Heterogeneity and Behavioral Responses to Unemployment Benefits over the Business Cycle

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Abstract

I investigate in this paper the cyclicality of partial equilibrium behavioral responses to unemployment insurance (UI) in the US. I use administrative data on the universe of unemployment spells in five states from 1976 to 1984, and identify the effect of both benefit level and potential duration in the regression kink (RK) design using kinks in the schedule of UI benefits, thus overcoming the issue of endogeneity in UI benefit variations. I correlate the estimates of the average treatment effects with proxies for labor market conditions and find no evidence of cyclicality. I then extend the RK design to the estimation of quantile treatment effects and effects on the survival and hazard function. I find strong and significant distributional effects of both benefit level and potential duration. Heterogeneity in the responses of short and long spells seems more pronounced during good times than in recessions. I discuss the implications of these results for the optimal design of UI policies.

KEYWORDS: business cycle, unemployment insurance.

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Introduction

State contingent unemployment insurance (UI) rules are commonly used in developed countries. Yet, their effect on aggregate welfare is unknown and highly debated [1]. Increasing the generosity of the UI system provides consumption smoothing benefits for unemployed people to the extent that they are (at least partially) credit-constrained. On the other side, it creates a deadweight loss that depends critically on the extent of moral hazard, captured by the behavioral response of unemployment duration to UI generosity (Chetty [2006]). There are at least three different types of reasons for why the behavioral responses to UI (and thus the welfare cost of UI) might vary over the business cycle. First, the economic environment is not the same in recession and in expansion. Some factors, exogenous to the generosity of the UI system, but affecting optimal search effort, vary over the cycle. Examples of such factors include the average wealth level, or search technology[2]. Second, unemployed individuals are not the same in good times and bad times, across observable or unobservable characteristics, and there is potentially significant heterogeneity in behavioral responses. Finally, increasing unemployment insurance may induce equilibrium adjustments or spillover effects, the extent of which depends on the size of the treated population. In bad times, when more people are unemployed, these equilibrium effects are likely to be stronger. Such job search externalities can be due to social interactions as in Topa [2001] or Lalive [2003]). They can also arise because of job rationing during recessions as in Landais et al. [2010].

As a consequence, aggregate and partial equilibrium responses to UI are very likely to differ, and so are their cyclicality. The partial equilibrium response (or micro-elasticity) is the elasticity of the probability of unemployment for a worker whose individual unemployment benefits change. The aggregate response (or macro-elasticity) is the elasticity of aggregate unemployment when

[1] In every major recession since the 1950s, Congress has enacted a temporary program providing additional weeks of federally funded unemployment insurance benefits to cope with adverse conditions on the labor market. But rarely has the debate about whether to increase the generosity of the Unemployment Insurance (UI) system been as heated and as politically divisive as since the beginning of the Great Recession and the introduction of the Emergency Unemployment Compensation (EUC) by the Unemployment Compensation Extension Act of 2008.

[2] Note that these factors are not fully observed by the legislator. Otherwise, UI rules could be made directly contingent on these factors.

the generosity of UI changes for all workers. In an equilibrium search and matching framework, the macro-elasticity accounts for potential equilibrium adjustment in labor market tightness that follows a change in UI, whereas the micro-elasticity takes labor market tightness as given. Many other spillover effects of UI, such as social interactions, that would create a wedge between the micro- and macro-elasticity of unemployment can also be considered. Importantly, Landais et al. [2010] show that both the aggregate and the partial equilibrium responses matter to determine the optimal level of UI in a broad class of equilibrium search and matching models. In particular, the partial equilibrium response is always a necessary statistics, and is even sufficient in some subclasses of models such as in the canonical Pissarides [2000] with Nash bargaining on wages. This motivates a two-pronged research agenda. In this paper, I focus on the estimation of the cyclicality of the partial equilibrium responses. In a companion paper, I investigate the presence of job search externalities, which are responsible for the wedge between the micro- and macro-elasticities in Landais et al. [2010], using the Regional Extended Benefit Program in Austria.

Estimation of the cyclical evolution of partial equilibrium responses to UI is in itself an empirical challenge. First because of the difficulty to find exogenous time invariant variations in UI benefits for a subset of unemployed workers in the same labor market. Second because of the difficulty to replicate such estimation in a large number of labor markets with different initial conditions. The paper most closely related to the empirical analysis performed here is Schmieder et al. [2011] who investigate the effect of sharp discontinuities in UI duration entitlements by age in Germany over twenty years.

I contribute to the existing literature in several ways. First, I provide robust estimates of the effects of both benefit level and potential duration in the US using kinks in the schedule of state UI benefits in the regression kink (RK) design popularized by Nielsen et al. [2010] and Card et al. [2009]. I use administrative data from the Continuous Wage and Benefit History Project (CWBH) on the universe of unemployment spells in five states in the US from 1976 to 1984. Since identification in the regression kink design relies on estimating changes in the slope of the relationship between an assignment variable and some outcomes of interest, the granularity of the CWBH data is a key advantage and smaller samples of UI recipients would in general not exhibit enough statistical power to detect any effect in a RK design. While most of the recent development in the empirical literature on unemployment insurance has been achieved using exhaustive administrative data from European countries and sharp discontinuities in eligibility rules, this paper shows that the combination of kinked schedules in state UI rules and access to exhaustive state UI records offers promising avenues for the development of research on UI in the US. Second, I correlate the estimates with proxies for labor market conditions and find no cyclicality in the estimated average effects, even when controlling for changes over time in observable characteristics of the unemployed population at the kink. This suggests that the average welfare cost of UI is constant over the cycle in partial equilibrium or in equilibrium models like Pissarides [2000] with Nash bargaining or Shimer [2004] and Hall [2005] with wage rigidity. Increasing the generosity of UI in recessions would then be sub-optimal, unless substantial spillover effects create a wedge between the partial equilibrium and the aggregate labor supply effects of UI. Third, I investigate the effect of unobserved heterogeneity by extending the regression kink design to the identification of quantile treatment effects and other distributional effects. While significant advances have been achieved in the literature on duration with the development of semi-parametric proportional hazard models, the regression kink design offers the opportunity to go one step further and investigate heterogeneity in a fully non-parametric way. Note that the heterogeneity between long term and short term unemployed that I focus on in this paper can originate from very different sources. It can be due to unobserved differences in the utility cost of search, or to the properties of the matching process of workers to firms. More fundamentally, it is impossible to disentangle selection from duration dependence. Nevertheless, documenting to what extent short term and long term unemployed differ in their behavioral response is central to optimal UI design, especially during recessions. My results confirm the existence of significant heterogeneity in responses to the potential duration of benefits, but also demonstrate the existence of significant heterogeneity in the response to benefit level, a point overlooked in most studies of the effect of benefit levels, which traditionally rely on semi-parametric proportional hazard models and therefore impose the effect to be a constant location shift of the hazard rate at all point in time. Moreover, correlating the estimated quantile treatment effects with measures of state labor market conditions, my results suggest that the heterogeneity in behavioral responses is more pronounced in good times than during recessions.

The remainder of the paper is organized as follows. I discuss in section 1 how the paper is related to the existing literature. In section 2, I explain the identification strategy based on a regression kink design. I provide institutional background on the state UI systems and present the data in section 3. Results for the effects of benefit level are presented in section 4 and results for the effects of potential duration are exposed in the following section. The last section discusses the implications of the results for optimal UI policies.

1 Related Literature

A large empirical literature is devoted to the estimation of labor supply effects of UI[3]. These studies use very different sources of variation to identify the effect of UI generosity, and it is useful to clarify how they relate to the micro and macro-elasticity concepts. The ideal experiment to estimate the micro-elasticity is to offer higher UI benefits to a randomly selected *small* subset of individuals within a labor market and compare unemployment durations between these treated individuals and the rest of the unemployed. Studies in the literature comparing individuals with different benefits in the same labor market at a given time, while controlling for individual characteristics, are primarily estimating micro-elasticities. It is nevertheless difficult to find credibly exogenous sources of variations in UI benefits in the same labor market at a given time. So far, the most credible sources of identification have come from sharp discontinuities in the potential duration of benefit entitlements by age that exist in several European countries (see for instance Lalive [2008] in Austria, or Schmieder et al. [2011] in Germany). Such sharp discontinuities do not exist for the *level* of UI benefit, and do not exist at all in the US, but the idea used in this paper

[3] A general survey on labor supply responses can be found in Krueger and Meyer [2002] and a survey on the effect of UI potential duration is given in Card et al. [2007]

of relying on kinky UI schedules to estimate the effect of UI on labor supply originates from Card et al. [2009] who use data from the Washington Reemployment Bonus Experiment to investigate the effect of the average weekly benefit amount on insured unemployment durations.

To investigate the cyclicality of the micro-elasticity, it is necessary to replicate the same estimation procedure across labor markets with different initial labor market conditions. The closest empirical setting to the ideal experiment is that of Schmieder et al. [2011], who use sharp variations in the potential duration of unemployment benefits by age in Germany, population-wide administrative data, and a regression discontinuity approach to identify compellingly the evolution over time of the micro-elasticity of unemployment duration with respect to the *potential duration* of benefit entitlement. Their elasticity estimates are broadly constant over the German business cycle. Kroft and Notowidigdo [2011], using CPS data, also try to identify the cyclical behavior of labor supply effects of UI and find that higher state unemployment decreases the effect of UI benefit level on exit rates from unemployment. But their setting is less ideal than that of Schmieder et al. [2011] to estimate the micro-elasticity. First, they have to rely on variations of average UI benefit level in each state$\times$year cell, which makes their estimate more likely to capture a macro elasticity. And second, they need to make the identifying assumption that the variations of average UI benefit level in each state$\times$year cell are somehow exogenous.

The empirical literature on labor supply effects of UI has also devoted attention to the heterogeneity of the effect of UI over the duration of one's spell. But this interest has been mainly focused on responses with respect to potential duration of UI, with a large sub-literature devoted to the question of the spike at benefit exhaustion. While the theoretical literature has acknowledged the possibility of variation in the utility cost of search over the duration of unemployment spell [4], there is little empirical evidence on heterogenous effects of UI benefit level across the distribution of unemployment durations.

[4] Krueger and Mueller [2011] find evidence of decreasing search effort over the course of unemployment, which is not accounted for by standard search models, but is consistent with the idea of an increase in the utility cost of search over time unemployed.

2 Empirical Strategy

The empirical challenge, when trying to identify the cyclical behavior of the micro-elasticity of unemployment duration with respect to UI benefits, lies in the difficulty to find credible and time invariant sources of identification with exogenous variations of UI benefits for a small subsample of unemployed workers and then replicate this experiment in numerous labor markets with different initial tightness conditions. It is even more complicated to find exogenous variations in both benefit level and potential duration. Most sources of variations used in the literature on US data come from changes in state legislation over time, with the issue that these changes might be endogenous to labor market conditions. I describe in this section how one can use the presence of kinked schedules in the relationship between previous earnings and both benefit level and benefit duration to estimate the cyclical behavior of the micro-elasticity of labor supply to UI benefits.

2.1 Regression Kink Design

There has been recently a considerable interest for RK designs in the applied economics literature. References include Nielsen et al. [2010], Card et al. [2009], Dong [2010] or Simonsen et al. [2010]. The reason of this recent development is that in many settings, RK designs offer valid non parametric inference of the average treatment effect in the absence of instruments. Conditions for the validity of the RK design are stringent nevertheless, more stringent than in the RD design. RK designs also require a lot of statistical power to detect local changes in the slope of the conditional expectation function. Here, I consider a model where the treatment is continuous and is a known deterministic function of the running variable, as in Nielsen et al. [2010] or Card et al. [2009]. This type of setting can be thought of as a *sharp* design in the sense that everyone is a complier and obeys the same treatment assignment rule. But the identification strategy can be extended to classes of model with *fuzzy* design where the functional form for the treatment is unknown, as in

Dong [2010] who consider a binary treatment. I am interested in the following model:

$$Y = y(b, D, W_1, W_2, \varepsilon)$$

where Y is a duration outcome, b (the level of UI benefits) and D (total potential duration of benefits) are two continuous regressors of interest, W_1, W_2 are two other potentially endogenous regressors, and ε is unobservable heterogeneity. Note that I allow for completely unrestricted non-additive heterogeneity. This very general non-parametric framework has the advantage of nesting a wide range of duration model such as the accelerated failure-time model or other semi-parametric duration models. In particular, I do not impose modeling assumptions that may be empirically not valid such as the proportional hazard assumption traditionally used in duration analysis. Note also that, similar to RD designs, other covariates are generally not needed for consistency in estimating the average (unconditional) treatment effect, though they may be useful for efficiency or for testing the validity of RKD assumptions. However, if desired, additional covariates Z could be included in the analysis by letting all the assumptions hold conditional upon the values Z may take on. In the estimation, I also consider models where I include them as additional regressors.

$H(.)$ is the c.d.f. of ε. I define two average marginal effects of b and D, α and β as:

$$\alpha = \int \frac{\partial y(.)}{\partial b} dH(\varepsilon | b, w_1) \quad \text{and} \qquad \beta = \int \frac{\partial y(.)}{\partial D} dH(\varepsilon | D, w_2)$$

These constructs are the effect of a marginal increase in b (resp. D) for b, w_1 (resp. D, w_2) fixed at their kink point value integrated on the distribution of the unobservable. This can be thought of as an average treatment effect (ATE) weighted by the ex ante probability of being at the kink given your heterogeneity type.

The key element of the RK design is the fact that $b = b(W_1)$ (resp. $D = D(W_2)$) is a deterministic, continuous but kinked function of the endogenous assignment variable W_1 at $W_1 = k_1$ (resp. $W_2 = k_2$). Using this kink in the relationship between b and W_1 (resp. D and W_2), it is possible to identify α and β under two conditions. The first is a regularity condition: $\frac{\partial y(.)}{\partial b}$ (resp. $\frac{\partial y(.)}{\partial D}$) is continuous in b (resp in D) and $\frac{\partial y(.)}{\partial w_1}$ is continuous in w_1 for all b, w_1, ε (resp. $\frac{\partial y(.)}{\partial w_2}$ is continuous in w_2 for all D, w_2, ε). This condition states that the direct marginal effect of the assignment variable on the outcome should be smooth. The second condition is a smooth density condition. The c.d.f of W_1 (resp. W_2) conditional on ε $F_{W_1|\varepsilon}(W_1|\varepsilon)$ is twice continuously differentiable in W_1 at $W_1 = k_1$ (resp. $W_2 = k_2$) for all ε. This second condition requires that the derivative of the conditional probability density function is continuous for all ε at the kink so that density of the unobserved heterogeneity evolves smoothly with the assignment variable at the kink. Under these two conditions, we have:

$$\alpha = \frac{\lim_{w_1 \to k_1^+} \frac{\partial E[Y|W_1=w_1]}{\partial w_1} - \lim_{w_1 \to k_1^-} \frac{\partial E[Y|W_1=w_1]}{\partial w_1}}{\lim_{w_1 \to k_1^+} \frac{\partial B(w_1)}{\partial w_1} - \lim_{w_1 \to k_1^-} \frac{\partial B(w_1)}{\partial w_1}}$$

$$\beta = \frac{\lim_{w_2 \to k_2^+} \frac{\partial E[Y|W_2=w_2]}{\partial w_2} - \lim_{w_2 \to k_2^-} \frac{\partial E[Y|W_2=w_2]}{\partial w_2}}{\lim_{w_2 \to k_2^+} \frac{\partial D(w_2)}{\partial w_2} - \lim_{w_2 \to k_2^-} \frac{\partial D(w_2)}{\partial w_2}}$$

The two conditions are needed because a marginal increase in the assignment variable induces an effect on the outcome through b (because of the deterministic relationship between b and the assignment variable) but also through the direct effect of the assignment variable on the outcome and through the change in the distribution of the unobserved heterogeneity. Only if the latter two effects are smooth and cancel out by differencing on both sides of the kink can the change in the derivative of the conditional expectation function at the kink isolate the causal effect of b on the outcome.

