

THE EMPLOYMENT EFFECTS OF STATE HIRING CREDITS

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State and federal policymakers grappling with the aftermath of the Great Recession sought ways to spur job creation, in many cases adopting hiring credits to encourage employers to create new jobs. Virtually no evidence is available, however, on the effects of these kinds of counter-recessionary hiring credits, with the only evidence coming from much earlier studies of the federal New Jobs Tax Credit in the 1970s. This article provides evidence on the effects of state hiring credits on job growth. Some specific types of hiring credits—including those targeting the unemployed, those that allow states to recapture credits when job creation goals are not met, and refundable hiring credits—appear to have succeeded in boosting job growth, particularly during the Great Recession period and perhaps also during recessions in general. At the same time, some evidence suggests that these credits can generate much more hiring than net employment growth, consistent with the credits encouraging churning of employees that raises the cost of producing jobs through hiring credits.

The Great Recession led to levels of job loss and unemployment that were the worst on record since the Great Depression (Elsby, Hobijn, and Sahin 2010; Martínez-García and Koech 2010). For most states, unemployment rates climbed to higher levels than in any postwar recession, and in general the high levels of unemployment reached during the Great Recession were more persistent than in past recessions (Pittelko 2011).

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Naturally, state and federal policymakers grappling with the aftermath of the Great Recession sought ways to spur job creation. Many states enacted hiring credits to encourage employers to create new jobs, and the Hiring Incentives to Restore Employment (HIRE) Act established a modest credit for most of 2010 at the federal level. Our goal in this article is to provide evidence on the effects on job growth of state hiring credits like those adopted during and after the Great Recession, using evidence from this period and earlier.

As summarized in Neumark (2013), some research literature argues that hiring credits are ineffective (Katz 1998; Dickert-Conlin and Holtz-Eakin 2000; Bartik 2001). Most of the evidence pointing to ineffective hiring credits, however, comes from programs that target the disadvantaged. By contrast, much less evidence exists on more broadly targeted or non-categorical hiring credits that explicitly try to incentivize job creation, especially during recessions. Essentially, the only evidence comes from the federal New Jobs Tax Credit (NJTC) of the late 1970s, which was enacted to spur recovery from the severe recession earlier in that decade. Evidence based on the NJTC is more positive, suggesting that a hiring credit that is non-categorical and creates explicit incentives for job creation can help create jobs.¹ This evidence is very limited, however—both because it is dated and because of the usual difficulties of identifying the effect of policy at the national level, stemming from the problem of constructing a counterfactual for what would have happened absent the NJTC.

As this article documents—for the first time, to the best of our knowledge—many state hiring credits exist. Several of these were adopted prior to the Great Recession, and more were enacted during and after the Great Recession. Yet virtually no empirical work examines these state credits.² Drawing on an extensive database on state hiring credits that we have constructed, we ask whether state hiring credits of the type adopted during and after the Great Recession, as well as earlier, boost employment. We focus on a subset of types of state hiring credits for which we expect effects on job growth, and for which there is likely to be sufficient variation in the data to reliably estimate policy effects.

A long-standing concern suggests that hiring credits can be inefficient at creating jobs, and can instead reward hiring that does not create net job

¹Katz (1998: 31) concluded that evidence from studies of the NJTC showed that a “temporary, non-categorical, incremental subsidy has some potential for stimulating employment growth.” More recently, researchers have taken a stronger position on the NJTC’s effectiveness (Bishop 2008; Bartik and Bishop 2009).

²A few exceptions exist. Bartik and Erickcek (2010) evaluated the MEGA Tax Credit Program in Michigan, which is quite different from other hiring credits. In addition, some researchers have evaluated small-scale, more-targeted hiring credit (or “voucher”) experiments (see Burtless 1985, and the discussion in Hollenbeck and Willke 1991). Finally, Chirinko and Wilson (2016) estimated the effects of state hiring credits. Their article focused on some subtler issues of the timing of effects based on the effective date and the signing date of the credit (“fiscal foresight”). Our article differs in numerous ways, including studying a much more comprehensive set of state hiring credit programs. Chirinko and Wilson restricted attention to 21 state hiring credits classified as permanent (no expiration date specified) in the 1990 to 2007 period; by way of contrast, see Table 1 (discussed below).

growth as firms churn employees to exploit hiring credits (meaning that firms may fire or lay off some workers while also hiring others because it allows the firm to benefit from hiring credits without actually increasing the number of employees). By looking at the effects of hiring credits on hiring as well as net job growth, we can assess the importance of these inefficiencies.³

What we can learn about the effects of hiring credits from the available data and policy variation has limitations, including potential endogeneity of hiring credits, difficulties in measuring some features of hiring credits, and relatively few instances of variation in some kinds of credits. Although we take these limitations seriously, we argue that given the lack of evidence on the effects of hiring credits, and such strong interest in whether government policy can spur job creation, it is important to learn what we can from the existing data and policy variation.

To summarize the findings, we conclude that some specific types of hiring credits have succeeded in boosting employment. The features associated with effective credits are refundability, allowing for recapture of payments if the required goals were not met, and targeting the unemployed. These results are consistent with our expectations based on past research on the effectiveness of hiring credits and the problems hiring credits have to overcome to spur job growth (see Neumark 2013). A refundable hiring credit ought to have a greater impact on firms because it is valuable even if the firm does not have taxable income in the current period. Recapture provisions should make hiring credits more effective. And credits targeting the unemployed, especially during a period such as the Great Recession when unemployment should not be a stigmatizing characteristic, should be more effective.⁴ Indeed, new evidence on federal hiring credits adopted in response to the Great Recession also suggests that, for example, credits targeting the unemployed were effective in this period (Farooq and Kugler 2015). All in all, though, the results provide some evidence that judiciously chosen hiring credits can help increase job growth, especially during severe downturns. At the same time, we find some evidence justifying concerns in the previous literature that hiring credits generate more gross hiring than they generate net employment growth.

Empirical Approach

We use a differences-in-differences empirical strategy to contrast the job growth experiences of states that did and did not implement particular types of hiring credits, while controlling for other factors to isolate the

³Another potential inefficiency, which we do not address, is windfalls in the form of credits paid to firms that would have created new jobs absent the credit.

⁴Kroft, Lange, and Notowidigdo (2013) provided evidence consistent with this idea. In particular, they found that employers pay attention to labor market conditions in interpreting unemployment as a negative signal. Though employers are less likely to call back those unemployed for a longer spell, the stigmatizing effect of a long unemployment spell is weaker in a slacker labor market.

effects of state hiring credits, including a counterfactual business cycle measure (*PSE*, for “predicted state employment”) as well as other controls (*X*) discussed below. We include up to 12 lags of the hiring credit variables in our analysis of monthly data, covering one year subsequent to the adoption of the credits.

More specifically, we denote the level of state employment as E_{jt} , and denote by HC_{jt} a dummy variable for a hiring credit in state j and period t ; as explained later, we estimate the effects of different types of hiring credits in most of our analyses, in which case HC_{jt} becomes a vector. Let T_t denote period dummy variables (for each unique month in the sample), S_s denote state dummy variables, and M_r denote calendar month dummy variables.

The baseline model that underlies our estimates of the effects of hiring credits on employment is:

$$(1) \quad \ln(E_{jt}) = \alpha_0 + \sum_{k=0}^{12} \beta_k HC_{j,t-k} + \sum_{k=0}^{12} \gamma_k \ln(PSE_{j,t-k}) + T_t \tau \\ + \sum_s \sum_r \{ \mu_{sr} S_s \times M_r \} + \sum_s \{ \pi_s \ln(PSE_{jt}) \times S_s \} + \sum_s \{ \alpha_{1st} t \} + X_{jt} \theta + \varepsilon_{jt}$$

In this model, $HC_{j,t}$ is a dummy variable equal to 1 in the month a hiring credit turns on and every subsequent month, $HC_{j,t-1}$ is equal to 1 in the month after a hiring credit turns on and every subsequent month, and so on. In other words, the effects of hiring credits arise contemporaneously and with a lag, and the effect persists, not changing after 12 months (unless the credit ends).⁵

The specification includes the counterfactual business cycle measure based on predicted state employment (*PSE*), in logs, also with lags up to 12 months. The time dummy variables (*T*), for each month in the sample, control for aggregate factors not captured in the control variables. The interactions between the state dummy variables (*S*) and the calendar month dummy variables (*M*) allow for different monthly patterns of employment changes by state.⁶ The interactions between the counterfactual cyclical measure and the state dummy variables allow the effects of this cyclical variable to differ by state.⁷ State differences in the effects of the business cycle measure could arise, for example, because the same magnitude of the shock to two different states could reflect employment changes in different industries, with different cyclical sensitivity. Or states may differ in their exposure to domestic and international markets, even if their industry composition is similar.

⁵Consistent with the effects of hiring credits arising with a lag, data on California’s New Jobs Credit suggest that the number of jobs for which the credit was claimed was very low (200 to 300 jobs per month) in the first couple of months after it took effect but then rose to a higher level—about 1,500 jobs per month. (See https://www.ftb.ca.gov/businesses/New_Jobs_Credit.shtml, accessed December 21, 2012. We estimated jobs for which the credit was claimed by dividing total credits paid by the maximum \$3,000 credit per worker.)

⁶Note that this sum is taken over all states but one, since the full set of month dummies will subsume the monthly pattern for one state, and over all months, so that these subsume the standard fixed state effects. These fixed effects will also appear in the first-differenced version we estimate (discussed below), capturing the state-specific linear trends in Equation (1).

⁷Again, the sum is taken over all states but one, since the main effects capture the effect for one state.

We estimate the model in first-differenced form, for the change in log employment (dropping the first observation for each state), to ease interpretation and to scale states similarly, using the specification:⁸

$$(2) \quad \{[\Delta \ln(E_{jt})] \times 100\} = \alpha + \sum_{k=0}^{12} \beta_k \Delta HC_{j,t-k} + \sum_{k=0}^{12} \gamma_k \Delta \ln(PSE_{j,t-k}) + T_t \tau \\ + \sum_s \sum_r \{\mu_{sr} S_s \times M_r\} + \sum_s \{\pi_s \Delta \ln(PSE_{jt}) \times S_s\} + \Delta X_{jt} \theta + \Delta \varepsilon_{jt}.$$

We multiply the change in log employment by 100, so the estimated effects of hiring credits can be interpreted as percentage point changes in employment. Robust inference requires clustering the data at the state level to allow for arbitrary patterns of serial correlation within states, and heteroscedasticity across states. With 50 states, the asymptotic approximations should provide reliable inference (Cameron, Gelbach, and Miller 2008).

The key parameters in Equation (2) are the β_k 's, which capture the contemporaneous and lagged effects of changes in hiring credits on employment. If hiring credits boost employment, we would expect the values of the β_k 's to be positive, at least for some period. By contrast, we could find the β_k 's equal to 0 even if many employers claim hiring credits, when they are claiming credits for hiring that would have occurred absent the credit, or otherwise manipulating their workforces in ways that make them eligible for credits without creating jobs.

