansas	.0037658 (.0050692) [0.458]	N/A	-.0014033 (.0003516) [0.000]	-.0014153 (.0066143) [0.831]
Delaware	-.0098723 (.0074406) [0.185]	N/A	-.0004315 (.0005005) [0.389]	-.0020297 (.0105851) [0.848]
Louisiana	.0091261 (.0081494) [0.263]	N/A	.0007403 (.000711) [0.298]	-.0003803 (.0093863) [0.968]
Maryland	-.0096811 (.0225205) [0.667]	N/A	.0003096 (.0003322) [0.351]	.0091922 (.0048206) [0.057]
Michigan	.0166498 (.0056798) [0.003]	N/A	.002756 (.000491) [0.000]	.0071584 (.0079036) [0.365]
North Carolina	-.00251 (.010097) [0.980]	N/A	-.0001168 (.0006384) [0.855]	.0031376 (.0047467) [0.509]
Pennsylvania	-.005225 (.0118127) [0.658]	N/A	.0001453 (.0005752) [0.801]	.0006309 (.0039522) [0.873]
Tennessee	-.0054907 (.005922) [0.354]	N/A	.0013391 (.0004001) [0.001]	-.0050376 (.0044964) [0.263]
Vermont	-.0005291 (.0063187 [0.933]	N/A	.0035202 (.00397) [0.000]	-.0035768 (.0094069) [0.704]

**Table 5: Estimated Impacts of JCTCs on Employment Growth,
By State, by Interval, for a Given Regime
Equation (25)
(continued)**

Panel C: Retroactive Regime

	Interval			
State	Pre-Date 1 (1)	Inter-Date (2)	At Date 2 (3)	Post-Date 2 (4)
Georgia	-.0030595 (.0080633) [0.704]	-.0099379 (.0083804) [0.236]	.0008594 (.0003294) [0.009]	-.0068225 (.0084785) [0.421]
Idaho	.0047795 (.0074387) [0.521]	.0067461 (.0043557) [0.121]	-.001219 (.0003896) [0.002]	-.010062 (.010003) [0.314]
Illinois	-.000603 (.0031327) [0.847]	-.0083853 (.0060092) [0.163]	-.0009255 (.0002926) [0.002]	.0044462 (.0041512) [0.284]
Indiana	.0056828 (.0049090) [0.247]	.0017365 (.0022449) [0.439]	.0012819 (.0003226) [0.000]	.0093474 (.0061262) [0.127]
Maine	-.020655 (.0123579) [0.095]	.0031706 (.0091914) [0.730]	.000219 (.0004905) [0.655]	-.0039959 (.0092487) [0.666]
Virginia	-.0039841 (.0040641) [0.327]	-.0020634 (.0010153) [0.042]	.0021527 (.0004757) [0.000]	-.0034900 (.0045733) [0.445]

Notes to Table 5: Empirical results based on OLS estimation of equation (25) for the period 1990.1 to 2009.9. The γ coefficients vary by state. The coefficients are multiplied by the average interval length, and thus can be interpreted as the total effect over the interval. Fixed state and time effects are included. The cells contain the point estimate, the standard error in parentheses, and the p-value in brackets for the null hypothesis that the coefficient equals zero. Standard errors are heteroscedastic-consistent. N/A: not applicable.