Note that the assumptions needed for the validity of the RK design are somewhat stronger than for the validity of a RD design, since not only the conditional p.d.f. of the assignment variable but its derivative also need to be continuous for all unobservable individual types ε. These assumptions are always fundamentally untestable, i.e. whether each individual's ex ante density and its derivative are continuous is fundamentally untestable, since for each individual we only observe one realization. But first, knowledge of the institutional details are a good way of assessing the credibility of the RKD identification assumption. In the case of UI, manipulation of the assignment variable seems complicated and the local random assignment seems likely to hold. Very few people know the schedule of UI benefits while still employed. Moreover, to be able to perfectly choose ex ante one's position in the schedule of both benefit level and potential duration, it is necessary to know continuously one year in advance the date at which one gets fired and the schedule that shall apply then[5] and to optimize continuously not only one's highest-earning quarter but also the ratio of base period earnings to the highest-earning quarter. Second, it is always possible to check empirically for clear violations of the RKD assumptions. In particular, to assess the validity of the smooth density assumption, it is useful to check whether pre-determined covariates have a c.d.f that is twice continuously differentiable with respect to the assignment variable. I do so by estimating changes in the slope of the conditional expectation function of some pre-determined covariates like age, education or gender given the assignment variable.

Estimation of α and β is straightforward. The denominator of the estimand is deterministic, and is the change in the slope of the schedule at the kink. The numerator is the change in the slope of the conditional expectation function of the outcome given the assignment variable at the kink. It can be simply estimated by running parametric polynomial models of the form:

$$E[Y|W=w] = \mu_0 + [\sum_{p=1}^{\bar{p}} \gamma_p (w-k)^p + \nu_p (w-k)^p \cdot D] \quad \text{where } |w-k| \leq h \tag{1}$$

where W is the assignment variable, $D = 1_{[W \geq k]}$ is an indicator for being above the kink threshold, h is the bandwidth size, and the change in the slope of the conditional expectation function is given by ν_1.

Because the RK design fully controls for labor market conditions (that may be endogenously determined by the level of benefits) by netting out its effects across similar individuals at the kink,

[5] As shown in figure 2, the schedule changes rather frequently.

the estimated elasticities $\widehat{\alpha} \cdot \frac{b_{max}}{\overline{Y_1}}$, where $\overline{Y_1}$ is mean duration at the kink point, can be interpreted as a micro-elasticity. By estimating this elasticity in labor markets with different initial unemployment conditions, this paper provides evidence on the cyclical behavior of the micro-elasticity.

2.2 Quantile Treatment Effects

There are two reasons why it is important to go beyond the weighted ATE and investigate quantile treatment effects. First, estimation of the ATE may be biased because of censoring of unemployment spells at the UI exhaustion point. In practice, around 10 to 15% of spells are censored. But this percentage might evolve endogenously with labor market conditions, spuriously affecting the cyclical behavior of the estimates. Second, heterogenous effects of UI benefits across unemployment spells are of particular importance for the optimal design of UI benefit schedules. There is an important literature on the spike of the hazard rate at benefit exhaustion (see Meyer [1990] and Card et al. [2007]) which relates to non homogenous effects of the potential duration of UI across the distribution of unemployment durations. But there is little emphasis in the literature on the fact that UI benefit levels may have very different effects on short term and long term unemployed as well. When it comes to estimating the effect of benefit levels, most studies rely on semi-parametric proportional hazard models and therefore impose the effect to be a constant location shift of the hazard rate at all point in time.

The advantage of the RKD setting is that it can easily be extended to the estimation of quantile treatment effects. Let $Q_\tau(Y|W=w) = F^{-1}(\tau|W=w)$ define the τ-th conditional quantile of the outcome given the assignment variable. I am interested in the Quantile Treatment Effect (QTE) of a continuous regressor b, the UI benefit level[6]:

$$\alpha_\tau = \frac{\partial Q_\tau(Y|W=w)}{\partial b}$$

[6]The same logic applies to QTE of potential duration D.

Under the assumption that $\frac{\partial Q_\tau(Y|W=w)}{\partial w}|_{b=b(w)}$ is smooth, the logic of the RK design can be extended to identification of the QTE and we have:

$$\alpha_\tau = \frac{\lim_{w \to k_1^+} \frac{\partial Q_\tau(Y|W=w)}{\partial w} - \lim_{w \to k_1^-} \frac{\partial Q_\tau(Y|W=w)}{\partial w}}{\lim_{w \to k_1^+} \frac{\partial b(w)}{\partial w} - \lim_{w \to k_1^-} \frac{\partial b(w)}{\partial w}}$$

Estimation of α_τ is also straightforward. The denominator of the estimand is deterministic, and is the change in the slope of the schedule at the kink. The numerator is the change in the slope of the τ-th conditional quantile of the outcome given the assignment variable at the kink. It can be simply estimated by running Quantile Regression Models of the following form:

$$Q_\tau[Y|W=w] = \mu_0 + [\sum_{p=1}^{\bar{p}} \gamma_{\tau p}(w-k)^p + \nu_{\tau p}(w-k)^p \cdot D] \quad \text{where } |w-k| \leq h$$

and the numerator of the RK estimand of the τ-th QTE is given by $\nu_{\tau 1}$.

3 Data and Institutional Background

3.1 Data

The data used is from Continuous Wage and Benefit History (CWBH) UI records[7]. CWBH data contains the exhaustive of all unemployment spells and wage records for five US states between 1978 and 1984[8]: Idaho, Louisiana, Missouri, New Mexico and Washington[9]. Three advantages of the data are worth noting[10]. First, CWBH data provides accurate information on the level of benefits, potential duration[11], previous earnings and work history over time. Given the large degree of measurement error found in survey data, administrative data like the CWBH are the only reli-

[7] I am especially grateful to Bruce Meyer and Patricia M. Anderson for letting me access the CWBH data.
[8] Records for Idaho begin in 1976.
[9] The CWBH also contains a small sample of records for Pennsylvania that we were not able to exploit.
[10] For further details on the CWBH dataset, see for instance Moffitt [1985a]
[11] For first tier only. For other tiers, imputation based on legilsation.

able source if one wants to implement identification strategies such as the regression kink design used in this paper. Administrative data was also supplemented by a questionnaire given to new claimants in most states participating to the CWBH project and which gives additional information on socio-demographic characteristics of the claimants such as ethnicity, education, spouse's and dependents' incomes, capital income of the household, etc[12]. Second, since identification in the regression kink design relies on estimating changes in the slope of the relationship between an assignment variable and some duration outcomes of interest, the granularity of the CWBH data, which contains the exhaustive of unemployment spells, is a key advantage and smaller samples of UI recipients would in general not exhibit enough statistical power to detect any effect in a RK design. Finally, labor market conditions exhibit a lot of variation for the states and years for which CWBH data is available. Figure 1 displays the evolution of monthly unemployment rates computed from the Current Population Survey in the five states for the time period available in the CWBH dataset. The data spans a period of low unemployment (1976 to 1979) followed by two recessions (January to July 1980 and July 1981 to November 1982). Following the 1981-1982 recession, the US unemployment rate surged above 10% and reached higher levels than during the Great Recession. In this respect, the CWBH data offers an interesting comparison with the current situation of the US labor market.

Unemployment Insurance claims are observed at weekly frequencies in the administrative data so that all duration outcomes are measured and expressed in weeks. I focus on three duration outcomes. The duration of paid unemployment, which corresponds to the number of weeks a claimant receives unemployment compensation for a given spell. The duration of claimed unemployment corresponds to the number of weeks a claimant is observed in the administrative data for a given unemployment spell. This duration differs from the duration of paid unemployment. First, because most states have instated waiting periods, which means that after a claim has been filed, there is a minimum period during which the claimant cannot receive any UI compensation. Second, because a lot of spells exhibit interruptions in payment with the claimant not collecting any check for a

[12]Some of these questionnaire information are unfortunately not available for certain years depending on the state.

certain number of weeks without being observed in the wage records. Our third duration outcome of interest is the duration of the initial spell as defined in Spiegelman et al. [1992] The initial spell starts at the date the claim is filed and ends when there is a gap of at least two weeks in the receipt of UI benefits.

The main disadvantage of the data is that an observation is censored once a claimant has exhausted his benefits. Behavior beyond the exhaustion point cannot be analyzed. In practice, around 10 to 15% of spells are censored (cf. table 1). Estimation of quantile treatment effects in section 4.2.3 deals with the inconsistency of average treatment effects in the presence of censoring.

3.2 Institutional Background

The identification strategy relies on discontinuities in the schedule of UI benefits in US states. In this section, I describe the main features of the states UI legislation. In almost all US states, UI benefits depends on the labor market activity of the claimant in the period before becoming unemployed. This period is usually defined as the base period, and is traditionally the last four completed calendar quarters immediately preceding the start of the claim. The weekly benefit amount b received by a compensated unemployed is a fixed fraction τ_1 of his highest-earning quarter (hqw) in the base period[13] up to a maximum benefit amount b_{max}:

$$b = \begin{cases} \tau_1 \cdot hqw & \\ b_{max} & \text{if} \quad \tau_1 \cdot hqw > b_{max} \end{cases}$$

Figure 2 plots the evolution of the weekly benefit amount schedule in Louisiana for the time period available in the CWBH data. The schedule applies based on the date the UI claim was filed, so that a change in the maximum weekly benefit amount does not affect the weekly benefit amount of ongoing spells. In Louisiana, τ_1 is equal to $1/25$ which guarantees a constant replacement ratio

[13] Some states, such as Washington, use the average of the two highest-earning quarters in the base period. For details about states' legislation and sources, see appendix.

of 52% of the highest-earning quarter up to the kink, where the replacement ratio decreases. The number of weeks a claimant can collect UI benefits is determined by two rules. First, there is a maximum duration D_{max} that cannot be exceeded, usually 26 weeks. But the total amount of benefits that a claimant is able to collect for a given benefit year is also subject to a ceiling, which is usually determined as a fraction τ_2 of total earnings in the base period bpw. So the total amount of benefits collected is defined as:

$$B = min(D_{max} \cdot b, \tau_2 \cdot bpw)$$

This ceiling in the total amount of benefits determines the duration of benefits, since duration $D = \frac{B}{b}$ is simply the total amount of benefits divided by the weekly benefit amount. Duration of benefits can therefore be summarized as[14]:

$$D = \begin{cases} D_{max} & \\ \tau_2 \cdot \frac{bpw}{min(\tau_1 . hqw, b_{max})} & \text{if} \quad \tau_2 \cdot \frac{bpw}{min(\tau_1 \cdot hqw, b_{max})} \leq D_{max} \end{cases}$$

Duration is thus also a deterministic kinked function of previous earnings. To analyze independently the effects of duration and of the benefit amount in the regression kink design, it is useful to break down the sample in different subgroups. Figure A1 in the appendix summarizes the kinked schedules of the weekly amount and potential duration of UI benefits for Louisiana for all the different subgroups.

First, for claimants who hit the maximum weekly benefit amount, $b = b_{max}$, there is a kink in the relationship between potential duration and base period earnings bpw at $bpw = D_{max} \cdot \frac{b_{max}}{\tau_2}$

$$D = \begin{cases} D_{max} & \\ \tau_2 \cdot \frac{bpw}{b_{max}} & \text{if} \quad bpw \leq D_{max} \cdot \frac{b_{max}}{\tau_2} \end{cases}$$

[14] Idaho is the only state in the CWBH data with different rules for the determination of benefit duration. In Idaho, as explained in the appendix, there is no ceiling on the total benefit amount for a given benefit year, but the potential duration is a step function of the ratio bpw/hqw of the base period earnings to the highest quarter earnings.

The schedules of b and D for this subgroup is displayed on the left of panel B in figure A1. For claimants who are below the maximum weekly benefit amount, $b < b_{max}$, (right of panel B in figure A1) there is a kink in the relationship between potential duration and the ratio of base period earnings to the highest-earning quarter at $\frac{bpw}{hqw} = D_{max} \cdot \frac{\tau_1}{\tau_2}$

$$D = \begin{cases} D_{max} & \\ \frac{\tau_2}{\tau_1} \cdot \frac{bpw}{hqw} & \text{if } \frac{bpw}{hqw} \leq D_{max} \cdot \frac{\tau_1}{\tau_2} \end{cases}$$

Finally, if $\frac{bpw}{min(hqw, \frac{b_{max}}{\tau_1})} \leq D_{max} \cdot \frac{\tau_1}{\tau_2}$, then:

$$D = \tau_2 \cdot \frac{bpw}{min(\tau_1.hqw, b_{max})}$$

For these claimants, whose schedules are displayed on the left of panel A in figure A1, potential duration is always inferior to the maximum duration D_{max} but the relationship between duration and highest quarter earnings hqw exhibits an upward kink at $hqw = \frac{b_{max}}{\tau_1}$, which is also the point where the relationship between the weekly benefit amount b and hqw is kinked. When estimating the independent effect of b on unemployment duration in the regression kink design, I drop these observations and focus only on individuals with maximum potential duration ($D = D_{max}$) to avoid having two endogenous regressors kinked at the same point. The schedules for this subgroup is shown on the right of panel A in figure A1.

The rules for the determination of benefit duration discussed above constitute the basis of the UI benefit system (Tier I) that applies in each state. During recessions, and depending on state labor market conditions, two additional programs superimpose on Tier I to extend the duration that UI benefits are available. The first program is the permanent standby Extended Benefit program, federally mandated but administered at the state level (Tier II). This program provides with an additional duration of 50% of regular state duration up to the state maximum duration when the state unemployment rate reaches a certain trigger. When the EB program is in action, the slope of the relationship between previous earnings and benefit duration is steeper but the location of the kink is identical as shown for instance in figure 3. [15]

On top of the EB program, federal extensions are usually enacted during recessions (Tier III). During our period of analysis, the Federal Supplemental Compensation (FSC) program was in action from September 1982 to March 1985. The FSC program had four different phases with additional duration of 50% to 65% of state regular duration with maximum depending on state labor market conditions[16]. The FSC introduced additional kinks in the relationship between previous earnings and benefit duration as shown in figure 3 in the case of Louisinia[17]. Most importantly, benefit extensions create non-stationarity in the potential duration of benefits over the duration of a spell, which creates challenges for inference in the RK design, as I discuss in section 5.

4 The Effect of Benefit Level

I present in this section results of the analysis of the effect of benefit level in the RK design using the kink in the relationship between benefit level and highest quarter of earnings described in section 3.2. In the analysis, I divide for each state all the unemployment spells in subperiods based on labor market conditions, in order to get estimates of the effect of benefit level on unemployment duration in high and low unemployment regimes [18]

[15] Some specificities of EB program changed in 1981. Before 1981, two triggers existed: a national trigger, and a state trigger. In the Omnibus Budget Reconciliation Act of 1981, Congress voted to eliminate the national trigger entirely (effective July 1, 1981) and to permit the states to establish an optional trigger when the unemployment rate reaches 6 percent, rather than 5 percent. The mandatory trigger rate was also raised.

[16] For details on the FSC, see appendix and Corson et al. [1986]

[17] The figure is a simplified summary of the many different schedules that applied in Louisiana between 1979 and 1983. Within each phase of the FSC for instance, maximum durations changed several times based on state labor market conditions. See table III.1 in Corson et al. [1986] for complete details.

[18] Unemployment spells are divided according to the date the UI claim was filed, and the exact dates of the sub periods are the following: Idaho: 17jan1976 to 01jul1978, 01jul1978 to 01jul1981, 01jul1981 to 31dec1983. Louisiana: 01jan1979 to 06sep1981, 06sep1981 to 31dec1983. Misssouri: 01jan1978 to 01dec1979, 01dec1979 to 01jan1982, 01jan1982 to 31dec1983. New Mexico: 01jan1980 to 01jan1982, 01jan1982 to 31dec1983. Washington: 25jun1979 to 01jul1981, 01jul1981 to 31dec1983.

There are two main considerations behind the choice of subperiods. First, grouping unemployment spells over a larger period of time has the advantage of providing with a larger number of observations at the kink for statistical power. The pooled analysis will therefore yield more efficient estimates. But, this efficiency gain comes at a cost, because of the pooling of observations from different schedules when the maximum benefit amount changes frequently over time. For each unemployment spell, I center the highest quarter of earnings at the kink point in the schedule that is applicable given the date the claim was filed. If the maximum benefit amount increases from b_{max1} to b_{max2}, then the change in slope at the kink remains unaffected but the level of benefit at the kink is higher and the pooled estimate represent an average of the marginal effects at b_{max1} and b_{max2}. Another potential issue of choosing longer subperiods is the presence of high inflation rates from 1978 to 1982. One potential solution would be to express the nominal schedules in real terms. If p_t is the monthly price index, then, $b_r = min(\tau_1 \cdot hqw/p_t, b_{max}/p_t)$. The disadvantage of this technique is to create as many schedules as the number of months: even if the change in the derivatives of the benefit levels expressed in real term at the kink remains unchanged, the benefit level in real term at the kink changes every month, which adds a lot of noise in the analysis. I chose to keep schedules expressed in nominal terms but I display additional results for shorter sub periods to check for the sensitivity of the results to this issue of high inflation rates.