The counterfactual business cycle measure is intended to capture cyclical influences on employment growth in each state that could be related to the adoption of state hiring credits. We construct this counterfactual business cycle measure by applying national time-series changes in disaggregated industry employment to the state, based on the state's industry composition in a baseline period of stable aggregate economic growth (as in Bartik 1991). To provide a simple example, if a state, at baseline, had 50% of employment in the auto industry and 50% in the restaurant industry, then the counterfactual for employment change over a given period would be an equally weighted average of the employment change nationally in these two industries.

Data on total and industry employment come from the Quarterly Census of Employment and Wages (QCEW).⁹ The QCEW provides monthly employment at the state level and by the North American Industry Classification System (NAICS) industry level. To construct the counterfactual cyclical measure (*PSE*), we used industry employment at the NAICS 4-digit level.¹⁰ In the disaggregated state-by-industry QCEW data, the

⁸Note that this model is equivalent to one in which we first difference all of the variables in Equation (1), including the time dummy variables, the state-month interactions, and so on. That is, the dummy variables and interactions can remain in levels rather than first differenced, and the model fit is the same.

⁹Data can be downloaded at <http://www.bls.gov/cew/data.htm>.

¹⁰The Bureau of Labor Statistics introduced NAICS 2012, which applies to the QCEW, in 2011. This new version changes industry classification at lower levels of disaggregation only, though, and so does not affect our classification.

information is suppressed in some months for confidentiality reasons. In these cases, we scale up the non-missing entries proportionally to match total employment for the month. To avoid noise in our baseline industry composition, we compute the baseline industry employment by averaging over all 12 months in 1990 (one year before our first sample year), and then divide by the average of total employment across months. We still have to assign the baseline industry composition to one particular month to construct our counterfactual business cycle measure for each subsequent month, but the annual averages do not match any specific month. We therefore rescale industry employment so that multiplication by this average share matches June 1990 employment, and then construct the cyclical measure relative to that month. (When we look at the 2007 to 2011 sample to study the Great Recession period separately, we use 2006 data to construct our baseline in a parallel fashion.)

More generally, let subscripts j index states, k industries, and b the baseline period. Denote by SE_{jkb} total employment in state j , industry k , and period b , denote by AE_{kt} aggregate (national) employment in each period t in industry k , and denote by AE_{kb} aggregate employment in industry k in the baseline period b . Then state employment based solely on aggregate developments is predicted in each period subsequent to b by applying the national changes to the baseline composition, as in

$$(3) \quad PSE_{jt} = \sum_k SE_{jkb} \times \left(1 + \frac{AE_{kt} - AE_{kb}}{AE_{kb}} \right)$$

This equation predicts state employment in each period by applying the national growth rate of employment in each industry between the baseline period and that period to the baseline employment level in the corresponding industry in the state, and then aggregating, weighting by the baseline industry distribution of employment in the state.

The counterfactual business cycle variable does help predict employment growth. We estimated simple regressions of the first difference of log employment on the contemporaneous and lagged values of the first differences in the log of PSE and their interactions with state dummy variables, as in Equation (2) above, as well as dummy variables for every unique month in the sample (to capture common aggregate changes as flexibly as possible). Adding the full set of counterfactual business cycle measures to the model with the full set of month dummy variables increased the R^2 by between 7% and 9% across the three sample periods we consider—from approximately 0.65 to 0.70. The estimated effect of the business cycle measure is positive and strongly significant.

Numerous other controls are contained in X . First, to capture other state policies that could have varied contemporaneously with state hiring credits and affected hiring, we include measures of prevailing state minimum wages (the higher of federal or state minimum wages), and of extended unemployment insurance (UI) benefits, which can be important during

recessionary periods.¹¹ Since we use a first-difference model, these controls enter in first-difference form, with contemporaneous values and 12 lags of the first difference.

Second, although the minimum wage and UI strike us as among the most important policies for job growth, we recognize that other policies may be present that we fail to measure explicitly. As a result, we also include measures of the political control of states in each year, presuming that other policies adopted may vary systematically with the political party in control. Specifically, we use a dummy indicator for each year for whether the state has a Democratic governor.¹² This variable is entered in levels, on the presumption that political variables may affect the growth rate of employment, so that, effectively, the model in levels includes these variables multiplied by a time trend, resulting in the level in the first-differenced model.¹³

Third, when we focus on the Great Recession period, we account for the extent of the decline and the course of the recovery in each state having been strongly influenced by housing market developments. Housing market developments in each state are a potential confounding factor for a few reasons: they could have directly or indirectly affected hiring via the construction industry and spillovers from that industry; they could have spurred other responses during this period that are not captured in the controls discussed thus far, which could also affect hiring; and they could have provided advance warning to policymakers of a coming crash, leading them to adopt hiring credits in anticipation of labor market changes, which would imply a causal effect in the opposite direction of the one we are trying to estimate. To address these possibilities, we include state-specific measures of the price run-up in the 2000 to 2006 period, which are correlated with, and would have helped predict the extent of, the decline by state.¹⁴ (We do not want to include measures of price declines during the Great Recession, since they could have been influenced by the same policies.) Specifically, we construct dummy variables for the quantiles of the distribution of

¹¹For UI, we use Farber and Valletta's (2013) measure of the number of weeks of extended Unemployment Insurance benefits, both those added automatically from the Extended Benefits program and those from the Emergency Unemployment Compensation program. Both Farber and Valletta (2013) and Rothstein (2011) showed that these recent expansions in the length of unemployment insurance led to increased unemployment durations, particularly for the long-term unemployed. These extended benefits could therefore have slowed job growth. The control variable we use is the number of weeks beyond the normal 26 weeks of Unemployment Insurance that are available in that state and month.

¹²These original data are from Klarner (2013), with updates provided by Klarner via personal communication. We verified that the results were insensitive to a richer coding that also captured control of the institutions in the state legislature, although this precludes including Nebraska, which has a nonpartisan unicameral legislature. (Results are available upon request.) To include all states, we show results for the specification based on the governorship.

¹³We verified that including this variable in first differences had no impact on the results. (Results available upon request.)

¹⁴We use data from the Federal Housing Finance Agency (FHFA); see <http://www.fhfa.gov/DataTools/Downloads/pages/house-price-index.aspx> (accessed April 24, 2015).

housing price appreciation across states, and interact these with the month dummy variables, to allow for different patterns of change by state associated with this pre-recession price appreciation.

The broader point is that the counterfactual business cycle measure is not expected to be sufficient to fully predict the course of the state's economy absent state hiring credits. Bartik (1991) discussed whether non-traded (as opposed to traded) industries follow national trends, and showed that these actually do track quite closely. We would still expect that national industry trends applied to the state's industry composition sometimes miss potentially important variation. Housing market developments could be an important source of such variation, as state trends during the Great Recession period (and prior to it) diverged widely from national trends (see Mian and Sufi 2010), and hence we control for them explicitly. For similar reasons, when we estimate our models for the 2007 to 2011 period, we also account for the major federal effort to boost job growth in this period—the American Recovery and Reinvestment Act (ARRA).¹⁵

Fourth, as one additional way of adding much richer controls to the model, we estimate specifications including a full set of Census division \times month interactions. Note that the model written above includes interactions between state dummy variables and *calendar* months. But we cannot include a full set of state \times month interactions, since that is the level at which the state hiring credits vary. Nonetheless, interactions between Census divisions and the full set of months restrict all identifying variation to come from within divisions, and hence control for shocks or any other source of variation over time that is common to Census divisions.

Our analysis focuses on state hiring credits. As noted earlier, the federal HIRE Act, establishing a modest credit, was enacted in 2010. We are limited, however, to estimating the effects of state hiring credits. In many studies of similar policies that vary at the state and federal level, if a policy exists in some states, then adopting it at the federal level provides identifying information about the effect of the policy. This approach requires that the federal and state policies be substantively the same. In the context of state hiring credits and the HIRE Act, this is decidedly not the case, because state hiring credits provide credits against *state* taxes, whereas the HIRE Act provided credits against federal taxes.¹⁶

¹⁵See Chodorow-Reich, Feiveson, Liscow, and Woolston (2012) and Wilson (2012) for evidence on the effects of the ARRA on state-level employment.

¹⁶Another federal credit that changed recently is the Work Opportunities Tax Credit (WOTC). The WOTC targets veterans, short- and long-term TANF recipients, SNAP (Food Stamp) recipients, and others. It replaced the Targeted Jobs Tax Credit in 1996. A 2011 act (the VOW to Hire Heroes Act) extended benefits for veteran target groups, and established new categories for veterans who have been unemployed and veterans with service-connected disabilities (http://www.doleta.gov/business/incen tives/opptax/eta_default.cfm, accessed March 10, 2013). The act was adopted in late 2011 and did not take effect until 2012. Scott (2013) reported that in 2012 veterans were fewer than 4% of total WOTC certifications.

Although we estimate a rich model and try to capture many important dimensions of hiring credits and factors that could be confounded with them, limitations remain as to what we can do. First, it is possible that endogenous adoption of hiring credits biases the estimates from Equation (2). We do not believe that a good instrument is available for examining hiring credits. To the extent that they are adopted in response to shocks that hit a state's economy, these shocks cannot be—and are not—excluded from the model. And we are not aware of any political reason distinct from economic outcomes that would explain why some states adopt these credits and others do not, let alone why their adoption might vary over time (which it would have to do, given that the model includes fixed state effects). We are able to do a few things to address this issue, however. We examine whether lagged changes in state employment growth predict the adoption of credits. We find no such evidence, which makes it less likely that endogeneity is a concern, although this kind of analysis cannot rule out endogenous adoption of policy based on predicted future employment growth, as opposed to lagged employment growth. We also believe that including the housing price run-up controls in the analysis of the Great Recession period is a means of capturing a potentially endogenous response, because the housing price run-up could have been the basis for the predicted decline when the housing bubble burst. Finally, our models effectively include state-specific linear trends (because the model in first differences has a full set of state fixed effects for the growth of employment), which can help control for prior trends associated with the adoption of credits.

The State Hiring Credits Database

The key input into the empirical analysis is a detailed database on state hiring tax credits that we have constructed. The online Appendix (available at <http://journals.sagepub.com/doi/suppl/10.1177/0019793916683930>) describes the construction of this database; here we note key features of the database and the state hiring credits we capture.

States offer complex packages of incentives, ranging from tax incentives based on different criteria (e.g., job creation), to financial assistance, technical support, training, incentives for creation of infrastructure, and so on. Hiring credits are only a part of this set of incentives, and thus we had to define criteria for inclusion of a program in the hiring credit database—the most important of which is the intent to create (or retain) jobs. We used the following criteria for the inclusion of a program in the state hiring credit database:

The program's law or regulations require firms to create or retain jobs or to increase payroll. Programs aimed at attracting new companies to the state (e.g., headquarters programs) are also included since by definition they create new jobs and, in most cases, they include an explicit job creation requirement.

The program is broad in the sense that it covers a large portion of the state's firms or employees.¹⁷

The program is targeted directly at the employer that is creating jobs. For instance, we do not include programs that foster infrastructure improvement by local governments on behalf of a business that is creating jobs.