For the sake of brevity, I do not display the results of specifications and sensitivity analysis for all states and sub periods, but I rather focus on the case study of Louisiana. All the results for the other states can be found in the appendix.

4.1 Graphical Evidence

I begin by showing graphical evidence in support of the RKD assumptions. First, I plot densities of the assignment variable in order to detect potential manipulation of the assignment variable at the kink point. Figure 4 shows the number of spells observed in each bin of $250 of highest quarter of earnings[19] centered at the kink point in Louisiana for two subperiods. The first period from january 1979 to september 1981 is a period of relatively low unemployment in Louisiana (monthly unemployment rate of 7.0% on average). The second period from september 1981 to december 1983 is a period of very high unemployment in Louisiana with a monthly unemployment rate of 10.8% on average. The two histograms show no signs of discontinuity in the relationship between the number of spells and the assignment variable at the kink point. To confirm this graphical diagnosis, I also performed McCrary test as is standard in the Regression Discontinuity Design literature, and did not detect a lack of continuity at the kink for both periods. This test is of course only a partial one because, first, as explained above, the assumption of continuity of the ex ante individual density is fundamentally untestable, and second, because it does not provide evidence on the continuity of the derivative of the conditional density at the kink. But the spirit of the McCrary test can be simply extended to test for violation in the continuity of the derivative, as done in Card et al. [2009]. The idea is to regress the number of observations N_i in each bin of $250 on polynomials of the average highest quarter of earnings in each bin (centered at the kink) $(w-k)$ and the interaction term $(w-k)\cdot 1_{[W\geq k]}$. The coefficient on the interaction term is insignificant which supports the idea of a continuous derivative of the conditional density at the kink.

A key testable implication of a valid RK design is that the conditional expectation of any covariate should be twice continuously differentiable at the kink. This can be visually tested by plotting the mean values of covariates in each bin of $250 of the assignment variable as done in figure 5 for the first sub period in Louisiana. The four panels of figure 5 all suggest that the covariates evolve smoothly at the kink, in support of the identification assumptions of the RK design. Formal tests can also be performed by running polynomial regressions of the form described in equation 1. Results are described in the next subsection.

The pattern for the outcome variables offers a striking contrast with that of covariates, as shown

[19] The choice of the bin size in our graphical analysis is done using the formal test of excess smoothing recommended by Lee and Lemieux [2010] in the RD setting. A bin size of $250 is the largest that passes the test for all states and outcomes of interest.

in figures 6, 7 and 8 which display the evolution of the mean values of the outcome variable of interest (duration UI paid, duration UI claimed and duration of initial spell) in each bin against the assignment variable centered at the kink, for all five states, for the first sub period in each state. There is a visible change in the slope of the relationship between the three duration outcomes of interest and the assignment variable at the kink point of the benefit schedule for all five states. This provides supportive evidence for the identification of an effect of benefit level on unemployment duration in the RK design.

Figure 10 replicates the principle of figure 8 but, instead of plotting the mean value, I plot different percentiles of the distribution of the duration of initial spell against the assignment variable. Interestingly, no kink is visible for low percentiles of the distribution, where the duration of spells is very short (around 1 to 2 weeks), but a kink appears for higher percentiles of the distribution and seems more pronounced the higher one gets in the distribution until reaching the censoring point. For the 90th percentile, for instance, all spells hit the maximum UI duration, and we therefore cannot detect any kink. This evidence is suggestive of the existence of important distributional effects that motivate the estimation of quantile treatment effects, which also offers the advantage of circumventing the issue of biased average treatment effects because of the censoring of spells at the maximum UI duration.

4.2 Estimation Results

4.2.1 Average Treatment Effects

Table 2 shows the results for the baseline specification of equation 1 in the linear case for Louisiana and for the two sub periods 1979 to 1981 and 1981 to 1983. In each column, I report the estimate of the weighted average treatment effect $\widehat{\alpha} = -\frac{\widehat{\nu_1}}{\tau_1}$, with robust standard errors. Each estimate is done using nominal schedules, but the $\widehat{\alpha}$ are rescaled to 2010 dollars and they should be interpreted as the marginal effect of an extra dollar of 2010 in weekly benefit amount on the average duration (in weeks) of the outcome. I also report the elasticity with respect to the benefit level ($\varepsilon_b = \widehat{\alpha} \cdot \frac{b_{max}}{\overline{Y_1}}$, where $\overline{Y_1}$ is mean duration at the kink point) and its robust standard error, as well as the p-values from a Goodness-of-Fit test that consists in comparing the polynomial model to the same polynomial model plus a series of bin dummies. The results are consistent across the three duration outcomes of interest, with an estimated elasticity of between .36 and .41 for the first period and .51 and .6 for the second period. In each case, the linear specification is not considered too restrictive compared to the model including bin dummies as suggested by the large p-values of the Goodness-of-Fit test. For covariates, to the contrary, I cannot detect evidence of a significant change in the slope of the conditional expectation at the kink for any of the two periods.

In table 3, I analyze the sensitivity of the results to the choice of the polynomial order. I display the same results as in Table 2 for a linear, a quadratic, and a cubic specification. I also report the Aikake Information Criterion (AIC) for all specifications. The estimates for α are quite similar across the different specifications. Standard errors of the estimates nevertheless increase quite substantially with higher order for the polynomial. The AIC suggest that the quadratic specification is always dominated but the linear and the cubic specification are almost equivalent, and none of them is too restrictive based on the p-values of the Goodness-of-Fit test.

Table 4 explores the sensitivity of the results to the choice of the bandwidth level. Results are consistent across bandwidth sizes, but the larger the bandwidth size, the less likely is the linear specification to dominate higher order polynomials. An additional concern is that using relatively long sub periods with nominal schedules may lead to an attenuation bias because inflation causes an extra nominal dollar in weekly benefit amount to be worth less at the end of the sub period than at its inception. To examine the robustness of the results to this concern, I present results using a larger number of sub-periods in table 5. Estimates of α are clearly in line with the baseline, ranging between .025 and .055. The drawback of using shorter sub-periods is that the relationship between the assignment variable and the outcome becomes noisier. Even though the change in the slope is quite precisely estimated in each of the five sub-periods, the p-values of the Goodness-of-Fit test are smaller, indicating that the polynomial specifications have more difficulty fitting the data.

4.2.2 Cyclical Behavior

To examine the cyclical behavior of the elasticity, I replicate the estimation procedure for all states and periods, and then correlate the estimates with the average monthly unemployment rate from the Current Population Survey prevailing in the state for each subperiod. Results are displayed in figure 9. The estimated elasticities for all three outcomes are consistently around .4 for all states. The elasticity of the duration of paid unemployment exhibits a slightly negative correlation with the unemployment rate but the elasticity of the duration of initial spell does not. I investigate more formally the cyclicality of the estimates in table 6, where I regress the estimated effects of the benefit level on the average monthly unemployment rate U. The coefficient in column (1) is very small and not significantly different from zero (-.01 with a standard error of .035), which means that a 1 percentage point increase in the unemployment rate is associated with a .01 percentage point decrease in the estimated elasticity. To control for the different precisions of the estimates, I weight in column (2) the observations by the inverse of the standard error of the estimated elasticity. The coefficient is still small and not significantly different from zero, but the sign becomes positive. The acylicality of the elasticity is not affected by the inclusion of state fixed effects as shown in column (3).

One may worry that the cyclical behavior of the micro-elasticity is very sensitive to the choice of the benefit amount as a baseline. I therefore also computed elasticities with respect to the replacement rate $\varepsilon_{\Delta C}$ instead of the benefit amount. Estimates are consistently around .3 with an average of .267. Such estimates are on the lower end of the spectrum when compared to traditional benchmarks in the literature. Nevertheless, they compare well with other results obtained on the same data using different identification strategies. In particular, a widely cited benchmark is the elasticity of .56 found by Meyer [1990] on a smaller sample of CWBH records. Landais et al. [2010] actually show that on the same exhaustive CWBH data as the one used here, Meyer's estimates can be fully replicated using his specification, but that if one adds a richer set of non parametric controls for previous wages[20], the elasticity goes down to around .3. In column (4) of table 6, I correlate the elasticities with respect to the replacement rate with the unemployment rate, I also find that the estimated elasticities are acyclical: the coefficient for the unemployment rate is .001 with a standard error of .03. I also use in column (5) the estimated average treatment effects α instead of the estimated elasticities and do not find any statistically significant correlation with the unemployment rate.

The average state unemployment rate may be an imperfect proxy for labor market conditions. Unfortunately, statistics on job openings by state are not available in the US for this time period and it is therefore impossible to compute measures of state labor market tightness. Instead, I use the average growth rate of state unemployment and correlate it with the estimates in column (6), again finding no statistically significant correlation. Overall these results strongly suggest that the average effect of UI benefit level on unemployment duration is acylical.

Concern might arise that the lack of correlation is driven by changing characteristics at the kink over the cycle or across states. To examine this potentiality, I also correlate the estimates of the elasticity with average characteristics at the kink. Figure A2 in appendix shows that there is little variation within state over time of observable characteristics at the kink. Most variation in observable characteristics at the kink comes therefore from differences across states. In column (7) of table 6 I regress the estimated elasticities on all observable characteristics at the kink and the unemployment rate. Interestingly, correlations suggest that the elasticity decreases with age, education and the number of dependents in the household. A higher number of dependents, like the presence of a working spouse, increases the possibility of self insurance of the household. Higher levels of education are correlated with higher earnings and savings levels, which may also explain

[20] Meyer [1990] only controls for previous wages using the log of the base period earnings. In practice, one can add a richer set of non parametric controls to mitigate the concern of endogeneity of UI benefit variation and still get sufficient variation in the benefit level to estimate the elasticity. However, the endogeneity issue is not fully resolved and only the RK design addresses properly this concern.

a lower behavioral response to social insurance[21].

Finally, one may be concerned that the cyclical behavior of the ATE estimates is biased because of the cyclicality of the censoring of unemployment spells at the UI exhaustion point. During recessions, the duration of unemployment spells tend to increase, pushing more people beyond the exhaustion point. At the same time, extensions of UI benefit duration in recession works in the opposite direction, so that overall, the percentage of spells censored is relatively stable over the business cycle and is unlikely to drive the results. In any case, estimation of quantile treatment effects further addresses this concern of censoring.

4.2.3 Quantile Treatment Effects

As suggested by figure 10, the estimation of quantile treatment effects reveals the existence of a significant level of heterogeneity in the response to UI benefit level across the distribution of spells. I estimate quantile treatment effects for the 10th to 70th conditional percentile of the distribution of initial spell because all these percentiles are below the censoring point in the bandwidth of interest. In figure 11, I report the estimates with their bootstrapped standard errors for the two sub periods in Louisiana, and the red line indicates the level of the baseline average treatment effect estimated above. The effect is statistically insignificant for low percentiles of the distribution but increases for higher percentiles of the distribution in both periods. Interestingly, the profile of the quantile treatment effects seems flatter in period 1981-1983, when the unemployment rate is higher, than in period 1979-1981 for which the profile appears relatively steep.

I replicate these estimates for all states and periods and correlate the estimates with the unemployment rate. Graphical evidence is provided in figure 13 in appendix. Column (8) to (10) of table 6 present the results. Column (8) shows that the quantile treatment effects increase with the

[21] Note that the heterogeneity of the effects of UI by education, age, or any other covariates can easily be investigated in the RK design by replicating the results for different sub-populations of interest.

percentile τ of the distribution at which they are estimated. For the first 20% of the distribution of spells, the effect is even always equal to zero. In column (9), I interact τ with the average state monthly unemployment rate, and the coefficient is negative and significant, which confirms the evidence of figure 11 that the heterogeneity in the effect of benefit level is less pronounced when the conditions in the labor market deteriorate. I found similar pattern when looking at the elasticities computed from the quantile treatment effects (column (10)), or when looking at the duration of UI claims or the duration of paid UI.

From a search theoretical standpoint, it can be more straightforward to analyze the effect of benefit level on the hazard rate out of unemployment. Following a logic similar to that of the estimation of quantile treatment effect, I also investigate distributional effects by estimating the effect of benefit level at every point of support of the hazard function below the maximum duration of Tier I. I run linear probability models of the following form:

$$Pr[Y=t|Y \geq t, W=w] = \mu_{t,0} + [\sum_{p=1}^{\bar{p}} \gamma_{t,p}(w-k)^p + \nu_{t,p}(w-k)^p \cdot D] \quad \text{where } |w-k| \leq h$$

where $\nu_{t,1}$ gives once again the numerator of the RK estimand for the effect of benefit level on the hazard rate at week t. Results are displayed in figure 12 and suggest that the effect of benefit level is not significant at the beginning of unemployment spells, and then increases with the duration of one's spell. I replicate these estimates for all states and sub-periods and display in column (11) of table 6 correlations of the estimates with the unemployment rate. The estimates are noisier than for QTE, but overall, I also find that the estimates of the effect of benefit level on the hazard rate are increasing with t, the number of weeks of unemployment at which the effect is estimated. I also find that the coefficient on the interaction between t and the unemployment rate U is negative, suggesting that the increasing pattern of the effect of benefit level is flatter when labor market conditions deteriorate.

Overall, this provide evidence of the existence of distributional effects of UI and two main results seem to emerge. First, an important fraction of short spells seems clearly unaffected by the generosity of the UI system, whereas longer spells are more responsive to it. And second, these distributional effects seem to flatten when labor market conditions deteriorate, with the effect of UI becoming more homogenous across the distribution of spells. The acyclicality of the average treatment effects seems to mask the presence of slightly cyclical distributional effects.

5 The Effect of Benefit Duration

5.1 The Issue of Benefit Extensions

The existence of a deterministic kinked relationship between the potential duration of UI and previous earnings offers the possibility to identify non parametrically the effect of potential duration on unemployment duration in the regression kink design. However, the presence of extremely frequent changes in the schedule of potential duration complicates the estimation. In Louisiana for instance the schedule changed 11 times between January 1979 and December 1983. When the sample begins with spells starting as of 01/14/1979, only Tier I is in effect. Then the national EB trigger goes on from 7/20/1980 to 1/24/1981, starting a period of EB in Louisiana. From 01/24/1981 to 09/12/1981, only Tier I is in effect again, but the state EB trigger goes on after 09/12/1981, which starts another period of EB. Before this new period of EB is over, FSC-I comes in effect starting 09/12/1982, and therefore FSC-I and EB apply. After 10/20/1982, the state trigger on EB goes off and only FSC-I remains in action. On 01/09/1983, FSC-II begins, and on 01/23/1983, the state EB trigger goes on again. On 03/20/1983 the maximum duration of the FSC-II program in Louisiana is increased to 16 weeks. On 03/31/1983 the FSC-III program comes into effect, and at the same date, the maximum duration for the Tier I program (standard state UI) is reduced from 28 to 26 weeks. On 06/19/1983 the maximum duration of the FSC-III extension goes down to 12 weeks. On 10/19/1983 the FSC-IV extension program begins, but its rules and its maximum duration in Louisiana are the same as for the FSC-III.

These frequent changes in the schedule of potential duration are a concern for identification because a fundamental requirement of the RK design is the stationarity of the schedule during the whole duration of a spell. Only observations for which the schedule did not change from the beginning of the spell to the end of the potential duration can be kept in the estimation sample. In Louisiana for instance, when I restrict the sample to spells with a stationary schedule throughout the whole potential duration of the spell, I am left with only 3 sub periods: the first sub period contains all spells beginning between 01/14/1979 and 01/31/1980, the second contains all spells beginning between 09/12/1981 and 05/01/1982, and finally the third sub period contains all spells beginning after 06/19/1983 to 31/12/1983. The small sample size issue of having to split the data in stationary sub periods is reinforced by the necessity to break down observations according to their weekly benefit amounts, since individuals with $b = b_{max}$ and $b \leq b_{max}$ face different schedules[22]. Given state UI parameters, sample size at the kink can become to small for inference. Because of these constraints, the number of estimates for the effect of potential duration is much more limited than for the effect of benefit level. Overall, only Louisiana, Missouri and New Mexico have valid RK designs with a sufficient number of observations at the kink for the estimation of the effect of potential duration.