The program is not geographically targeted. In particular, we do not include enterprise zone programs or local hiring programs.¹⁸

The program's costs are not borne by local governments. In particular, we do not include property tax abatements and tax-increment financing districts.

In addition, we do not include programs based on training, apprenticeships, or internships; on research and development; or those related to the film industry. We also do not include agricultural or financial programs (e.g., programs that provide loans or whose benefits are reductions in the interest rate on previous loans). By contrast, we do include programs that have broad targeting by industry (e.g., manufacturing), company type (e.g., small businesses), or groups of workers (e.g., the unemployed).

The hiring credits database provides information on job creation programs in all 50 states for the period 1969 to 2012, for which we identified 147 hiring credits. We also capture the timing of the enactment and expiration of hiring credits. In June 2012, 128 of these programs were current and 19 had expired or been replaced.¹⁹ Forty-five states had at least one hiring credit at some point during the whole period. Five states did not have any program (Alaska, New Hampshire, South Dakota, Washington, and Wyoming), and five states had at most one program (Hawaii, Maine, Minnesota, Montana, and Oregon). The remaining 40 states had two or more hiring credits over the period, and of these, most had two to four credits. Virginia is the state with the largest number of programs (a maximum of 10 during the sample period).

Table 1 provides information on when hiring credits were adopted and their durations. Most hiring credits were created after 1989, and more than one-third were created in 2000 or later. Although the table shows that many programs are in effect for fewer than 10 years, this is driven partly by many credits being adopted in later years. Overall, state hiring credits have lasted for an average of 12.5 years.

State hiring credits differ along several dimensions, although almost all (143) require the creation of new jobs. Table 2 provides a detailed description of the credits and an explanation of their coding in the database.²⁰

¹⁷For instance, we do not include Arizona's Credit for Employing National Guard Members or Massachusetts's Jobs Incentive Payments for Certain Biotechnology Companies.

¹⁸One exception is the inclusion of Kansas's Enterprise Zone Job Creation Tax Credit, because the incentives apply statewide.

¹⁹Two programs became ineffective after June 2012, and three additional programs became ineffective after December 2012.

²⁰The discussion here, and Table 2, do not cover all types of hiring credits included in the database. Rather, we focus on those most likely to have the greatest policy interest regarding the effects of a credit (such as credits targeting manufacturing), or on those with the strongest a priori reason to expect an effect on employment. Details on other credits are provided in the working paper version of this paper (Neumark and Grijalva 2015).

Table 1. Summary of State Hiring Credits, 1969 to June 2012

A. States	
States analyzed	50
1 or more hiring credits	45
No hiring credit	5
B. Basic information	
Total number of hiring credit programs	147
Creation date	
1969–1979	6
1980–1989	16
1990–1999	58
2000 to before Great Recession	37
During Great Recession	9
After Great Recession	21
Current as of June 2012	121
Duration of hiring credit programs	
0–10 years	73
11–20 years	53
21–30 years	13
31+ years	8

The first column of Table 3 summarizes the distribution of hiring credits along these dimensions.²¹

As Table 2 shows, hiring credit programs differ in a number of ways. Hiring credits vary in terms of tax savings. Credits may limit the benefits to equal the tax liability, or they may allow them to be higher than the tax liability. In the latter case, firms may either carry forward benefits above the current year's tax liability to future years, or they may receive the full amount of the benefits in the current year, if the credit is refundable. Almost one-third of programs do not specify this limit and almost half provide a carry-forward provision (Table 3).

Another potentially important dimension is the type of new jobs required. The employment required can be full-time, full-time equivalent (FTE), or part-time. In a few cases, the program does not specify the type of employment required. Full-time is the most common requirement.

State hiring credits also differ in targeting based either on employee's characteristics (unemployed, disabled, and welfare recipients) or employer's characteristics (such as industry). Hiring credits present some additional characteristics that may affect their impact on job creation. First, many programs try to ensure that credits are paid for new job creation by "recapturing" or "clawing back" some of the credits if net job creation is lower than required for payment of the credit.²² One might wonder

²¹Table A.1 in the online Appendix (available at <http://journals.sagepub.com/doi/suppl/10.1177/0019793916683930>) presents a list of all programs in our database, with their particular features.

²²For example, the Iowa New Jobs and Income Act states that if the Department of Revenue "determines that [the] business has failed in any year to meet any one of the requirements . . . the business or group of businesses is subject to repayment of all or a portion of the amount of incentives received." Similarly, the Arkansas Economic Development Act calls for repayment of all benefits received by a business, plus penalty and interest, if it does not create the required 100 new jobs within 24 months. Both programs allow for extensions for businesses to meet job creation goals.

Table 2. Definition and Coding of State Hiring Credits

Variable	Categories (Description)		
Tax treatment	Equal to tax owed	The maximum benefit that can be paid to a firm is the firm's tax liability.	
	<i>Form in which the program limits the economic benefits provided for each taxable year.</i>	Carry-forward	If the value of the benefit exceeds the firm's tax liability (or a specific percentage of it) for the taxable year, this excess may be carried forward to succeeding years and be used as a credit against the firm's future tax liability.
		Refundable	The whole benefit is paid even if it is higher than the value of the firm's tax liability.
Type of new jobs required	Not specified		
	Full-time	New employee works for 30 or more hours per week.	
	<i>Type of job the firm needs to create to obtain the benefits of the program. The type of job is defined by the minimum number of hours of work performed per week.</i>	Full-time equivalent	One or more new employees work a number of hours per week that add up to one full-time employee's hours requirement.
		Part-time	New employee works at least 10 hours per week.
Targeting by industry	Not specified		
	Targeted	Program applies to a cluster of industries.	
	Manufacturing	Program applies to manufacturing facilities.	
Targeting by type of worker^a	Not targeted	Program applies to all industries.	
	Unemployed	Program applies to the unemployed, i.e., individuals who attest to be not working and who have received unemployment compensation benefits and/or have been classified as unemployed by a competent office of employment.	
	Welfare recipient	Program applies to recipients of welfare aid, e.g., Temporary Assistance for Needy Families.	
	Disabled	Program applies to disabled workers, i.e., individuals who are considered to have a physical or mental disability which results in a substantial handicap to employment. This disability may be determined or certified by specific institutions such as the Division of Rehabilitation Services.	
	Not targeted	Program applies to all workers.	
	Recapture provisions	Yes	
<i>Program has specific provisions (e.g., penalties) if the requirements to obtain the credit were not met and/or maintained.</i>	No/not specified		
Temporary/permanent	Temporary		
	Permanent		
	<i>Program is originally enacted as temporary, permanent, or undetermined/not determinable. The classification is assumed to be a feature of each program throughout its duration.</i>	Undetermined/not determinable	

^aWe examine these types of credits only during the Great Recession period, when there happened to be no variation in credits targeting welfare recipients, so credits targeting this specific group do not appear in the analysis.

Table 3. Total Number of State Hiring Credits, 1969 to June 2012, and Total Number of Changes in States with Specific Types of Credits, 1990–2011

Variable	1969–2012	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Any credit	147	0	2	0	3	4	8	3	2	2	0	0	0	2	0	0	0	0	0	0	2	0	0
Tax treatment^a																							
Equal to tax owed	21	0	0	0	2	0	2	0	1	2	0	0	0	0	1	0	0	1	0	1	1	0	1
Carry-forward	67	0	0	0	1	1	6	2	1	3	2	0	0	2	0	0	0	1	0	0	3	0	0
Refundable	17	0	0	0	1	1	3	0	0	0	0	0	0	0	0	1	2	0	1	0	0	2	2
Not specified	43	0	2	2	0	4	1	3	2	0	0	1	0	1	1	0	2	1	0	1	3	2	2
Type of new jobs required																							
Full-time	90	0	0	2	1	2	7	3	3	2	1	0	0	0	1	0	1	0	0	0	3	1	0
Full-time equivalent	24	0	1	0	2	1	0	2	1	1	0	0	0	1	0	0	1	0	2	1	1	2	1
Part-time	11	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	2	0	0	0	0	0
Not specified	22	0	1	0	0	2	1	0	2	1	1	0	0	1	0	0	0	1	0	0	1	0	0
Targeting by industry^a																							
Targeted	33	0	1	1	0	1	5	1	2	0	1	0	0	2	0	0	0	0	0	1	0	1	0
Manufacturing	4	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
Not targeted	114	0	1	0	3	3	5	2	2	3	1	0	1	0	2	0	1	0	0	0	2	1	0
Targeting by type of worker^a																							
Unemployed	7	0	0	1	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	3	1
Welfare recipient	8	0	0	1	0	1	2	0	1	2	1	0	0	0	0	0	0	0	0	0	0	0	0
Disabled	10	0	0	0	0	0	1	0	2	0	1	0	0	0	1	0	0	2	0	0	0	1	0
Not targeted	124	0	2	1	3	4	8	3	2	2	0	0	0	2	0	0	0	0	0	0	2	0	0
Recapture provisions																							
Recapture	54	0	1	1	3	3	6	4	2	2	0	0	0	1	0	0	2	0	1	0	1	2	0
No recapture	93	0	1	0	0	2	4	1	3	1	2	0	0	1	1	0	0	2	0	0	3	1	1
Temporary/permanent																							
Temporary	44	0	0	0	1	1	5	1	1	1	0	1	1	2	1	1	3	3	0	2	0	6	3
Permanent	97	0	2	1	3	3	7	4	2	2	0	0	0	1	2	0	1	1	0	0	2	0	2
Undeterminable	6	0	0	0	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

^aThe classification for this variable is not mutually exclusive.

whether these provisions of tax credits have any teeth. In fact, state reports show that recapture provisions are actually used, and that states do recover credits (or other financial incentives) when job creation or other goals are not met.²³ Moreover, even if this recovery does not often occur, the threat of recovery may enhance the effects of hiring credits that include recapture provisions.

Finally, credits can differ based on whether they are temporary or permanent. Though this distinction is clear at the theoretical level—and we would predict a stronger effect of a temporary credit in shifting hiring to the period when the credit applies—this difference is not so clear in practice. For example, an apparently permanent credit can be repealed and a temporary one extended.

Our identifying information comes from changes in state hiring credits during our sample period. Our analyses focus on whether a state has a particular type of credit, rather than counts of potentially similar credits. Thus, we need to know how many states experienced a change in having a particular type of credit. This information is reported in the remaining columns of Table 3, for the classifications of hiring credits we consider. Some credits of interest—in particular, full-time versus part-time, and credits targeting manufacturing—exhibit too little variation to reliably estimate their effects. The same is true for credits targeting the unemployed or other groups over the full sample period, although the Great Recession period that we study separately has relatively more variation in this type of credit. For these reasons, plus the ambiguity in classifying temporary and permanent credits, in our empirical analysis we largely focus on the tax treatment dimension (most importantly, refundability), and whether credits have recapture provisions. We also estimate the effects of credits overall, and, for the Great Recession period, the effects of credits targeting the unemployed or other types of workers.

We present analyses for related sets of features of specific classes of hiring credits, such as credits that allow for recapture or clawbacks compared to those that do not. Thus, for example, for a two-way classification of hiring credits, two dummy variables HC^1_{jt} and HC^2_{jt} can be defined and substituted for the single HC_{jt} in Equation (2) above. This approach allows us to estimate the effects on job growth of each type of credit within a broad class of credits. We do not simultaneously estimate the effects of all dimensions of state hiring credits we consider.

²³See, for example, reports on clawbacks for North Carolina (http://www.nccommerce.com/Portals/0/Incentives/CLAWBACK-REPORT_Apr-2015.pdf), Indiana (<https://transparency.iedc.in.gov/Additio%20Public%20Information/Economic%20Incentives%20and%20Compliance%20Report%20Period%20ending%20December%2031,%202014.pdf>), Florida (http://www.floridajobs.org/business/DEO_EDP_PROD.htm, under “Quick Action Closing Fund, Inactive”), Mississippi (<https://merlin.state.ms.us/reports/FY2013%20Mississippi%20Incentives%20Report.pdf>, pp 20-11), and Maryland (<http://business.maryland.gov/Documents/ProgramReport/MarylandEconomicDevelopmentAssistanceAuthorityAndFundFY2011.pdf>) (all accessed April 22, 2015). A list of websites providing information on penalties for noncompliance is available in appendix 5 of Good Jobs First (2012).

Table 4. Estimated Effects of State Hiring Credits on Employment, Credit Dummy Variables Specifications, First Differences, 1995–2011

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+4 Lags</i>	<i>+8 Lags</i>	<i>+12 Lags</i>	<i>Joint significance</i>
Credit	−0.22 (0.17)	−0.05 (0.12)	−0.07 (0.19)	0.01 (0.30)	0.18
Equal to tax owed	−0.04 (0.20)	0.13 (0.32)	0.14 (0.39)	−0.11 (0.66)	0.00 ⁺⁺ , ^{††}
Carry-forward	−0.20 (0.16)	−0.05 (0.13)	−0.10 (0.19)	−0.07 (0.27)	0.39
Refundable	0.17 (0.15)	0.12 (0.18)	0.39* (0.21)	0.44* (0.25)	0.00 ⁺⁺ , ^{††}
Not specified	0.03 (0.13)	−0.03 (0.23)	−0.06 (0.30)	−0.10 (0.37)	0.17
Recapture	−0.01 (0.18)	0.22 (0.15)	0.34* (0.20)	0.42 (0.30)	0.08
No recapture	0.04 (0.10)	−0.05 (0.09)	−0.12 (0.15)	0.01 (0.19)	0.81

Notes: The dependent variable is the first difference of the log of QCEW employment, multiplied by 100. The specification includes the first difference of the job credit dummy or dummies, and 12 lags of these first differences. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported. (See Figure 1 for cumulative effects through each lag length.)

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible types of tax treatment, and so on. The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 12 lags of the first difference of the control for extended UI benefits; a dummy variable for the political party of the governor (measured annually); dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables.

Cyclical control is constructed using 1990 as the baseline year. Data are monthly. $N = 10,150$ observations. Standard errors, reported in parentheses, are clustered at the state level. The column labeled “Joint significance” reports the p value for the joint significance of the 13 coefficients on the hiring credits (i.e., of the null hypothesis that all coefficients equal zero).

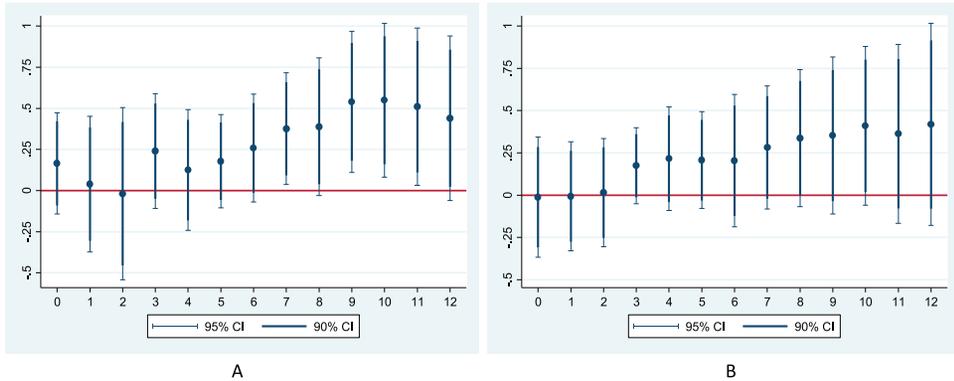
Superscripts *, #, and † indicate whether the p value for the null hypothesis is below 0.1 (one symbol) or 0.05 (two symbols), based, respectively, on the conventional hypothesis test (*), multiple testing using the Simes method (#), and multiple testing using the Sidak method (†). (For the joint significance column, the p value for the single test is reported, in lieu of asterisks. The multiple testing analysis is done separately for the joint significance tests.)

Results

Baseline Results

Our baseline results are from models estimated for the 1995 to 2011 period. We can extend the sample back a bit earlier, to 1991, and we do so later. But data on the UI benefit extensions are available beginning in 1995. The first panel of Table 4 reports estimates of the effect of a hiring credit of any kind, the second distinguishes between credits based on tax treatment, and the third focuses on recapture or clawback provisions. All specifications include a contemporaneous dummy variable (or dummy variables) for the

Figure 1. Estimated Effects of Selected State Hiring Credits on Employment, from Table 4 Specifications (1995–2011)



A. Refundable credits

B. Credits with recapture

Note: CI, confidence interval.

hiring credits included, plus 12 monthly lags of these. The table reports the contemporaneous coefficients, and then the cumulative effect including lags through 4, 8, and 12 months, as well as the p value for the joint significance of all 13 hiring credit variables. Finally, to give a more complete picture of the pattern of effects implied by the coefficients, Figure 1 displays the estimated cumulative effect through each successive month (along with confidence intervals) for key results. This figure (and similar ones that follow) shows that the results are not qualitatively different when looking at or summing over somewhat different lag lengths.²⁴

As reported in the first panel of Table 4, no evidence of an effect of hiring credits exists when no distinctions are made among the features of hiring credits. To interpret the magnitudes, since the dependent variable is the log difference in employment multiplied by 100, the estimated coefficient of 0.01 on the cumulative (through the 12-month lag) effect implies that enactment of a hiring credit without regard to its specific features increases employment by a very small (and in this case insignificant) 0.01%.

The next panel distinguishes credits based on their tax treatment. We would expect refundable credits to be the most valuable, since these reward firms for hiring even if they do not have taxable income in the current year.²⁵ The evidence is consistent with positive effects of refundable credits, whereas for all the other tax-treatment classifications of hiring credits, the

²⁴The p value for the joint significance test is sometimes low for types of credits for which we see opposite-signed effects at different lag lengths (e.g., the “Equal to tax owed” row in Table 4). However, this does not mask evidence of significant effects through different lag lengths. In our main analyses (reported in Tables 5 and 6), there are no cases of insignificant effects for the lag lengths reported in the table and significant effects through different lag lengths.

²⁵Credits with carry-forward provisions should also be more valuable than credits that are limited by the current tax owed, if taxable income is expected in the future.

estimated effects are small and statistically insignificant (and alternate signs). The estimated effects of refundable credits grow and become statistically significant at the 10% level over the longer run, based on conventional hypothesis tests, denoted by asterisks in Table 4. At the 12-month lag, the point estimate of 0.44 implies that employment is 0.44% higher—a sizable effect.

The final panel categorizes hiring credits based on the presence of a recapture mechanism, which we would expect to lead to more effective credits, either by enforcing job creation goals or by encouraging only firms that could actually meet them to apply for credits. The evidence is consistent with this prediction, as the estimates for hiring credits with recapture provisions are positive, and the estimate through the 8-month lag is significant at the 10% level.²⁶

Thus, the evidence from Table 4 suggests that refundable hiring credits, and credits that allow for recapture of payments if the required goals were not met, may have succeeded in boosting employment. The evidence for refundable credits is statistically stronger, as shown in Table 4 and, with greater detail, in Figure 1.²⁷

Given that we are testing for significant effects among any number of potential policies—or doing “multiple testing”—we could be overstating

²⁶One question is whether our estimates are too imprecise because we freely estimate contemporaneous and 12 lags of the effects of hiring credits. Given that our effects tend to grow over time, but not always monotonically, we estimated more restrictive versions of the models that capture the effects of each hiring credit in just two coefficients, with terms that are linear and quadratic in months for 12 months (the lag length in Equation (2)). In levels, the hiring credit variable becomes:

$$\beta_L \cdot \min\{(k + 1), 13\}HC_{j,t} + \beta_Q \cdot [\min\{(k + 1), 13\}]^2 HC_{j,t}.$$

Thus, in the month the hiring credit turns on, the linear variable in the model is β_L , in the next month it is $2\beta_L$, and so on, reaching a maximum of $13\beta_L$ at the equivalent of the full lag length in Equation (2), while the corresponding quadratic terms are β_Q , $4\beta_Q$, and so on. The effect of HC changes based on these coefficients (for example, increasing at first if β_L is positive, but at a decreasing rate and perhaps diminishing if β_Q is negative), reflecting a change in the effectiveness of the credit through the months covered, at which point the effect stops changing. As before, we estimate the model in first differences, so the effects arise and change over the lag length of the credit variables, and then eventually revert to zero.

The resulting estimates are reported in Appendix Table A.1. Focusing attention on the credits for which we found some consistent evidence of positive effects—refundable credits and credits with recapture provisions—no notable difference in precision from these more restrictive specifications was observed, and the estimated magnitudes are of the same order of magnitude. In our view, no a priori basis occurs for imposing the restrictions in this table, and given that doing so does not sharply increase the precision of the estimates, we prefer the unrestricted estimates.

²⁷We also estimated the specifications in Table 4 adding a full set of interactions between Census division dummy variables and all calendar months, so that the variation comes fully from within Census divisions, allowing for unmeasured policy differences (or other effects) that vary regionally. Estimates are very robust. We also estimated the specifications in Table 4 defining the hiring credit variables as counts of the number of credits with a particular feature, rather than dummy variables for the presence of a credit with a particular feature. As noted earlier, the variation in the number of credits may not be meaningful if the addition of credits of a type that already exists in a state simply indicates a proliferation of small programs that do little to change incentives to hire. In this case we no longer find any evidence of positive effects on employment growth from refundable credits or credits allowing recapture. (Results are available from the authors upon request.)

Table 5. Estimated Effects of State Hiring Credits on Employment, Credit Dummy Variables Specifications, First Differences, 1991–2011

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+4 Lags</i>	<i>+8 Lags</i>	<i>+12 Lags</i>	<i>Joint significance</i>
Credit	−0.05 (0.10)	−0.03 (0.09)	−0.08 (0.12)	0.06 (0.19)	0.80
Equal to tax owed	0.05 (0.17)	0.03 (0.26)	−0.13 (0.34)	−0.24 (0.50)	0.00 ⁺⁺ , ^{††}
Carry-forward	−0.13 (0.12)	0.06 (0.12)	0.02 (0.18)	0.01 (0.24)	0.42
Refundable	0.14 (0.12)	0.13 (0.14)	0.34* (0.20)	0.41* (0.22)	0.00 ⁺⁺ , ^{††}
Not specified	0.05 (0.10)	−0.08 (0.17)	0.04 (0.20)	0.07 (0.25)	0.20
Recapture	0.07 (0.12)	0.10 (0.12)	0.19 (0.14)	0.28 (0.21)	0.77
No recapture	0.04 (0.09)	0.00 (0.10)	−0.01 (0.13)	0.13 (0.17)	0.52

Notes: The dependent variable is the first difference of the log of QCEW employment, multiplied by 100. The specification includes the first difference of the job credit dummy or dummies, and 12 lags of this first difference. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported. (See Figure 2 for cumulative effects through each lag length.)