Figure 14 shows the density at the kink in Louisiana for the three sub periods with stable potential duration schedule. In all three sub periods, the number of observations in the estimation sample around the kink is four to five times smaller than for the estimation of the effect of benefit level. The histograms show no signs of discontinuity in the relationship between the number of spells and the assignment variable at the kink point. I also test the RK design assumption of a twice differentiable conditional density at the kink more formally, as in section 4. The second key assumption for the validity of the RK design, namely that the conditional expectation of any covariate should be twice continuously differentiable at the kink, seems also confirmed by graphical evidence. Figure 15 plots the mean values of covariates in each bin of the assignment variable for

[22]Note also that for individuals hitting the maximum weekly benefit amount $b = b_{max}$, the location of the kink changes every time the statutory maximum weekly benefit amount is increased which further reduces the estimation sample size of observations with stationary schedule.

one particular sub-period and all four panels suggest that the covariates evolve smoothly at the kink [23]. This graphical diagnosis is also confirmed for each sub-period by formal tests for the existence of a kink in the relationship between covariates and the assignment variable in table 7.

The strategy followed in figure 15 can be once again replicated for the outcome variables of interest. Figure 16 plots the mean values of the duration of initial spell in each bin of the assignment variable for the 3 sub-periods of analysis in Louisiana. In contrast with figure 15, figure 16 shows clear signs of a kink in the relationship between the assignment variable and the duration of initial spell at the kink. But the smaller sample size at the kink makes the relationship between the outcome and the assignment variable a little noisier visually than in the case of the kink in the benefit level schedule depicted in figures 6 to 8.

5.2 Estimation Results

Estimation of the ATE of potential duration in the RK design is similar to that of benefit level, and relies on the estimation of the numerator of the RK estimand with polynomial regressions of the form described in equation 1. Table 7 presents the results for the average treatment effect $\widehat{\beta}$ with robust standard errors for Louisiana. For each of the three sub periods with stable schedules[24], I report the estimates of the preferred polynomial specification based on the Aikake Information Criterion. The effect of an additional week of UI on average duration is consistently around .2 to .5 and statistically significant for all duration outcomes and sub-periods of interest. The linear specification is always preferred and is never rejected by the Goodness-of-Fit test as indicated by the reported p-values. For covariates in columns (4) to (8), to the contrary, the same estimation procedure does not reveal any kink in the relationship with the assignment variable, which supports the validity of the RK design.

[23]Note that because of a smaller sample size, a smaller bin size is recommended to avoid excess smoothing when using formal tests for the choice of the optimal bin size.

[24]For the third sub period, the 12 weeks maximum duration of FSC-III and FSC-IV introduces a small second kink in the schedule, visible in figure 3, but due to a lack statistical power to detect its effect, I focus on estimation of the effect of the larger kink.

The estimates of an increase of .2 to .3 weeks of unemployment with each additional week of UI are in line with previous estimates in the US such as Moffitt [1985b], Card and Levine [2000], and Katz and Meyer [1990]. They are also slightly higher than existing estimates in Europe using RD designs such as Schmieder et al. [2011] for Germany. This could be due to much longer baseline durations in European UI systems. In Schmieder et al. [2011] for instance, baseline potential durations, at which the effect of an extension of UI are estimated, are between 12 to 24 months, which is 2 to 4 times longer than in the US.

But an important concern is that censoring of unemployment spells may bias upward the estimates of the ATE. This censoring issue is more acute for the effect of potential duration than in the case of benefit level: even in the absence of any behavioral response, an increase in potential duration amounts to shifting the truncation point of the distribution of spells to the right, and therefore affects the mean of the truncated distribution even if the mean of the uncensored distribution stays constant. To address this concern, I also estimate quantile treatment effects following the same procedure as in the case of benefit level. Results show that there is considerable heterogeneity in the effect of potential duration across the distribution of unemployment spells, as exemplified by figure 17, which plots the estimates of the quantile treatment effects of potential duration on the duration of initial spell in Louisiana for the three sub-periods. For spells below the median of unemployment duration and for the first two sub-periods, an additional week of covered UI has no effect on the duration of initial spell, but as one moves closer to the benefit exhaustion point, the effect becomes strongly positive and significant. I find the same evidence of heterogeneity when estimating the effect pointwise at each point of support of the hazard function, as done in section 4.2.3 for the effect of benefit level. This evidence is in line with earlier findings that exit rates from insured unemployment spike near the exhaustion point. But interestingly, this pattern seem to be less pronounced in the third sub-period when labor market conditions are very bad. The effect of an additional week of benefit is then positive and significant on much lower percentiles of the distribution of spells.

To confirm this evidence, I replicate the estimates for the states and sub-periods for which there are enough observations at the kink for inference. As explained above, because of benefit extensions, we are left with only eight different states and sub-periods. These estimates also span a narrower range of labor market conditions, which gives us less power to investigate the cyclical behavior of the effect of potential UI duration than for the effect of benefit level. The average monthly state unemployment rate among the states and sub-periods available for the RKD estimation of potential duration ranges from 4.7 to 9.0% with a mean of 7.3 and a median of 7.4. Overall, estimates are in line with that of figure 17. The quantile treatment effects of an additional week of potential duration β_τ range from -.15 to 1.17 with a mean value of .20. In table 8, I present results concerning the behavior of the QTE estimates. Column (1) to (3) show that the effects of an additional week of UI are larger for higher quantiles of the distribution of unemployment spells. In particular, column (3) shows that the QTE are not statistically different from zero for the lower 50 percent of the distribution of unemployment spells. In column (4) to (6), I investigate whether this heterogeneity varies with labor market conditions. I interact the quantile index with the average monthly state unemployment rate. Because of the narrow span of unemployment conditions, I simply use a dummy variable equal to 1 if the the state average monthly unemployment rate is superior to 7.5% to proxy for tight vs slack labor markets. Column (4) shows that the profile of QTE is somewhat flatter when the unemployment rate is high. In column (6) I use the average monthly growth rate of unemployment in each state[25], and again use a simple indicator for this rate being above .8% to proxy for labor market conditions. I do not detect here any difference in the profile of QTE.

Overall, these results suggest that there is heterogeneity in the effect of potential duration, but this effect seems driven by responses close to the exhaustion point, as already pointed out in the literature focusing on the spike of exit rates at exhaustion. Some evidence suggests that this heterogeneity might be less pronounced in recessions, but, given my identification strategy and the

[25]The average monthly growth rate of unemployment among the states and sub-periods available for the RKD estimation of potential duration ranges from -.72 to 1.9% with a mean of .78% and a median of .80%.

time span of the data, I have less power to investigate the cyclical behavior of the estimates.

Discussion

How are the estimates presented here informative about the effect of UI extensions in the Great Recession? UI institutions have little changed since the late 1970s: replacement rates and baseline durations of state UI programs are more or less the same. Nevertheless, institutional settings were different, and in particular, the relative generosity of safety nets for the long term out-of-work has declined significantly in the 1990s with a complete overhaul of welfare programs. A consequence is that elasticities are potentially slightly greater today because the continuation value of unemployment past the UI exhaustion point is lower than in the CWBH data. Rothstein [2011], however, finds very small effects of UI extensions during the Great Recession, but his identification strategies might be picking up a macro-elasticity estimate, which might be lower during recessions in the presence of negative job search externalities.

The acyclicality of the average (partial equilibrium) effect of UI on unemployment duration has important welfare consequences. Following the framework of Landais et al. [2010], it suggests that the average welfare cost of UI is constant over the cycle in partial equilibrium or in equilibrium models with infinitely elastic labor demand (such as Pissarides [2000] with Nash bargaining or Shimer [2004] and Hall [2005] with wage rigidity). Increasing the generosity of UI in recessions would then be sub-optimal, unless substantial spillover effects of UI create a wedge between the micro- and the macro-elasticity of UI. Investigating the presence of such job search externalities and their variation over the business cycle should therefore be an important part of the research agenda on the optimality of state contingent UI. In a companion paper, I use the Regional Extended Benefit Program (REBP) in Austria, which dramatically increased the duration of benefits from 30 to 209 weeks for workers aged above 50 in some regions of Austria during 1988–1993[26]

[26] Lalive [2008] shows that this program led to a large decrease in job-search effort for treated workers.

to evaluate whether comparable untreated workers in treated regions experience a reduction in unemployment duration.

A contribution of this paper is to show that the acyclicality of the average effect of UI masks the presence of heterogenous effects between short term and long term unemployed. A substantial literature already discusses the possible origins of such heterogeneity in the case of the effect of potential duration. In a standard search framework, effort should be increasing steadily with unemployment duration until the point of exhaustion (see Mortensen [1977] or Katz and Meyer [1990] for instance). This fails to explain, first why short spells are totally unresponsive to an increase in potential duration, and second, why we see such a spike in exit rates at the exhaustion point. The unresponsiveness of short spells could be due to salience issues. The actual potential duration that an unemployed worker is eligible to is a complex function of her previous earnings. Most unemployed might not realize their actual potential duration until the very end of their spell. The spike at exhaustion being only visible in administrative unemployment records and not in non-employment duration, this also points out to the existence of hidden information on job offers and acceptances as suggested in Card et al. [2007].

Heterogeneity in the effect of benefit level is much less documented. In the standard search framework with finite potential duration, the effect of benefit level on the hazard rate should be declining over time because of the entitlement effect. This entitlement effect is however likely to be second order, and the effect of benefit level is usually assumed constant over time. Results presented here suggest to the contrary that the effect of benefit level on the hazard rate is increasing over time. Note, once again, that it is of course impossible in the RKD identification framework to disentangle duration dependence from selection. In other words, this increase in the effect of benefit level on exit rate out of unemployment can be due to a change in the effect of benefit level on each individual over time or to a different mix of unobserved heterogeneity types at each point of support of the hazard function. These two potential sources of heterogeneity have nevertheless different welfare implications. If the heterogeneity is mainly driven by different idiosyncratic utility costs of search that are private information, then standard model with only unobserved effort and moral hazard might be inappropriate[27]. But the increase in the effect of benefit level over time can originate from duration dependence, such as an increasing marginal utility cost of search for each individual over the course of her unemployment spell as suggested in Krueger and Mueller [2011], due to motivation issues or other psychological factors.

[27] Hagedorn et al. [2002] or Fuller [2009] incorporate this additional informational friction in model of optimal UI. Interestingly, this additional type of information asymmetry and the cyclical behavior of heterogeneity suggests an important role for the potential duration of UI as a screening device between high and low types for the utility cost of search.

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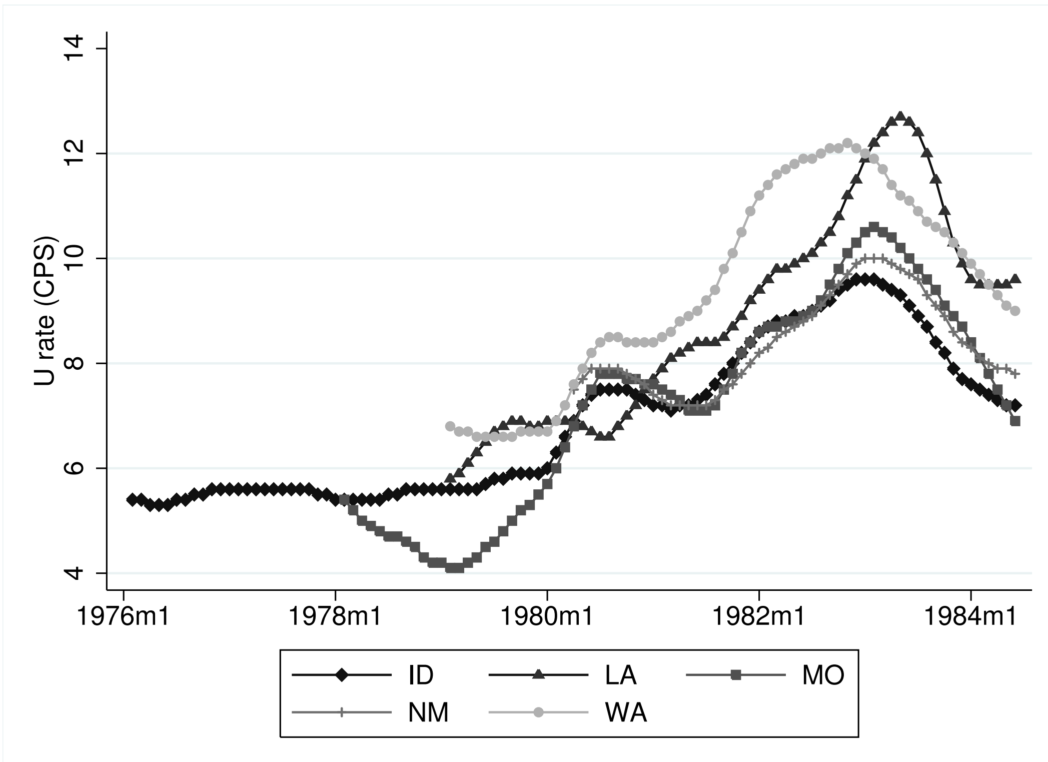
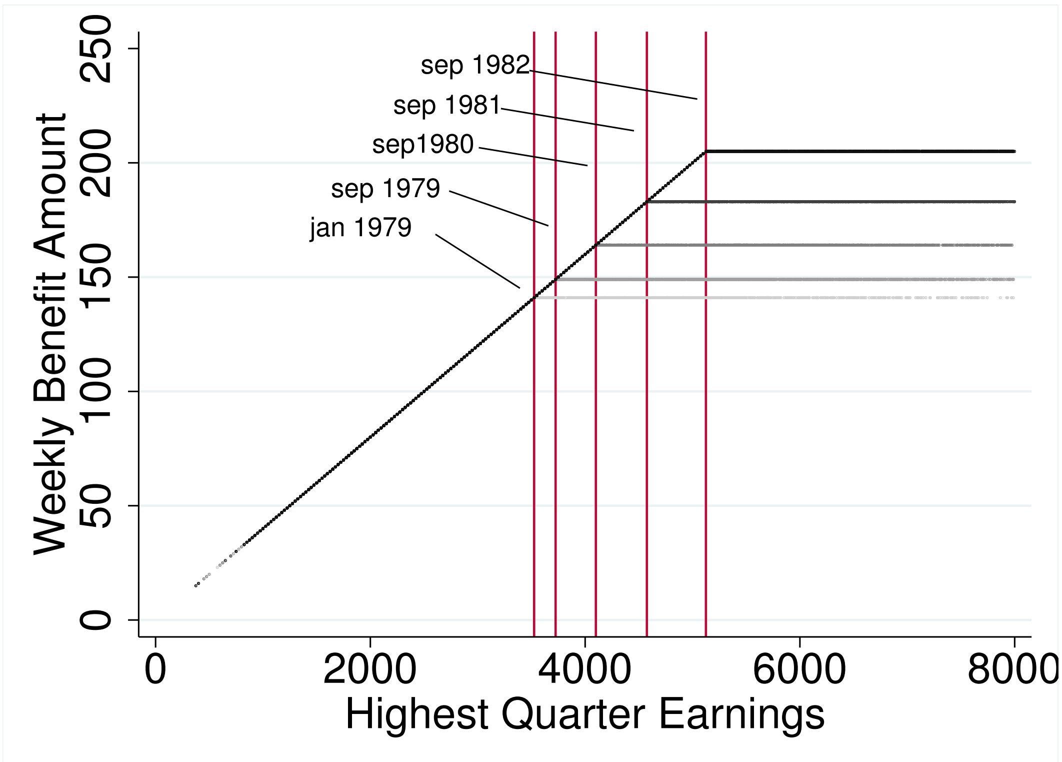
Figure 1: UNEMPLOYMENT RATES IN CWBH STATES 1976-1984



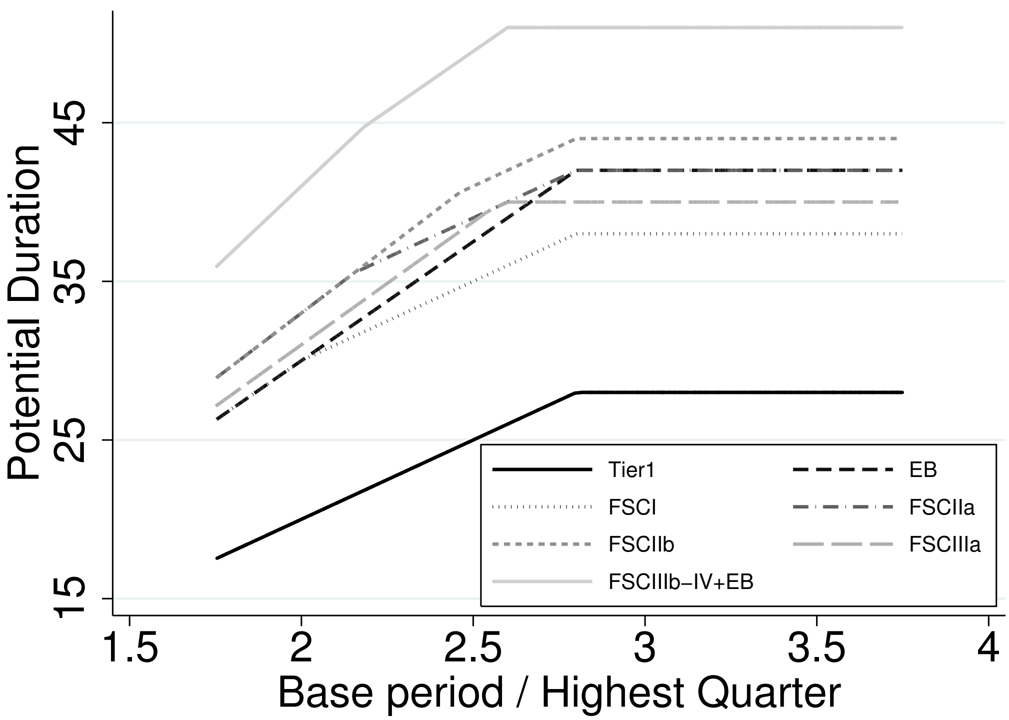
Sources: Current Population Survey
Notes: The graph shows the evolution of the monthly unemployment rate in the 5 states with the universe of unemployment spells available from the CWBH data. The CWBH data for the 5 states covers period of low unemployment as well as the two recessions of 1980 and 1981-82 with two-digit national unemployment rates, which gives the opportunity to examine the evolution of behavioral responses to UI over the business cycle.

Figure 2: LOUISIANA: SCHEDULE OF UI WEEKLY BENEFIT AMOUNT, JAN1979-DEC1983



Sources: Louisiana Revised Statutes RS 23:1592 and yearly *Significant Provisions of State Unemployment Insurance Laws* 1976 to 1984, Dpt of Labor, Employment & Training Administration.
Notes: The graph shows the evolution of the schedule of the weekly benefit amount (WBA) as a deterministic and kinked function of the highest quarter of earnings in Louisiana. The schedule applies based on the date the UI claim was filed, so that a change in the maximum weekly benefit amount does not affect the weekly benefit amount of ongoing spells.

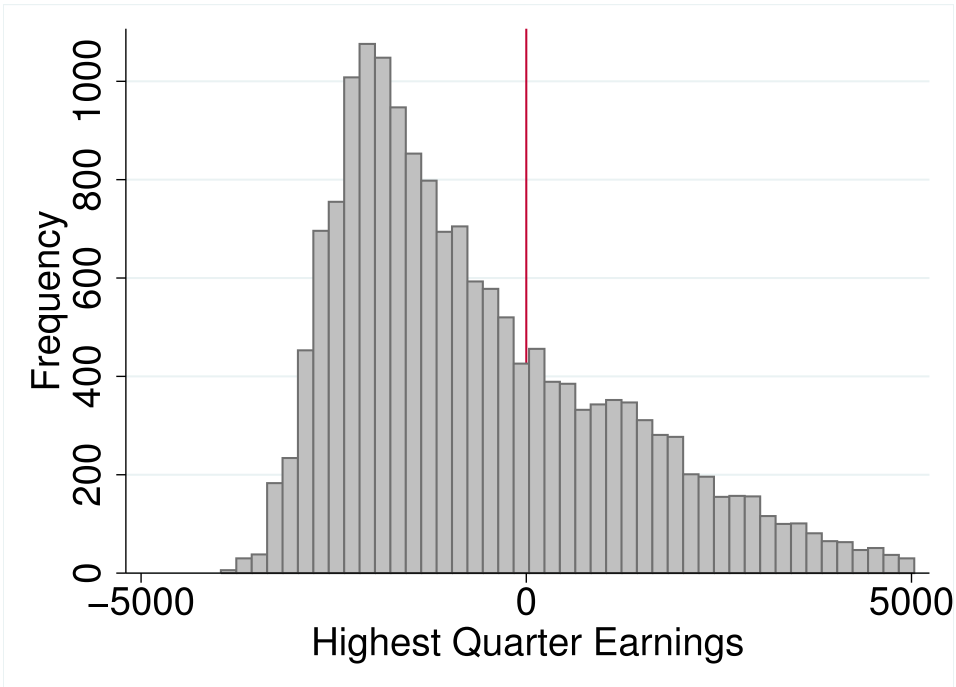
Figure 3: LOUISIANA: SCHEDULE OF UI POTENTIAL DURATION, JAN1979-DEC1983



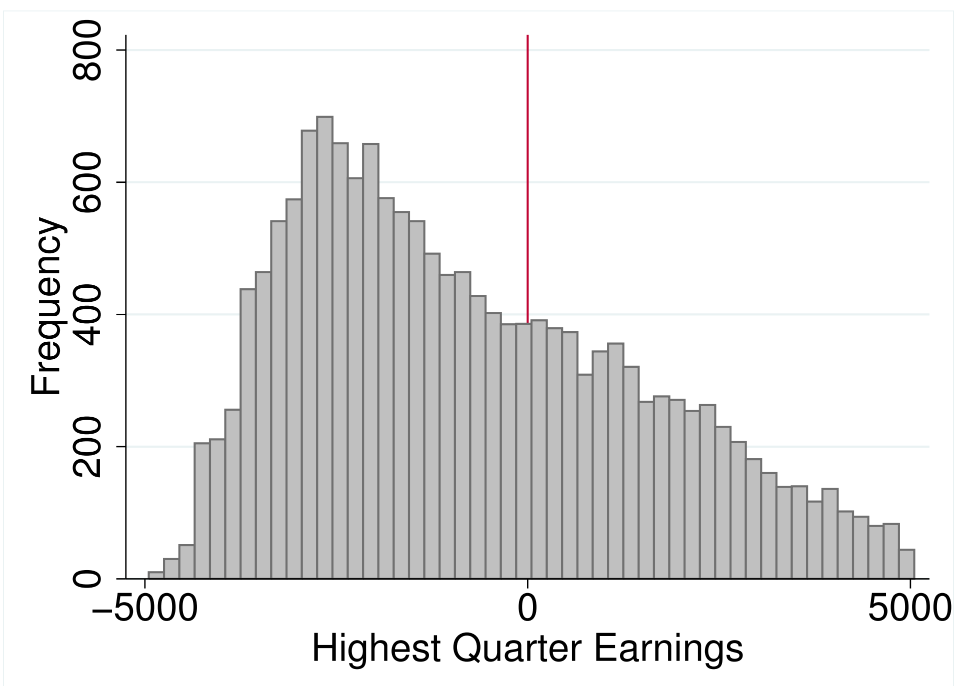
Sources: Louisiana Revised Statutes RS 23:1592 and weekly state trigger notice reports
Notes: The graph shows the evolution of the schedule of the potential duration of UI benefits as a deterministic and kinked function of the ratio of base period earnings to highest quarter of earnings in Louisiana. The schedule applies based on the date of the week of certified unemployment so that changes in the schedule do usually affect ongoing spells. Specific eligibility rules also apply to qualify for the different phases of the FSC.

Figure 4: LOUISIANA: NUMBER OF OBSERVATIONS IN EACH BIN OF HIGHEST QUARTER EARNINGS

A. JAN 1979 - SEPT 1981

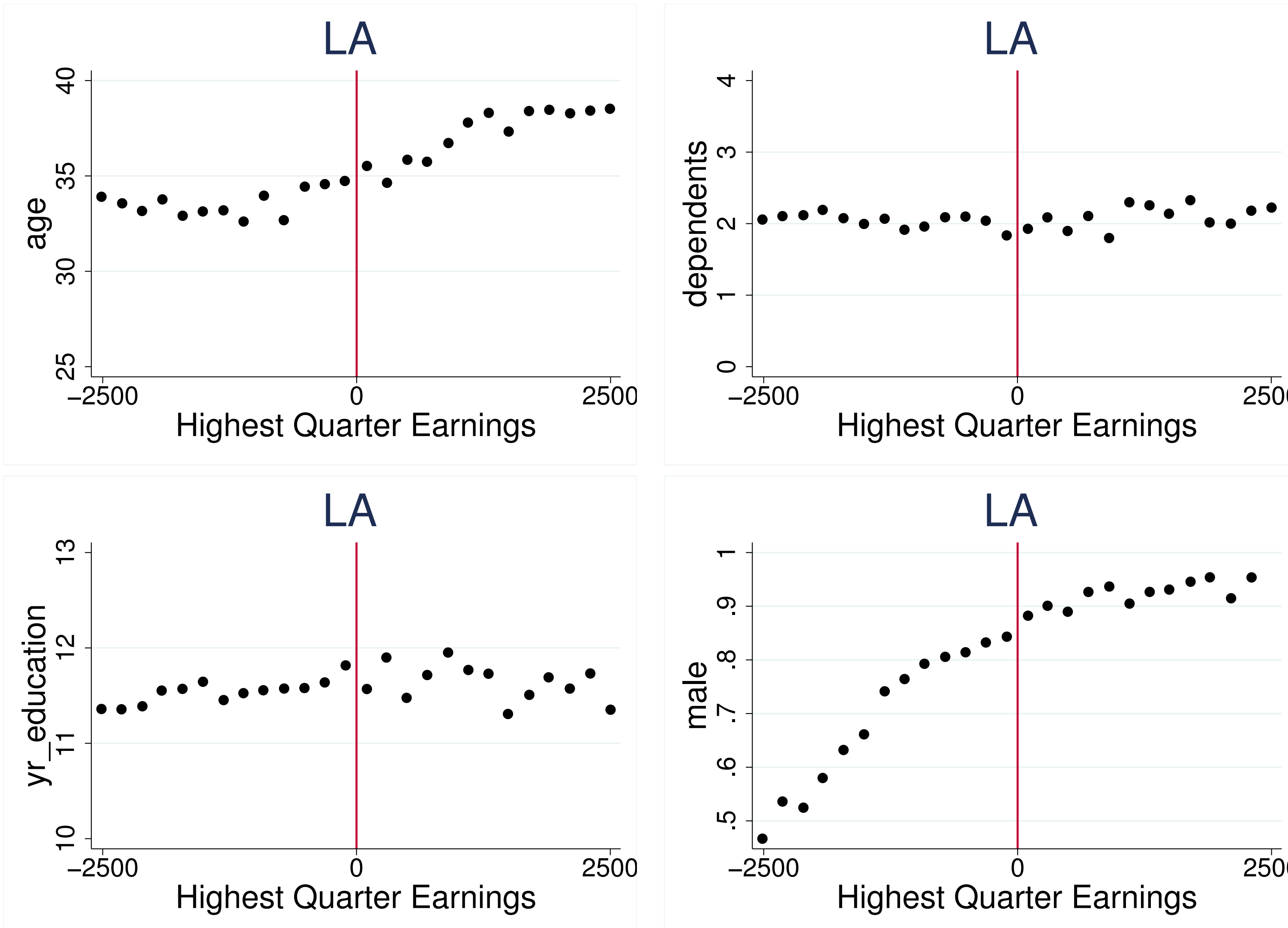


B. SEPT 1981 - DEC 1983



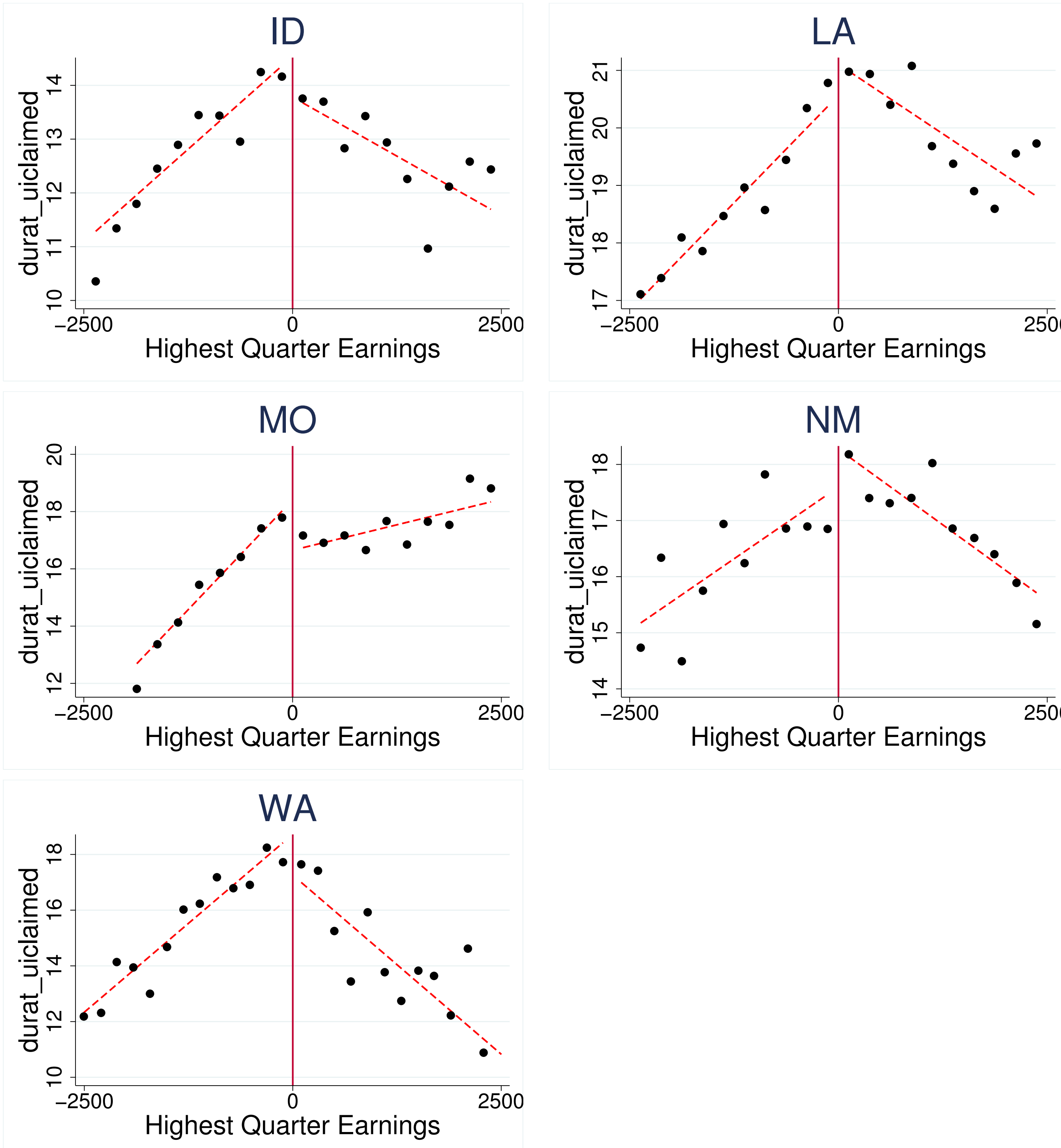
Notes: The graph shows the p.d.f of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The binsize is 250 and passes the test of excess smoothing recommended in Lee and Lemieux [2010].