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible bases for benefits, and so on. The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; a dummy variable for the political party of the governor (measured annually); dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables. The UI benefits control is not included because the data do not extend back to the beginning of the sample period used in this table.

Cyclical control is constructed using 1990 as the baseline year. Data are monthly. $N = 12,550$ observations. Standard errors, reported in parentheses, are clustered at the state level. The column labeled “Joint significance” reports the p value for the joint significance of the 13 coefficients on the hiring credits (i.e., of the null hypothesis that all coefficients equal zero).

Superscripts *, #, and † indicate whether the p value for the null test of no effect is below 0.1 (one symbol) or 0.05 (two symbols), based, respectively, on the conventional hypothesis test (*), multiple testing using the Simes method (#), and multiple testing using the Sidak method (†). (For the joint significance column, the p value for the single test is reported, in lieu of asterisks. Multiple testing analysis is done separately for the joint significance tests.)

the statistical significance of the results (e.g., Anderson 2008). The statistical treatment of this problem entails adjusting the critical values to account for testing multiple hypotheses simultaneously, and hence conventional critical values based on single hypothesis tests are understated, leading to too-frequent rejection of the null hypothesis.

Table 4 also provides information on adjusted statistical tests, as do Tables 5 and 6. Whereas asterisks indicate whether the estimated effects reported are significant at the 5% or 10% level based on conventional tests, the symbols # and † are used to indicate whether the null hypothesis of no

Table 6. Estimated Effects of State Hiring Credits on Employment, Credit Dummy Variables Specifications, First Differences, with ARRA Spending and Housing Appreciation Controls, 2007–2011

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+4 Lags</i>	<i>+8 Lags</i>	<i>+12 Lags</i>	<i>Joint significance</i>
<i>ARRA variable(s)</i>	<i>Contemporaneous</i>	<i>+6 Lags</i>	<i>+12 Lags</i>	<i>+24 Lags</i>	
ARRA	-0.05 (0.08)	-0.17 (0.14)	0.02 (0.23)	0.17 (0.37)	0.00
Credit	0.11 (0.15)	-0.14 (0.23)	-0.67* (0.40)	-0.84* (0.49)	0.00 ^{+,††}
Equal to tax owed	-0.49 ^{*,+,††} (0.12)	-0.72 (0.56)	-1.15 (0.70)	-2.47* (1.37)	0.00 ^{+,††}
Carry-forward	0.12 (0.10)	-0.09 (0.46)	-0.49 (0.56)	-0.51 (0.86)	0.00 ^{+,††}
Refundable	0.12 (0.30)	0.27 (0.37)	0.55 (0.42)	0.17 (0.61)	0.00 ^{+,††}
Not specified	0.28* (0.14)	0.03 (0.29)	-0.14 (0.40)	-0.18 (0.48)	0.00 ^{+,††}
Recapture	0.33 (0.23)	0.57 ^{*,+,††} (0.18)	0.64 ^{**} (0.31)	0.82* (0.42)	0.00 ^{+,††}
No recapture	0.27* (0.14)	-0.15 (0.20)	-0.40 (0.28)	-0.49 (0.34)	0.00 ^{+,††}
Unemployed	0.61 ^{*,+,††} (0.14)	0.84 ^{*,+,††} (0.21)	0.82 ^{*,+,††} (0.33)	1.16 ^{*,+,††} (0.45)	0.00 ^{+,††}
Disabled	-0.94 ^{*,+,††} (0.19)	0.15 (0.33)	0.34 (0.24)	0.36 (0.74)	0.00 ^{+,††}
No targeting	0.12 (0.16)	-0.14 (0.23)	-0.69* (0.41)	-0.84 (0.51)	0.00 ^{+,††}

Notes: The dependent variable is the first difference of the log of QCEW employment, multiplied by 100. The specification includes the first difference of the job credit dummy or dummies, and 12 lags of this first difference. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported. (See Figure 3 for cumulative effects through each lag length.)

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible bases for benefits, and so on. The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 12 lags of the first difference of the control for extended UI benefits; a dummy variable for the political party of the governor (measured annually); dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables.

We add contemporaneous ARRA-obligated spending, and 24 lags, in logs. Spending is entered in logs so zeros are replaced with ones in levels before taking logs. (Cumulative effects through 6, 12, and 24 lags are reported.) We also add dummy variables for the quintiles of housing price appreciation for the 2000–2006 period interacted with calendar-month dummy variables. We report estimates of the coefficients of ARRA spending only for the first specification; results were similar for the other models.

Cyclical control is constructed using 2006 as the baseline year. Data are monthly. $N = 2,950$ observations. Standard errors, reported in parentheses, are clustered at the state level. The column labeled “Joint significance” reports the p value for the joint significance of the 13 coefficients on the hiring credits (i.e., of the null hypothesis that all coefficients equal zero).

Superscripts *, #, and † indicate whether the p value for the null test of no effect is below 0.1 (one symbol) or 0.05 (two symbols), based, respectively, on the conventional hypothesis test (*), multiple testing using the Simes method (#), and multiple testing using the Sidak method (†). (For the joint significance column, the p value for the single test is reported, in lieu of asterisks. Multiple testing analysis is done separately for the joint significance tests.)

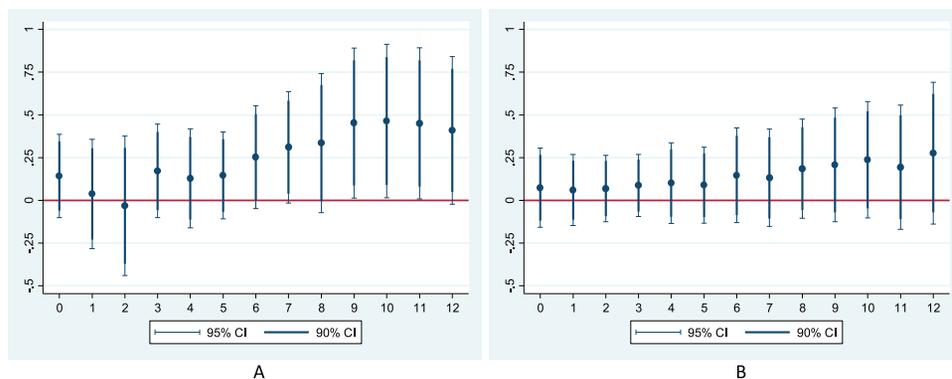
effect is rejected at the 5% or 10% level using two different multiple testing procedures based on all the estimates (and associated p values from individual tests) shown in the corresponding table.

The † symbol describes results using the Sidak procedure, which, like the Bonferroni method, controls the “family-wise error rate,” but is more appropriate to our case that is based on p values from a set of two-tailed tests derived from normally distributed test statistics. The # symbol is based on the Simes procedure, which instead controls the “false discovery rate”—a less conservative approach (as reflected in some of the results reported later). Controlling the family-wise error rate can be interpreted as indicating that statistical significance at a given level—for example, at the 5% level—means that we are 95% confident that all rejected hypotheses are false. Controlling the false discovery rate, by contrast (again, at the 5% significance level), means that we are 95% confident that at least some of the rejected hypotheses are false—a lower standard but still higher than for individual tests.²⁸ Applying these methods to the analysis in Table 4 weakens the strength of the conclusions. The estimated effects of hiring credits that are refundable or that have recapture provisions are no longer statistically significant at the 10% level (as indicated by the absence of # or † symbols on the estimates flagged by asterisks). We return, after presenting additional results, to the interpretation of the multiple testing results for the larger body of evidence we present.

We also estimated models that include interactions of the hiring credit variables with the indicator for Democratic control of the state-house (our political control variable). In all cases the interactions were positive and generally significant for refundable credits (for the longer-term cumulative effects). This result could be related to unmeasured characteristics of the credits adopted when Democrats served as governor, including generosity or breadth of the credits—which, as explained earlier, we found difficult to measure. (These results are available upon request.) Thus, these specifications provide stronger evidence of positive effects of these hiring credits, although we do not feature these estimates because it is not clear why the party of the governor is critical. Below, we consider an alternative hypothesis—that the effects of hiring credits vary over the business cycle—for which we think the predictions are clearer.

²⁸Controlling the family-wise error rate is more appropriate when there is the potential for “harm” from falsely rejecting any of the tested hypotheses. By contrast, controlling the false discovery rate is more appropriate when harm is less likely to be caused by a single true hypothesis being falsely rejected, as long as some are correctly rejected (Pike 2011). We view the second approach as more applicable to our analysis. For example, if we estimate positive effects at multiple lags, but we falsely reject the hypothesis of no effect at some lags but not others, that would still imply a positive effect on which policymakers might want to act. See also Newson and the ALSPAC Study Team (2013), which discusses the merits and shortcomings of these two approaches.

Figure 2. Estimated Effects of Selected State Hiring Credits on Employment, from Table 5 Specifications (1991–2011)



A. Refundable credits

B. Credits with recapture

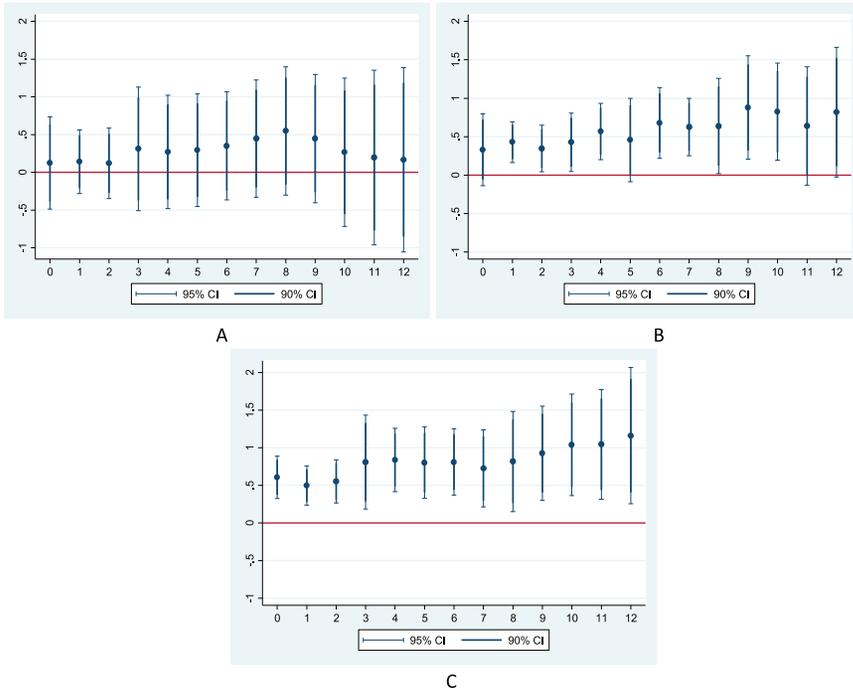
Note: CI, confidence interval.