Figure 5: COVARIATES VS HIGHEST QUARTER EARNINGS, LOUISIANA JAN 1979- SEP 1981



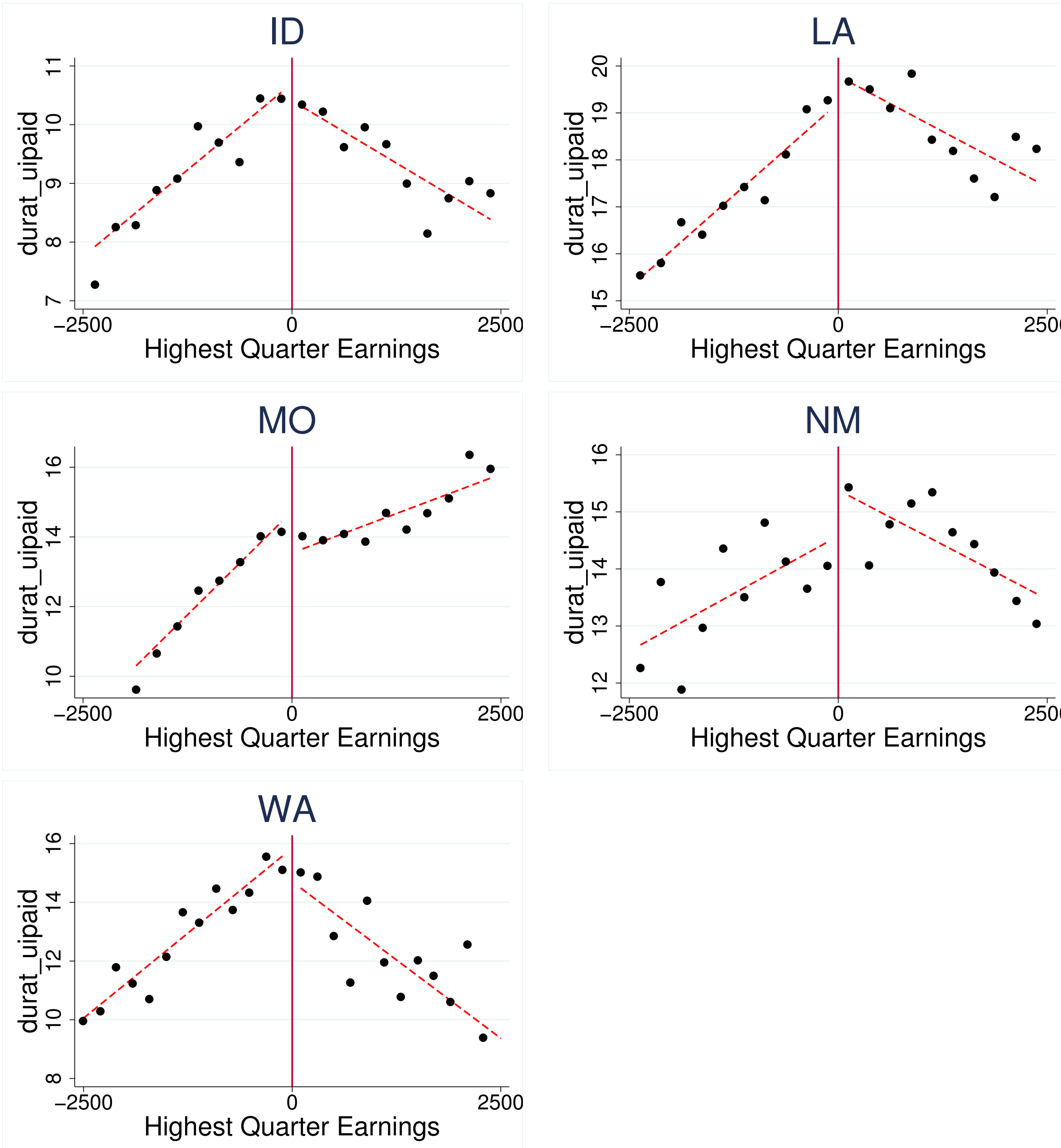
Notes: The graph shows for the first sub-period of analysis in Louisiana the mean values of the covariates in each bin of $250 of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The graph shows evidence of smoothness in the evolution of covariates at the kink, in support of the RKD identification assumptions. Formal tests of smoothness are displayed in table 2.

Figure 6: RKD FOR THE EFFECT OF BENEFIT LEVEL: DURATION OF UI CLAIMS VS HIGHEST QUARTER EARNINGS FOR ALL 5 STATES



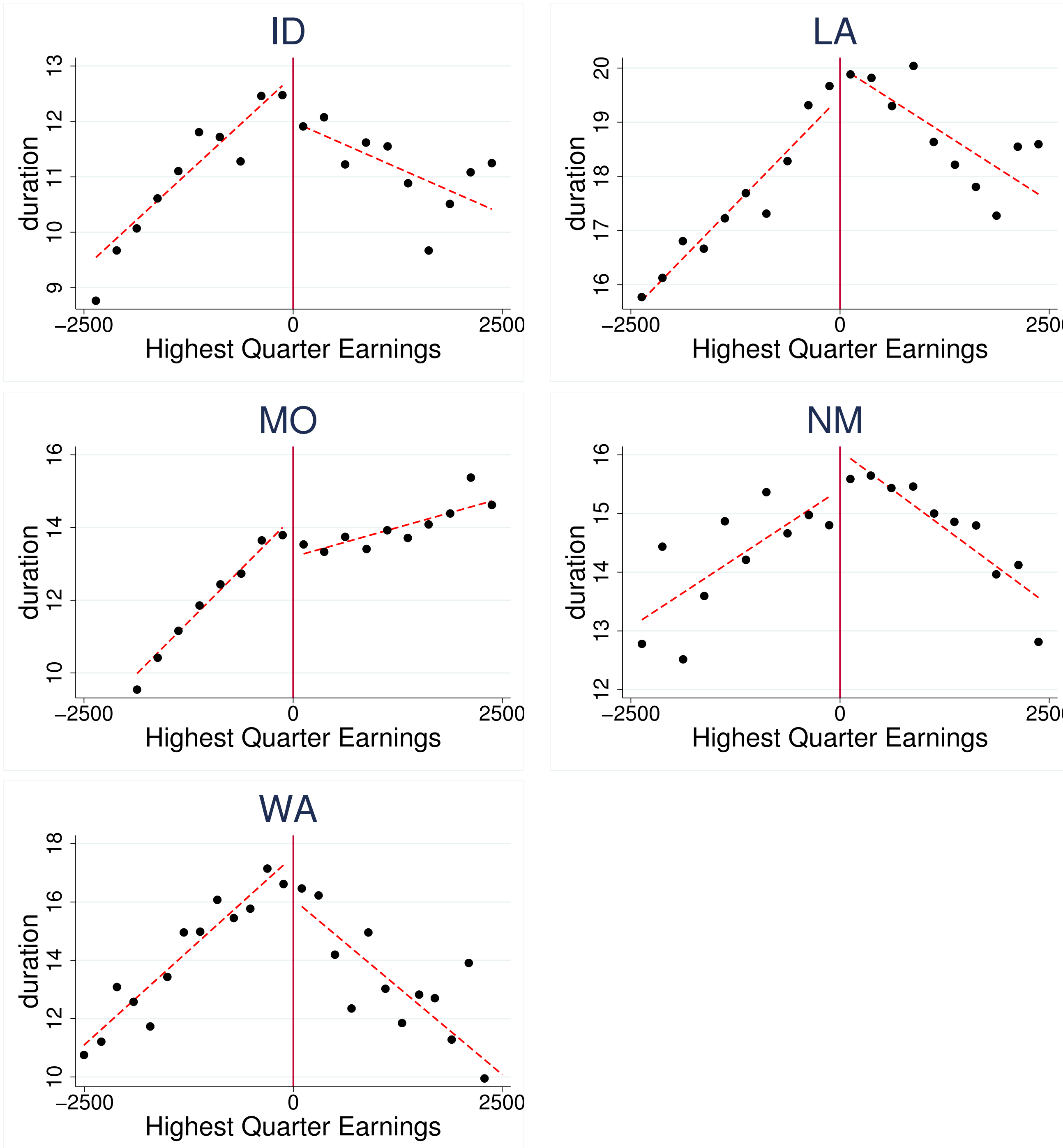
Notes: The graph shows for the first sub-period of analysis in each state the mean values of the duration of UI claims in each bin of $250 of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The graph shows evidence of a kink in the evolution of the outcome at the kink. Formal estimates of the kink using polynomial regressions of the form of equation 1 are displayed in table 2. The red lines display predicted values of the regressions in the linear case allowing for a discontinuous shift at the kink.

Figure 7: RKD FOR THE EFFECT OF BENEFIT LEVEL: DURATION UI PAID VS HIGHEST QUARTER EARNINGS FOR ALL 5 STATES



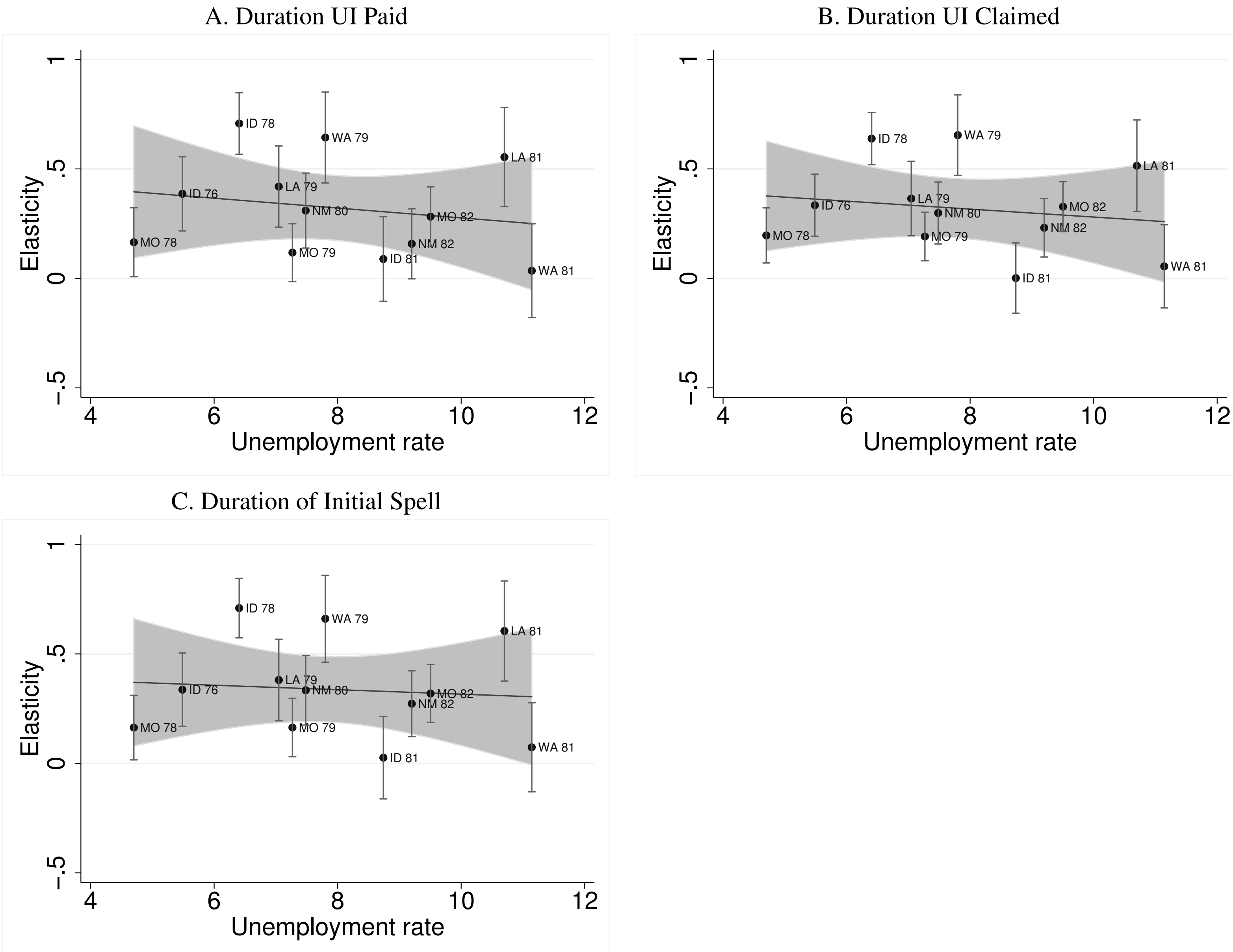
Notes: The graph shows for the first sub-period of analysis in each state the mean values of the duration of paid UI in each bin of $250 of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The graph shows evidence of a kink in the evolution of the outcome at the kink. Formal estimates of the kink using polynomial regressions of the form of equation 1 are displayed in table 2. The red lines display predicted values of the regressions in the linear case allowing for a discontinuous shift at the kink.

Figure 8: RKD for the Effect of Benefit Level: Duration of Initial Unemployment Spell vs Highest Quarter Earnings for All 5 States



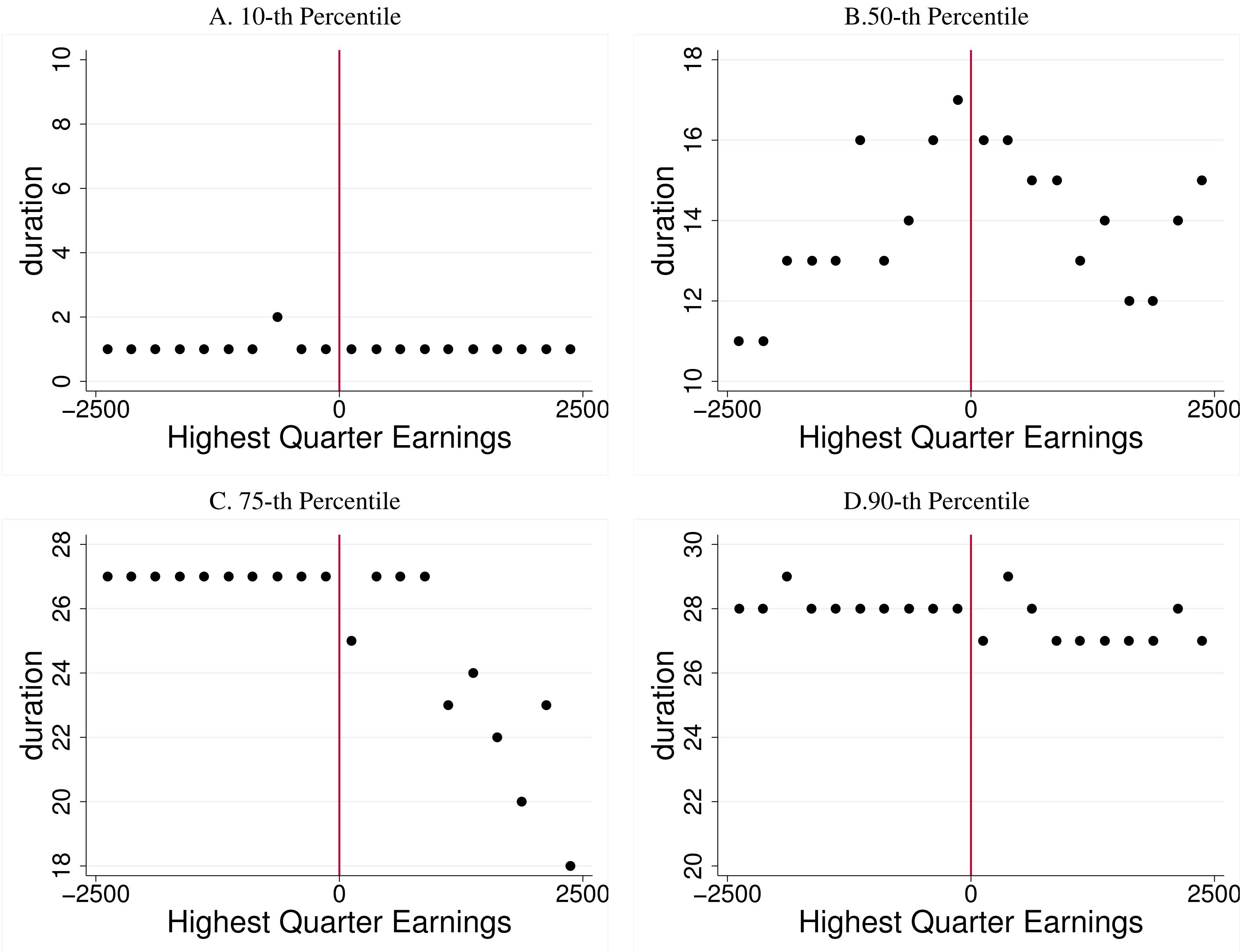
Notes: The graph shows for the first sub-period of analysis in each state the mean values of the duration of initial spell in each bin of $250 of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The graph shows evidence of a kink in the evolution of the outcome at the kink. Formal estimates of the kink using polynomial regressions of the form of equation 1 are displayed in table 2. The red lines display predicted values of the regressions in the linear case allowing for a discontinuous shift at the kink.

Figure 9: CORRELATION BETWEEN ESTIMATES OF ELASTICITY WITH RESPECT TO BENEFIT LEVEL & UNEMPLOYMENT RATE



Notes: The graph correlates the estimated elasticities (with their 95% confidence interval) of the three duration outcomes with the average monthly unemployment rate in the state during each sub-period computed from the CPS. The line displays the result of a regression fit (with weights equal to the inverse of the standard errors) and the grey area is the 95% robust confidence interval of the fit.

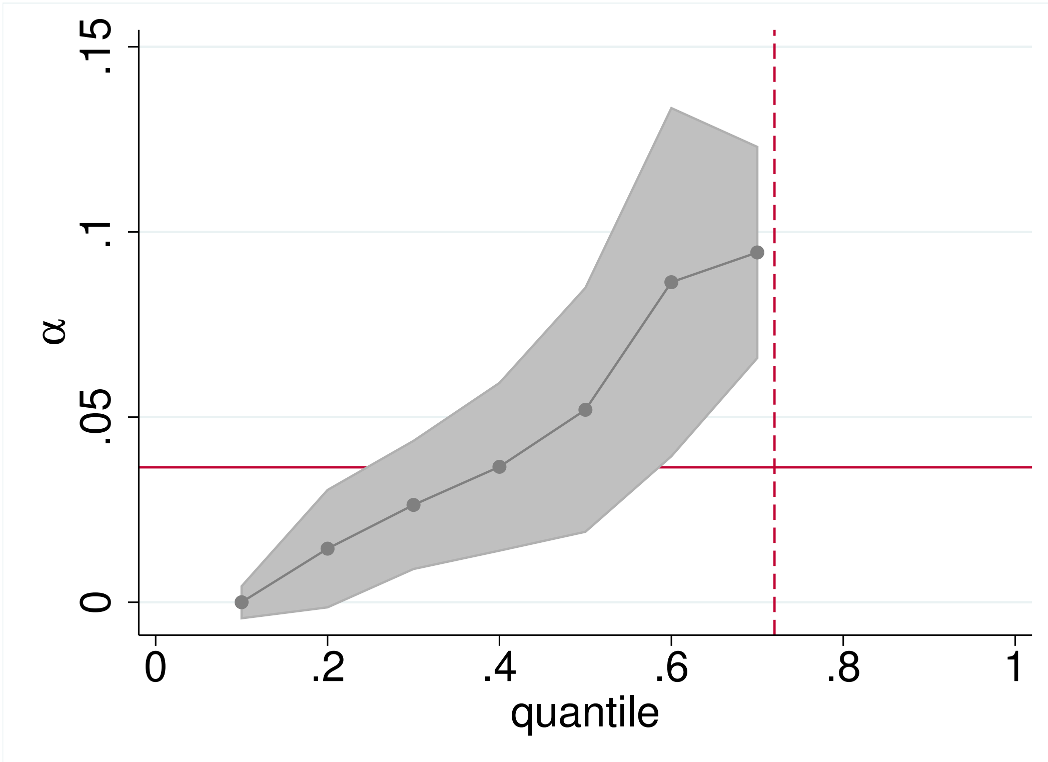
Figure 10: DISTRIBUTIONAL EFFECTS: DURATION OF INITIAL SPELL, LOUISIANA 1979-1981



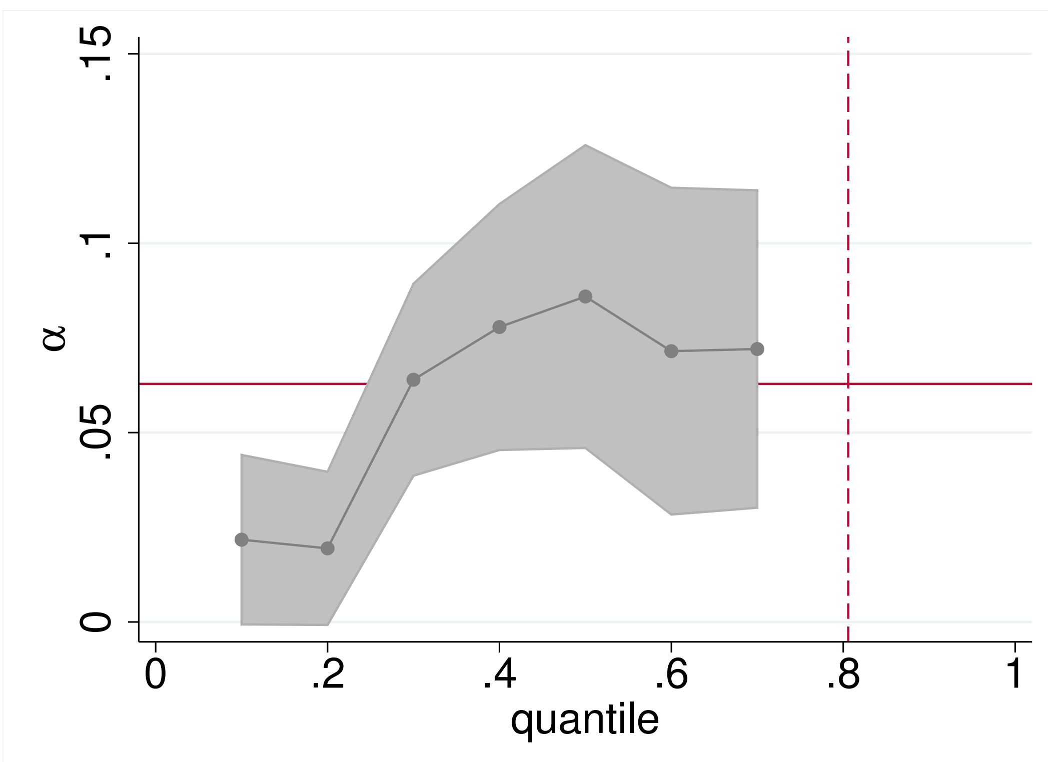
Notes: The graph shows for the first sub-period of analysis in Lousiana different percentiles of the duration of initial spell in each bin of $250 of highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level. The assignment variable is centered at the kink. The graph shows no evidence of a kink in the evolution of the outcome in the lower part of the distribution of duration but a sharp kink in higher parts of the distribution, until reaching the censoring point. Formal estimates of the quantile treatment effects are displayed in figure 11.

Figure 11: LOUISIANA, QTE ESTIMATES- DURATION OF INITIAL SPELL

A. 1979-1981: U=7.0%



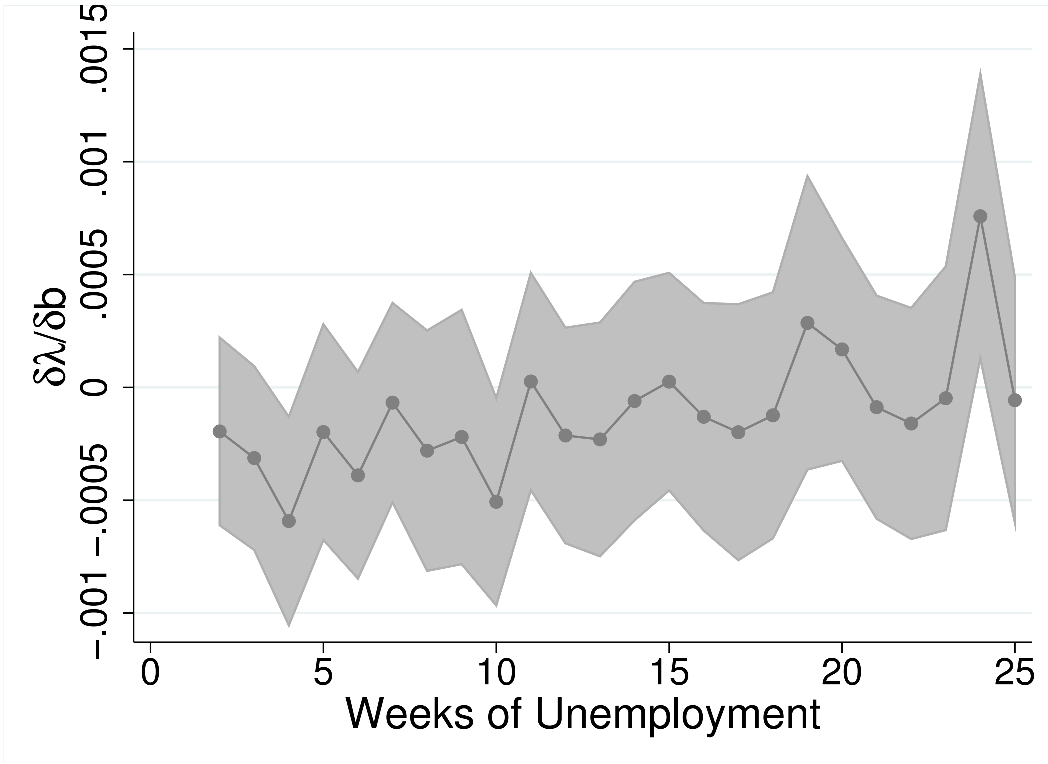
B. 1981-1983: U=10.8%



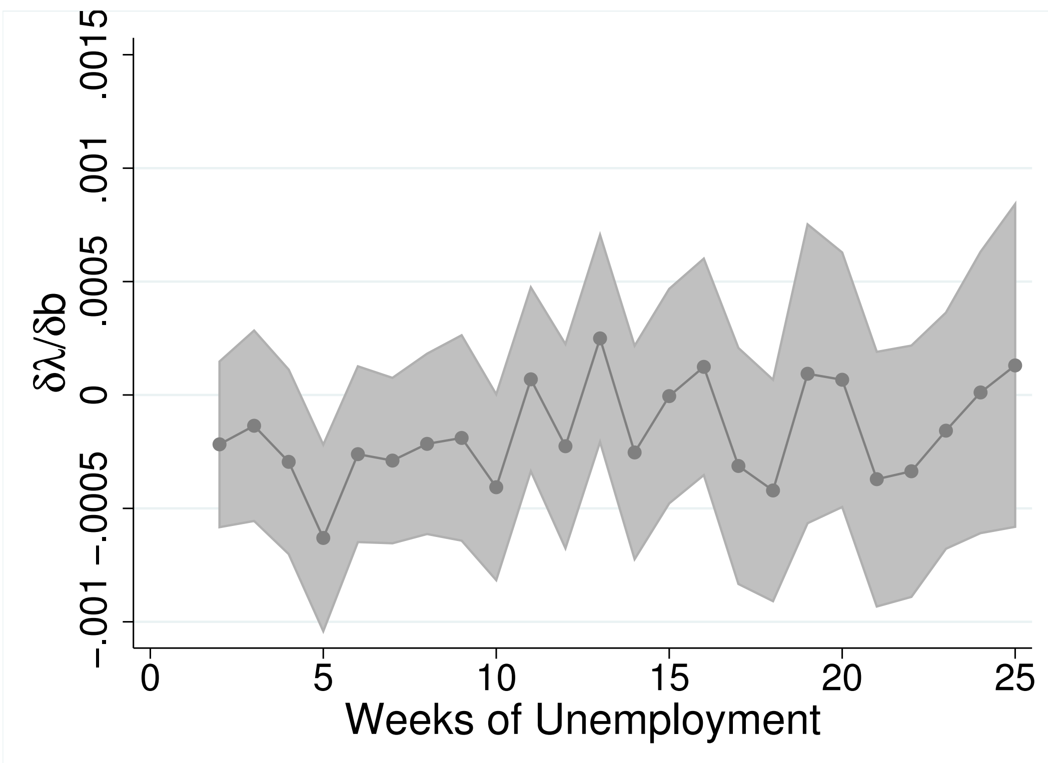
Notes: The graph shows in Louisiana for the two sub-periods of analysis the estimated quantile treatment effects of benefit level. The plain red line depicts the level of the baseline average treatment effect. The dotted red line depicts the minimum percentage of censoring across all bins of the assignment variable in the estimation bandwidth.

Figure 12: LOUISIANA, ESTIMATES OF THE EFFECT ON THE HAZARD RATE OF THE DURATION OF INITIAL SPELL OF A 10$ INCREASE IN WBA