Alternative Sample Periods and the Great Recession

Table 5 reports estimates extending the beginning of the sample period back to 1991, which necessitates dropping the UI benefit extension controls (see also Figure 2). The estimates are little changed from Table 4, which is not surprising since a lot of overlap occurs in the samples, and, to the extent that the effects of hiring credits differ during recessions—a question we turn to below—we see only a little bit more cyclical variation from the end of the recession in the early 1990s. We still find positive and sometimes significant effects of refundable hiring credits; but the point estimates for credits with recapture provisions are smaller and no longer statistically significant. Applying the multiple testing methods described above to the sample period covered in Table 5 again weakens the statistical evidence; in this case the estimated effects of refundable credits are no longer statistically significant.

More interesting than the extension back to 1991 is restricting our focus to the period of the Great Recession, in part because of questions about the credits adopted during this period, and, perhaps more so, because of general interest in whether hiring credits can help boost job growth during a severe economic downturn. Anti-recessionary hiring credits may be more effective, especially on the heels of a steep recession, because stigma effects are likely to be significantly weakened or eliminated for a credit that is either non-categorical or that targets the unemployed. Employers likely understand that many people become unemployed in a recession because of external adverse shocks to their employers, rather than because of individual low productivity, malfeasance, and so on. And when employment has largely been falling, hiring credits should do more to incentivize hiring that would not have occurred absent the credit.

Figure 3. Estimated Effects of Selected State Hiring Credits on Employment, from Table 6 Specifications (2007–2011)



- A. Refundable credits
 B. Credits with recapture
 C. Credits targeting unemployed
 Note: CI, confidence interval.

The estimates for the 2007 to 2011 period are reported in Table 6 (see also Figure 3).²⁹ For this period, we can re-introduce the UI benefit extension controls. And as noted earlier, we add controls for ARRA spending,³⁰

²⁹Given the shorter sample period, as Table 3 shows we have relatively few credits from which to estimate effects in this period. This shortage of credits raises the possibility of understated standard errors (Conley and Taber 2011).

³⁰The Recovery.gov website provides historical data on spending under ARRA using two different measures: obligations and outlays. Obligated funds are those that occur when a contract is assigned to a particular recipient; outlays occur only after the terms of the contract are satisfied. We use spending based on obligations because it precedes new employment creation. (Wilson's [2012] analysis of fiscal-spending job multipliers uses funding announcements, which precede obligations by several months. We prefer obligations, as these represent secured funds that are more closely related to new employment creation, both with respect to the time at which they occur and their magnitude. In addition, we include lags of obligated spending. In Wilson's analysis, the qualitative results are not affected by using the different measures of spending.) To be precise, our control is the log of additional monthly ARRA-obligated spending from all federal agencies excluding the Department of Labor (DOL). We do not include DOL because these funds are mainly used for payment of extended and expanded UI benefits, which we already include as a control. We use agency-reported data, following Wilson (2012), who noted that agency-reported data cover all ARRA spending, whereas recipient-reported data cover only a little over half of it. From May 2009 until December 2011 (when our sample period ends), the total amount of obligations was \$421.3 billion, and the total amount of outlays was \$365.2 billion.

and for housing price appreciation prior to the Great Recession. For refundable hiring credits, the point estimates are sometimes larger than for the longer sample period, though they are not statistically significant. (The standard errors are a good deal larger, likely because of the much smaller sample.) The evidence for positive effects of hiring credits including recapture provisions is stronger for this subperiod. The point estimates are large and statistically significant (at the 5% or 10% level) through 4, 8, and 12 months. The same is true for hiring credits targeting the unemployed, which have positive and statistically significant effects for all of the cumulative effects reported in the table (and the findings are echoed, at all lags, in Figure 3). For recapture provisions and credits targeting the unemployed, the effects are sizable. Credits with recapture provisions boosted jobs by 0.82% by 12 months after the adoption of such a credit, and credits targeting the unemployed by 1.16%.³¹ We do not have information on spending on such credits from the states that adopted them, but it is highly unlikely that states spent anything close to 1% of their economy's payroll on these credits, suggesting the benefits could well outweigh the costs.³²

One issue in interpreting these findings concerns the potential effects of anticipated future hiring credits. In principle, anticipated hiring credits (between enactment and implementation) can reduce hiring before the credit takes place, so that firms can capture the credit after it is implemented. In this case, a short-term analysis finding an employment increase in the period when a credit was enacted could reflect shifting of employment from the previous period to the current period, without any implication that on net more jobs were created. This outcome would most likely imply that the strongest response to the actual implementation of the credits would be immediate. By contrast, Tables 4 to 6 report cumulative effects out to 12 months, which generally grow over time for the kinds of credits for which we find positive effects—credits with recapture provisions, refundable credits, and credits targeting the unemployed (for the Great Recession period).³³

For the Great Recession period, the multiple testing analysis does not weaken the results for our key hiring credits nearly as much. For credits with recapture provisions, in one case (the cumulative effect through four

³¹Because the models estimated for the 2007 to 2011 period include the additional housing appreciation and ARRA spending controls, to provide a better comparison we also estimated models for the same period excluding these controls, based solely on the difference in sample period, with the estimates in Table 4. The resulting estimates were very similar to those reported in Table 6, and if anything a bit stronger. (The estimates are available upon request.)

³²Note that the effect of ARRA spending is positive (although not significant) at long lags. This outcome is consistent with Wilson's (2012) finding that long first-difference estimates of the effects of ARRA spending on job growth were positive, although he estimated a much different specification—including some IV estimates—and found large positive effects that exceed substantially other estimates of job creation by the ARRA (see Neumark 2013).

³³Chirinko and Wilson (2016) reported some dip in employment when the state hiring credits they studied were enacted but not yet effective, generating some upward bias in the estimated positive effects on employment. They also reported positive effects that accumulate over time.

months), with one of the two multiple testing methods we still reject the null of no effect, even at the 5% significance level. And for credits targeting the unemployed we often reject the null hypothesis using either method, and for cumulative effects through different numbers of lags; and we always reject the null hypothesis of no effect (at the 10% level) using the Simes method.³⁴

Considering the results across all three sample periods, the multiple testing analysis weakens our findings, leaving us with significant effects for only two types of hiring credits—those with recapture provisions, and, more so, those targeting the unemployed—and only for the Great Recession period. Perhaps this result is not surprising. As discussed earlier, there are theoretical reasons to expect positive effects of hiring credits targeting the unemployed, especially during recessions, and to expect that recapture provisions enhance the effects of hiring credits. At the same time, we think the multiple testing analysis may be too conservative because the evidence we find across all three sample periods is quite consistent, suggesting that the results are not driven by randomness that simply leads to occasional rejection of the null hypothesis across a large number of simple hypothesis tests. Nonetheless, the results show that even if one does regard the multiple testing results as definitive, statistical evidence remains for positive effects of hiring credits targeting the unemployed during severe recessions, and also of credits with recapture provisions (although this evidence is weaker).

The stronger estimated effects of refundable credits in the 2007 to 2011 period, compared to the longer sample periods, suggest that hiring credits may have stronger effects during recessions—especially during the kind of severe downturn that characterized the Great Recession. To provide more evidence on this question, we reverted to the longest sample period (1991 to 2011) but introduced interactions between the hiring credit variables and an indicator for national recessions (based on NBER recession dates) plus a period extending one year beyond the end of each recession, given the slow rebound of labor markets after recessions. Because of little variation in credits targeting the unemployed in the earlier years, we focus only on refundable tax credits and credits with recapture provisions (see Table 7 notes for details on the specification).

The estimates are reported in Table 7. For credits with recapture provisions, we do not find any evidence that such credits were generally more effective during and immediately after recessions, except in the very short term. There is, however, strong evidence showing positive and significant effects during recessions for refundable credits, with estimates between 1.3% and 2.1%. If the tax treatment of hiring credits matters, then the refundability of credits might be critical during recessions, since in such

³⁴In Neumark and Grijalva (2015), we reported the multiple testing results for analysis using all of the hiring credits we examined, which adds many for which we found no effect. However, the multiple testing results for this large set of estimates were the same as those presently reported in Tables 4 to 6.

Table 7. Estimated Effects of State Hiring Credits on Employment, with Interactions between Hiring Credit Variables and Recession Indicators, 1991–2011

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+4 Lags</i>	<i>+8 Lags</i>	<i>+12 Lags</i>	<i>Joint significance</i>
Refundable: direct effect	0.05 (0.12)	-0.01 (0.14)	0.22 (0.23)	0.25 (0.23)	0.00
Refundable: interaction	1.24** (0.13)	1.77** (0.22)	1.58** (0.32)	1.89** (0.32)	0.00
Refundable: direct effect + interaction	1.30** (0.04)	1.76** (0.15)	1.80** (0.15)	2.14** (0.28)	0.00
Recapture: direct effect	0.00 (0.13)	0.09 (0.11)	0.21 (0.16)	0.36* (0.20)	0.02
Recapture: interaction	0.41* (0.23)	0.10 (0.43)	-0.05 (0.36)	-0.40 (0.62)	0.00
Recapture: direct effect + interaction	0.42** (0.19)	0.19 (0.42)	0.16 (0.33)	-0.04 (0.60)	0.00

Notes: The dependent variable is the first difference of the log of QCEW employment, multiplied by 100. The specification includes the first difference of the job credit dummy or dummies, and 12 lags of this first difference. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported.

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible bases for benefits, and so on. The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; a dummy variable for the political party of the governor (measured annually); dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables.

We also add interactions between the hiring credit variables and indicators for recessions. The indicators for recessions are based on NBER dates, and a period one year from the end of each recession. The recession periods, therefore, are July 1990–March 1992, March 2001–November 2002, and December 2007–June 2010. These recession indicators are interacted with the contemporaneous value and all lags of the set of hiring credit variables included in each specification (e.g., equal to tax owed, carry-forward, refundable, and not specified, for the different credits based on tax treatment, for the refundable credit panel).

For each specification reported, the contemporaneous and the cumulative lags of these interactions are reported, for the specific types of hiring credits listed. The UI benefits control is not included because the data do not extend back to the beginning of the sample period used in this table.

Cyclical control is constructed using 1990 as the baseline year. Data are monthly. $N = 12,550$ observations. Standard errors, reported in parentheses, are clustered at the state level.

Superscripts * and ** indicate whether the p value for the null test of no effect is below 0.1 or 0.05, respectively, for the conventional hypothesis test. The column label “Joint significance” reports the p value for the joint significance of the 13 coefficients on the hiring credits (i.e., of the null hypothesis that all coefficients equal zero).

periods it is far more likely that companies are losing money and do not have taxable income, increasing the value of refundable credits.³⁵

Endogenous Determination of Hiring Credits?

It is possible that credits are adopted in response to past changes in employment in ways that could bias the estimated effects of credits in the previous tables. For example, there could be an “Ashenfelter dip” phenomenon in which credits are adopted in response to negative shocks, from which states then recover, imparting a positive bias to our estimated effects of hiring credits. Alternatively, credit adoption could be associated with underlying employment trends, with negative trends implying downward bias, and positive trends implying upward bias. We already noted, however, that the models include state-specific linear trends.

To assess whether endogenous determination of hiring credits based on past changes in job growth drives our results, we estimated regression models for adoption of hiring credits. We include the same control variables, as well as long lags of the first differences of log employment (up to 36 months). As reported in Table 8, we find no evidence of statistically significant relationships between past employment change and credit adoption, across any of our sample periods, making biases from endogenous adoption of credits unlikely. We also report the estimated effect of the political control variable in Table 8. It is never statistically significant, and the sign varies across different lags and samples.

Employment Growth Compared with Hiring

One potential problem with hiring credits is that they can lead firms to churn workers, earning more credits for hiring (and firing) workers with little of the intended impact on net employment growth. We have established some evidence of positive employment effects, so there is no reason to believe that the hiring credits we study generate *only* churning. Still, whether hiring credits generate meaningful levels of churning is an important policy question because it can drive up the costs of using hiring credits, per job created.

We use the Quarterly Workforce Indicators (QWI) data—which measure hiring—to study churning. We do this analysis for the 2007 to 2011 period, because the QWI data provide a quite unbalanced panel before the middle of the decade. Data are also available on separations, which might be of interest if we could identify separations induced by firms. But the Job Openings and Labor Turnover Survey (JOLTS) data show that quits are generally more than 50% of separations, although of course less so during and after the Great Recession, when layoffs and discharges rose.³⁶ Given

³⁵Credits with carry-forward provisions also could matter, if these companies expect to survive the recession, but we did not find such evidence.

³⁶See <http://research.stlouisfed.org/fred2/categories/32241> (accessed February 11, 2013).

Table 8. Estimated Effects of Lagged Employment Growth (and Political Control) on State Hiring Credits

	Credits with refundable benefit			Credits with recapture provisions			Credits targeting the unemployed			
	+ 6 lags	+ 12 lags	+ 24 lags + 36 lags	+ 6 lags	+ 12 lags	+ 24 lags + 36 lags	+ 6 lags	+ 12 lags	+ 24 lags + 36 lags	
1995–2011										
Employment growth	-0.0029 (0.0019)	-0.0048 (0.0033)	-0.0019 (0.0040)	-0.0044 (0.0080)	0.0019 (0.0024)	-0.0034 (0.0048)	-0.0057 (0.0085)			
Party of governor	0.0005 (0.0015)	-0.0005 (0.0011)	0.0005 (0.0010)	0.0008 (0.0008)	0.0021 (0.0013)	-0.0009 (0.0046)	0.0010 (0.0012)			
1991–2011										
Employment growth	-0.0003 (0.0019)	-0.0008 (0.0033)	0.0006 (0.0037)	-0.0005 (0.0067)	0.0013 (0.0035)	-0.0057 (0.0063)	-0.0095 (0.0087)			
Party of governor	0.0050 (0.0036)	-0.0002 (0.0010)	0.0003 (0.0011)	0.0004 (0.0011)	0.0049 (0.0053)	-0.0101 (0.0124)	0.0015 (0.0016)			
2007–2011 + ARRA and housing controls										
Employment growth	-0.0032 (0.0070)	-0.0045 (0.0067)	0.0017 (0.0095)	-0.0073 (0.0133)	0.0132 (0.0133)	0.0073 (0.0124)	0.0065 (0.0104)	0.0075 (0.0086)	0.0040 (0.0083)	0.0096 (0.0094)
Party of governor	-0.0017 (0.0109)	0.0356 (0.0397)	0.0395 (0.0396)	0.0071 (0.0054)	0.0094 (0.0110)	-0.0447 (0.0517)	0.0039 (0.0044)	0.0118 (0.0118)	0.0413 (0.0414)	-0.0020 (0.0027)

Notes: The dependent variable is the enactment of a credit of the type specified in the column headings. Linear probability model estimates are reported. The models include 36 monthly lags of the first difference of log employment (multiplied by 100). The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 12 lags of the first difference of the control for extended UI benefits, except that the specifications for 1991–2011 do not include the UI benefits; a dummy variable for the political party of the governor (measured annually) and 36 lags of this dummy variable; dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables.

Cyclical control is constructed using 1990 as the baseline year. Data are monthly. Each panel reports the cumulative effects through 6, 12, 24, and 36 lags of the first difference of the log of employment. Standard errors, reported in parentheses, are clustered at the state level.

Superscript * indicates whether the p value for the null test of no effect is below 0.1, for the conventional hypothesis test.

that we cannot separate out involuntary separations that firms could use, along with hiring, to churn workers, we present evidence from the QWI only on hiring (and on employment, for comparability).³⁷

The model is the same as the one used for the monthly data, but now the time unit is a quarter, and this entails some modifications. We use as dependent variables the first difference of the log of employment (number of jobs) at the beginning of the quarter, and of the number of workers who started a new job in the quarter (both multiplied by 100). The specification includes the first difference and four lags of the hiring credit variables, so the lags cover the same period as our earlier specifications using monthly QCEW data. The hiring credit variables are constructed from the monthly hiring credit dummies, set equal to 1/3, 2/3, or 1 if the credit (or a credit with a particular feature) is present in a state for one, two, or three months in a given quarter. The specification also includes the first difference of the log of the state-specific shock variable and four lags. This variable is constructed as the average of the monthly shock variables in each quarter. In addition, the specification includes interactions of the first difference of the shock variable with state dummy variables; first differences by quarter and four lags of the minimum wage prevailing in the state at the beginning of the quarter; first differences by quarter and four lags of the control for extended UI benefits; dummy variables for each quarter in the sample; and interactions between calendar-quarter dummy variables and state dummy variables.

Even though hiring is a (gross) flow into employment, we estimate the model for the change in hiring, paralleling the specifications for the change in employment. We are estimating whether a hiring credit boosts the hiring response *relative to* the employment response, so that firms can claim more credits when they increase hiring.³⁸ First, consider employment. When the credit is introduced, employment should grow because the cost of labor has fallen for firms where employment is growing (and which are therefore eligible for credit). In the steady state some firms are growing and some are

³⁷The QWI data are derived from the Longitudinal Employer-Household Dynamics (LEHD) Program at the U.S. Census Bureau. The employer and workplace reports are the same as the data reported to the BLS as part of the QCEW, although the two sources are not exactly equal. Moreover, by using the linked employer information in the LEHD, accessions of workers to new employers, and separations from those employers, can be observed. Beginning of period employment is conceptually and empirically similar to QCEW month one employment. Formally, a person is defined as employed at the beginning of a quarter when he has positive earnings with the same employer in both the previous and current quarters. Hires are recorded when an individual has positive earnings with a particular employer in the current quarter and not in the previous one. There is also a “new hiring” variable defined when an individual has positive earnings in the current quarter, with no earnings from the same employer during the previous four quarters, but here we use the “all hiring” measure.

³⁸In firm-level data we could test this directly, estimating a regression of hiring on the change in employment and the change in employment interacted with eligibility for a credit. That is, for given net employment growth, is gross employment growth greater when there is a credit? But in aggregate data this regression would not make sense, as there is likely always some hiring occurring, even when total employment is shrinking, and hence some firms are always eligible for a credit.

shrinking for random reasons. The growing firms are always eligible for the credit, which means that, on average, the cost of labor has declined. So we should see a permanently higher level of employment when a credit is in place. The growth in employment should occur over some limited period, however; that is, there is no reason the credit boosts the rate of employment growth permanently. Now, consider hiring. When employment is growing, firms have to, at a minimum, hire a number equal to employment growth. If there is an incentive to churn, then they hire more workers (and fire some workers, which we do not measure). They also may have to hire more than the net employment growth because of worker attrition, so a slightly higher effect of a credit on the change in hiring than the change in employment would not be indicative of churning.

Therefore, we should look at the change in hiring in the period when a change in employment occurs, and compare magnitudes. Once employment growth stops (then again some firms are always growing, and they have an incentive to churn because they are eligible for the credit), hiring, like employment, should be at a permanently higher level. Moreover, if we introduce attrition, then a higher employment level in the long run has to be associated with a higher level of hiring, even absent churning incentives. But the change in hiring (i.e., when hiring increases) should occur at the same time as the change in employment. If we instead regress the *level* of hiring on the *change* in the hiring credit, we do not see the higher churning associated with the employment increase, because hiring should be higher even when the credit is not changing. The longer-term effects of hiring credits on employment and hiring are of interest. However, it seems likely that the main effects of hiring credits will arise, and be detectable, in the period when the credits are implemented and will induce a reduction in labor costs for firms—including those induced to increase employment (and hiring) because of the credit.

The results for employment and hires are reported, respectively, in Tables 9 and 10. Some of the employment results are quite comparable to Table 6, which is not surprising, since the QWI and QCEW reflect the same underlying data. In particular, the recapture estimates are strongly positive, and the estimates for credits targeting the unemployed are positive, although a bit smaller with larger standard errors, and hence not significant. The estimated coefficients for refundable credits—which were the weakest results in Table 6—are no longer positive. The estimated effects in Table 9 are typically larger than in Table 6, presumably because the data are quarterly.

Table 10 turns to our main evidence from the QWI data, which pertains to hiring. To some extent these results reflect the employment results. In particular, credits allowing recapture and credits targeting the unemployed have large and significant positive effects. In both cases, however, the positive estimates are about 10 (or more) times as large as the effects on

Table 9. Estimated Effects of State Hiring Credits on Employment, Credit Dummy Variables Specifications, First Differences, 2007–2011, Quarterly Workforce Indicators Data

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+1 Lag</i>	<i>+2 Lags</i>	<i>+3 Lags</i>	<i>+4 Lags</i>	<i>Joint significance</i>
Credit	0.04 (0.27)	0.05 (0.51)	-0.35 (0.58)	-0.31 (0.73)	-0.68 (0.84)	0.12
Equal to tax owed	-0.57** (0.20)	-0.73* (0.40)	-1.12** (0.55)	-1.39** (0.64)	-2.62 (1.91)	0.00
Carry-forward	-0.44 (0.86)	-0.96 (1.52)	-1.14 (1.37)	-1.06 (1.00)	-1.73 (1.72)	0.55
Refundable	-0.10 (0.32)	-0.15 (0.30)	-0.24 (0.39)	-0.07 (0.65)	-0.68 (0.43)	0.12
Not specified	0.46* (0.27)	0.08 (0.39)	-0.09 (0.49)	-0.06 (0.58)	-0.29 (0.71)	0.02
Recapture	0.86** (0.28)	0.65 (0.53)	1.19** (0.36)	1.62** (0.66)	1.17* (0.64)	0.00
No recapture	0.52 (0.32)	0.01 (0.29)	-0.07 (0.36)	-0.19 (0.39)	-0.37 (0.46)	0.06
Unemployed	0.54** (0.22)	0.31 (0.23)	0.48 (0.46)	0.95 (0.65)	0.48 (0.77)	0.00
Disabled	-1.36** (0.31)	-0.71** (0.28)	-0.16 (0.28)	-0.07 (0.35)	-0.49 (0.55)	0.00
No targeting	0.03 (0.28)	0.06 (0.52)	-0.31 (0.60)	-0.25 (0.75)	-0.56 (0.88)	0.27

Notes: The dependent variable is the first difference of the log of employment using QWI data, multiplied by 100. QWI data are quarterly, rather than monthly. All data had to be collapsed to the quarterly level. The hiring credit dummy variables are defined as 1 if the credit is in place for all three months of a quarter, 2/3 if it is in place for two months, 1/3 if it is in place for one month, and zero otherwise. The specification includes the first difference of the job credit dummy or dummies, and 4 (quarterly) lags of this first difference. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported.

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible bases for benefits, and so on. The specification also includes the contemporaneous value and 4 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 4 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 4 lags of the first difference of the control for extended UI benefits; a dummy variable for the political party of the governor (measured annually); dummy variables for each quarter in the sample; and interactions between quarter-of-the-year dummy variables and state dummy variables.

We add contemporaneous ARRA-obligated spending, and 8 lags, in logs. Spending is entered in logs so zeros are replaced with ones in levels before taking logs. We also add dummy variables for the quintiles of housing price appreciation for the 2000–2006 period interacted with quarter-of-the-year dummy variables.

Cyclical control is constructed using 2006 as the baseline year. $N = 930$ observations. (Data are unavailable for Massachusetts and are missing for the last quarter for Colorado.) Standard errors, reported in parentheses, are clustered at the state level.

Superscripts * and ** indicate whether the p value for the null test of no effect is below 0.1 or 0.05, respectively, for the conventional hypothesis test.

Table 10. Estimated Effects of State Hiring Credits on Hiring, Credit Dummy Variables Specifications, First Differences, 2007–2011, Quarterly Workforce Indicators Data

<i>Credit variable(s)</i>	<i>Contemporaneous</i>	<i>+1 Lag</i>	<i>+2 Lags</i>	<i>+3 Lags</i>	<i>+4 Lags</i>	<i>Joint significance</i>
Credit	0.10 (2.47)	-5.24 (3.53)	-7.15 (6.97)	-8.67 (8.69)	-7.39 (8.93)	0.48
Equal to tax owed	-0.34 (2.91)	-3.45 (2.46)	0.35 (2.84)	-1.08 (4.46)	7.33* (4.20)	0.00
Carry-forward	-7.43** (2.76)	4.27 (3.09)	0.24 (4.30)	-1.61 (6.35)	0.81 (11.01)	0.00
Refundable	3.75 (2.88)	5.07 (4.47)	-0.08 (3.69)	3.21 (4.25)	7.23 (7.64)	0.38
Not specified	3.29 (2.87)	-5.02 (4.04)	1.25 (3.94)	-2.37 (3.29)	-1.20 (3.34)	0.44
Recapture	13.34* (7.49)	12.17** (2.91)	11.24 (7.53)	-0.16 (3.70)	11.84** (5.24)	0.00
No recapture	1.10 (2.62)	-2.45 (2.53)	-1.80 (3.84)	-1.95 (5.50)	-3.92 (5.32)	0.54
Unemployed	9.29** (4.17)	8.47** (3.35)	7.79 (4.87)	8.43** (2.73)	11.24** (5.14)	0.00
Disabled	7.90** (2.85)	2.42 (4.29)	5.64* (2.83)	5.90 (4.18)	8.84** (4.28)	0.00
No targeting	0.22 (2.50)	-5.16 (3.35)	-7.35 (7.14)	-9.02 (8.84)	-6.70 (8.94)	0.41

Notes: The dependent variable is the first difference of the log of hiring using QWI data, multiplied by 100. QWI data are quarterly, rather than monthly. All data had to be collapsed to the quarterly level. The hiring credit dummy variables are defined as 1 if the credit is in place for all three months of a quarter, 2/3 if it is in place for two months, 1/3 if it is in place for one month, and zero otherwise. The specification includes the first difference of the job credit dummy or dummies, and 4 (quarterly) lags of this first difference. In addition to the contemporaneous effect, the cumulative effects through 4, 8, and 12 lags are reported.

Each panel reports a different specification. The first includes only dummy variables for whether a credit exists, the second includes dummy variables for whether a credit exists with each of the four possible bases for benefits, and so on. The specification also includes the contemporaneous value and 4 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 4 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 4 lags of the first difference of the control for extended UI benefits; a dummy variable for the political party of the governor (measured annually); dummy variables for each quarter in the sample; and interactions between quarter-of-the-year dummy variables and state dummy variables.

We add contemporaneous ARRA-obligated spending, and 8 lags, in logs. Spending is entered in logs so zeros are replaced with ones in levels before taking logs. We also add dummy variables for the quintiles of housing price appreciation for the 2000–2006 period interacted with quarter-of-the-year dummy variables.

Cyclical control is constructed using 2006 as the baseline year. $N = 930$ observations. (Data are unavailable for Massachusetts and are missing for the last quarter for Colorado.) Standard errors, reported in parentheses, are clustered at the state level.

Superscripts * and ** indicate whether the p value for the null test of no effect is below 0.1 or 0.05, respectively, for the conventional hypothesis test.

employment overall, suggesting that there may be considerable churning generated by these credits.³⁹

Conclusions

State and federal policymakers grappling with the aftermath of the Great Recession sought ways to spur job creation, in many cases adopting hiring credits to encourage employers to create new jobs. This article provides new evidence on the effects of state hiring credits on job growth, both over the longer term and also focusing in part on the influence of credits adopted during and after the Great Recession. We find that some specific types of hiring credits—including those targeting the unemployed, those that allow states to recapture credits when job creation goals are not met, and refundable hiring credits—appear to have succeeded in boosting job growth, more so during the Great Recession period, or perhaps during recessions generally. This state-level evidence complements some recent evidence on positive impacts of federal hiring credits adopted in response to the Great Recession (Farooq and Kugler 2015).⁴⁰

There are some limitations to what can be learned about the effects of credits enacted in this period, in part because of the different kinds of credits that have been adopted, as well as difficulties in measuring all of their relevant features—especially generosity or the magnitude of incentives. Furthermore, there is a dearth of other evidence on the effects of hiring credit programs. As a result, our findings should be interpreted cautiously.

Nonetheless, our results do provide some evidence that particular types of hiring credits may have boosted job growth during the Great Recession, and perhaps during other recessions. Moreover, some of the results are consistent with what we might expect. A refundable hiring credit ought to have a greater impact on firms because it is valuable even if the firm does not have taxable income in the current period, which is more likely to be true during recessions. Recapture provisions should make hiring credits more effective. And credits targeting the unemployed, especially during a period such as the Great Recession when unemployment should not be a stigmatizing characteristic, should be more effective. Thus, the results provide some

³⁹Unfortunately, the limited number of hiring credits precludes asking some interesting questions suggested by the evidence, such as whether recapture provisions reduce the extent to which other types of credits generate hiring but not net job creation.

⁴⁰As reported in the working paper version of this paper (Neumark and Grijalva 2015), states adopted many other types of hiring credits for which we did not find evidence of statistically significant effects on employment. In many cases the results for these other credits are likely uninformative because not enough policy variation is available to draw strong conclusions. Nonetheless, we should point out that the evidence did *not* point to possibly economically significant positive effects that were not statistically significant. Rather, the estimated effects of these other types of hiring credits varied in sign or were negative, and were very small, with the only exception being hiring credits based on investment. Thus, in almost every case, the safest conclusion is that the true effects of these other kinds of credits are near zero.

evidence that judiciously chosen hiring credits increase job growth, especially during recessions.

Some evidence also justifies the concern that hiring credits generate more gross hiring than net employment growth. As discussed in Neumark (2013), estimates from the existing literature suggest that for every 10 hires for which hiring credits are paid, 1 net job is created. Nonetheless, inefficiencies this high can still be consistent with costs per job created in the United States in the \$30,000 or \$40,000 range, for example, if the credits pay \$3,000 to \$4,000 per hire. These costs are likely to be substantially below the costs of creating jobs through the fiscal stimulus in the form of the ARRA used to counter the Great Recession. And our evidence gives some guidance as to the kinds of features of hiring credits likely to make them effective.

All in all, the evidence is not overwhelming that hiring credits should be (or should have been) an important part of the policy response to the Great Recession, or should be part of the response to future recessions. But some evidence points in this direction, especially for particular types of hiring credits. Given these findings, there may be merit to enacting legislation establishing well-designed federal or state hiring credits that turn on automatically and aggressively when economic downturns occur. Such credits would complement other “automatic stabilizers” that seek to boost workers’ and families’ incomes when a recession occurs, such as Unemployment Insurance, welfare, and progressive taxation. For reasons discussed in the article, however, we clearly recognize the limitations to our evidence. More work is needed to provide more definitive evidence on the effects of hiring credits—whether they are adopted in response to recessions, or more generally.

Appendix

Table A.1. Estimated Effects of State Hiring Credits on Employment, Restricting Effects of Hiring Credits to Quadratic Specification, First Differences, 1995–2011

<i>Credit variable(s)</i>	<i>Linear</i>	<i>Quadratic</i>
Credit	−0.31 (0.34)	0.33 (0.30)
Equal to tax owed	0.78 (0.52)	−0.89* (0.48)
Carry-forward	−0.35 (0.28)	0.37 (0.23)
Refundable	0.48 (0.33)	−0.31 (0.38)
Not specified	0.18 (0.55)	0.03 (0.40)

(continued)

Table A.1. Continued

<i>Credit variable(s)</i>	<i>Linear</i>	<i>Quadratic</i>
Recapture	0.20 (0.32)	0.06 (0.30)
No recapture	-0.09 (0.35)	0.24 (0.33)

Notes: The dependent variable is the first difference of the log of QCEW employment, multiplied by 100. In contrast to Table 4, rather than including unrestricted coefficients of the contemporaneous and lagged hiring credit variables, the effects are restricted to lie along a quadratic function, as described in the text.

Each panel reports a different specification. The first includes only variables for whether a credit exists, the second variables for whether a credit exists with each of the four possible types of tax treatment, and so on. The specification also includes the contemporaneous value and 12 lags of the first difference of the state-specific shock variable (in logs); interactions of the first difference of the shock variable with state dummy variables; the contemporaneous value and 12 lags of the first difference of the log of the minimum wage prevailing in the state; the contemporaneous value and 12 lags of the first difference of the control for extended UI benefits; a dummy variable for the political party of the governor (measured annually); dummy variables for each month in the sample; and interactions between calendar-month dummy variables and state dummy variables.

Cyclical control is constructed using 1990 as the baseline year. Data are monthly. $N = 10,150$ observations. Standard errors, reported in parentheses, are clustered at the state level.

Superscript * indicates whether the p value for the null test of no effect is below 0.1, for the conventional hypothesis test.

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