A. 1979-1981: U=7.0%

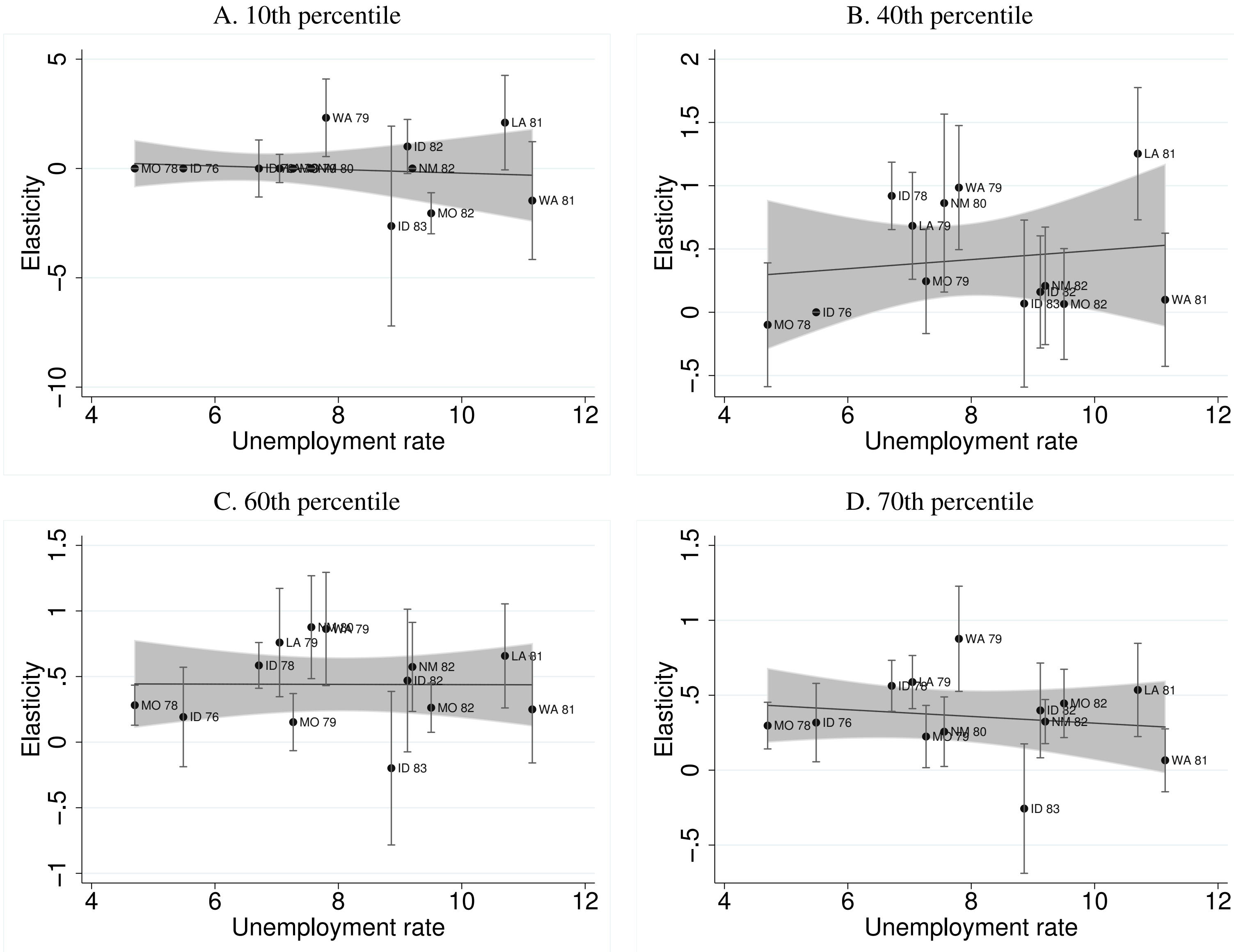


B. 1981-1983: U=10.8%



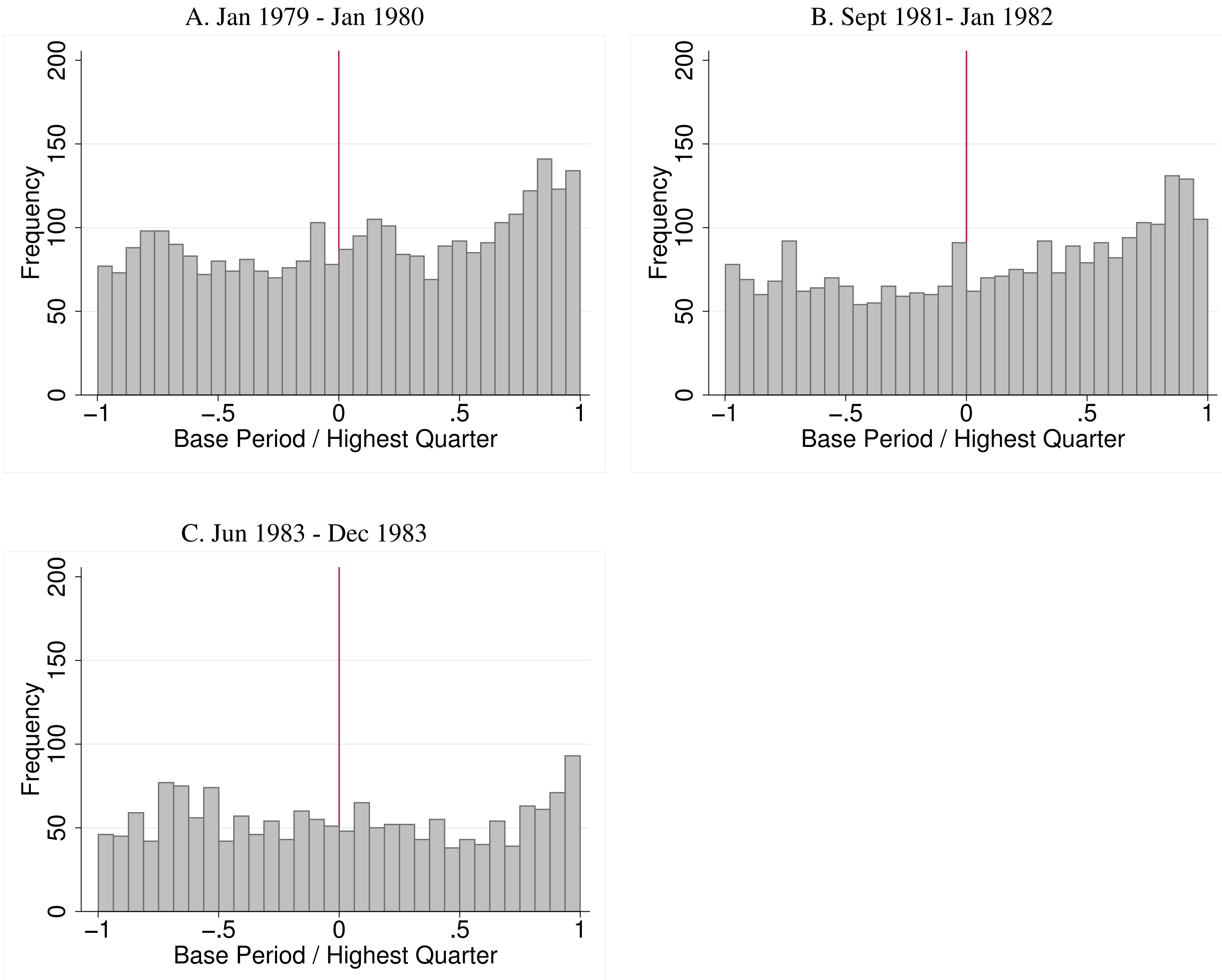
Notes: The graph shows in Louisiana for the two sub-periods of analysis the estimated effects of benefit level on the hazard rate at each week of unemployment duration.

Figure 13: Correlation between Estimates of the Elasticity w.r.t Benefit Level at Different Percentile of the Distribution & Unemployment Rate



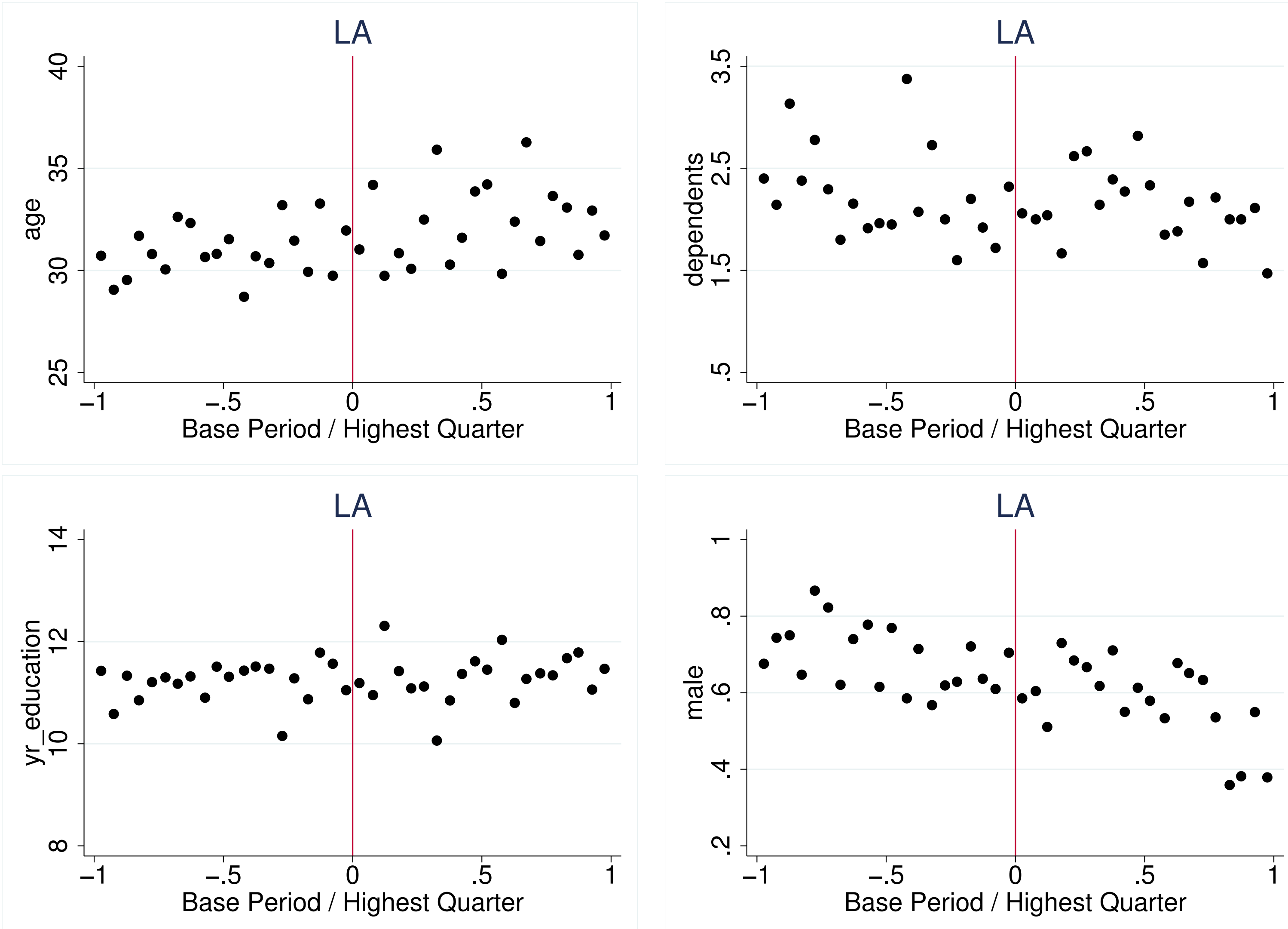
Notes: The graph correlates for different percentile of the distribution of the duration of initial spell, the estimated elasticities (with their 95% confidence interval) implied by the quantile treatment effects and the average monthly unemployment rate in the state during each sub-period computed from the CPS. The line displays the result of a regression fit (with weights equal to the inverse of the standard errors) and the grey area is the 95% robust confidence interval of the fit.

Figure 14: LOUISIANA: NUMBER OF OBSERVATIONS IN EACH BIN OF THE RATIO BASE PERIOD / HIGHEST QUARTER EARNINGS



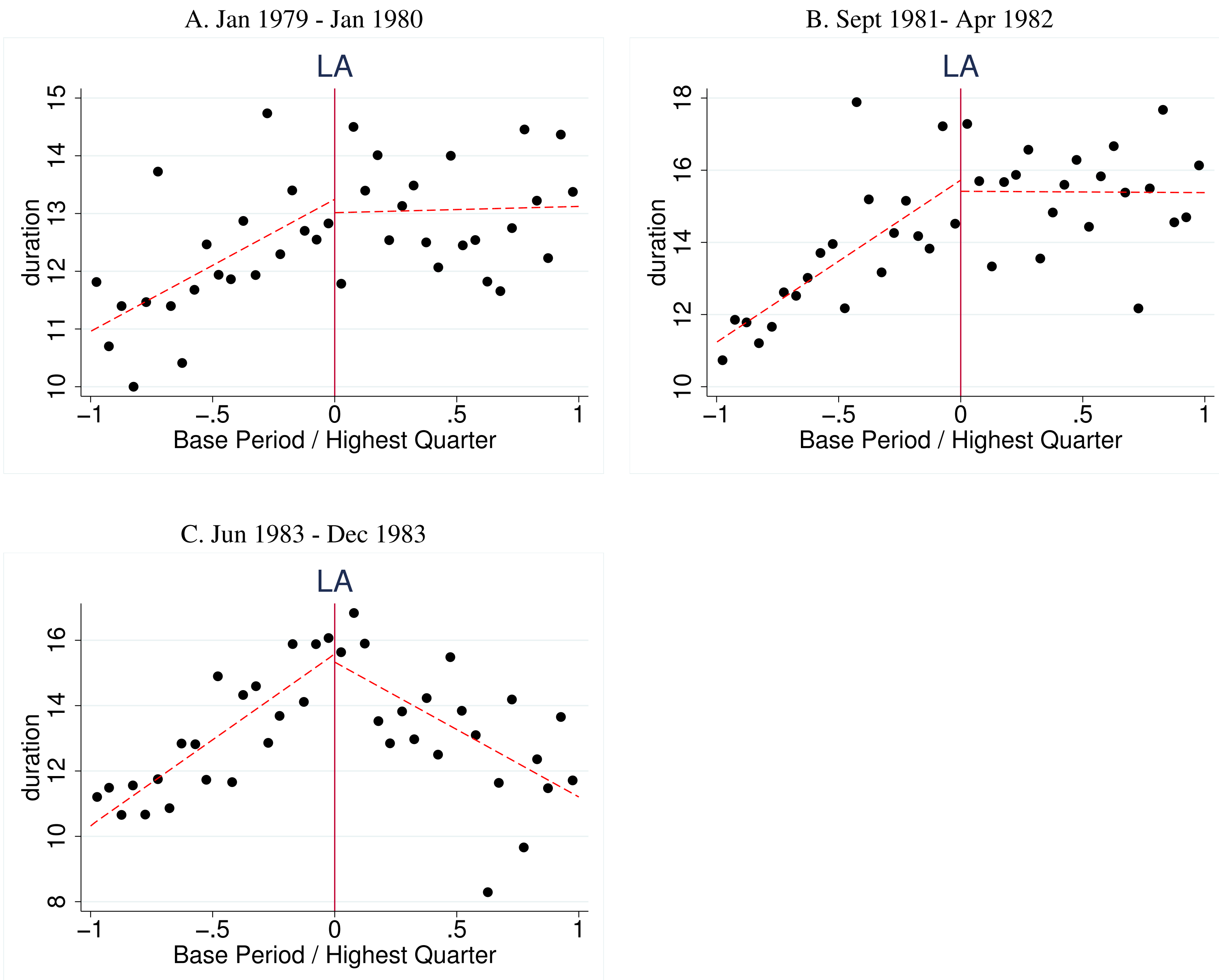
Notes: The graph shows the p.d.f of the ratio of base period to highest quarter earnings (centered at the kink), which is the assignment variable in the RK design for the estimation of the effect of potential duration. The binsize is .05 and passes the bin test of excess smoothing of Lee and Lemieux [2010]. The three sub-periods are chosen so that all individuals face a stable schedule for potential duration during the entire length of their potential duration.

Figure 15: COVARIATES VS RATIO BASE PERIOD / HIGHEST QUARTER EARNINGS IN LOUISIANA FOR JUN 1983 - DEC 1983



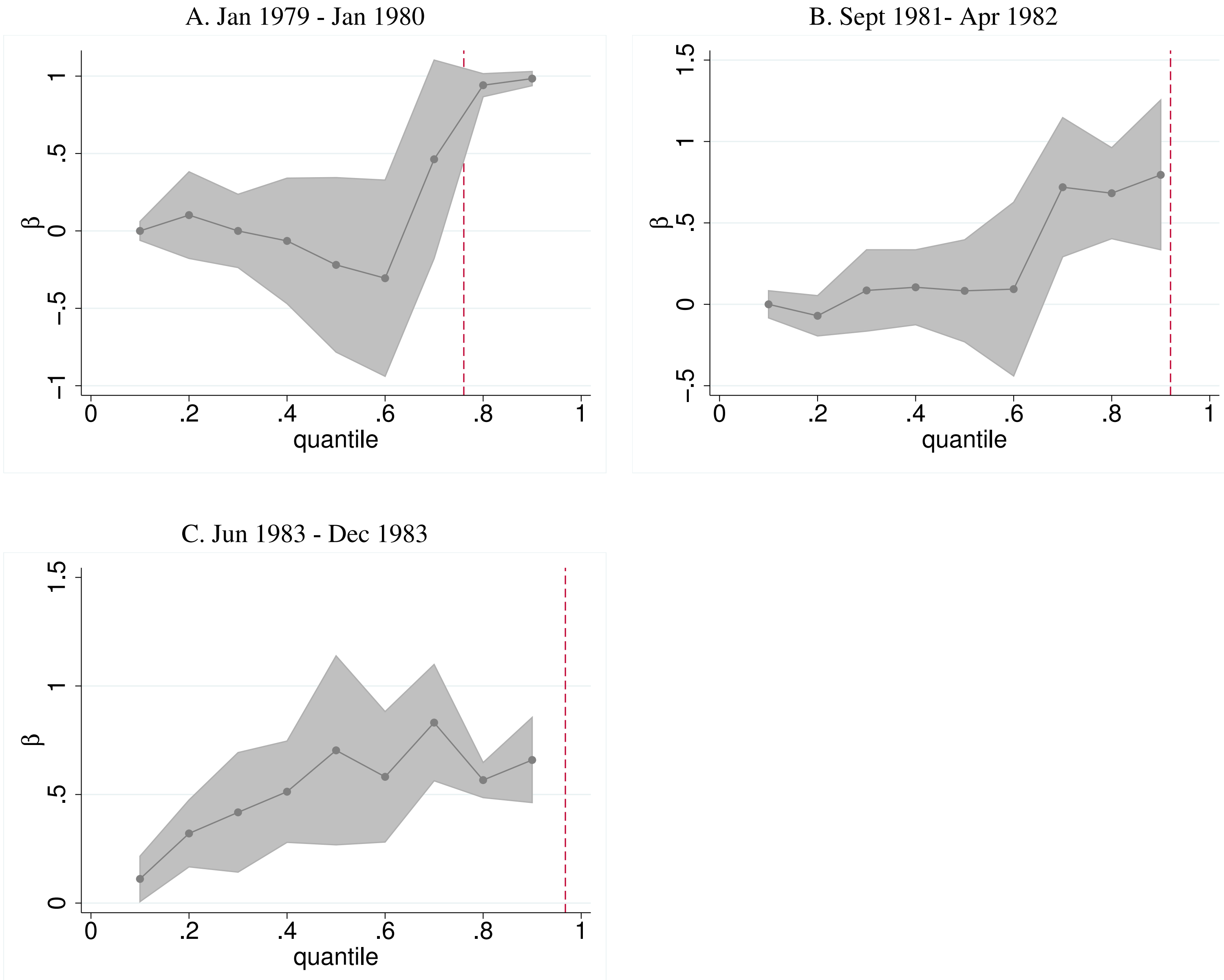
Notes: The graph shows for the last sub-period of analysis of potential duration in Louisiana the mean values of the covariates in each bin of .05 of the ratio of base period to highest quarter earnings, which is the assignment variable in the RK design for the estimation of the effect of potential duration. The assignment variable is centered at the kink. The graph shows evidence of smoothness in the evolution of covariates at the kink, in support of the RKD identification assumptions. Formal tests of smoothness are displayed in table 7.

Figure 16: RKD FOR THE EFFECT OF POTENTIAL DURATION: DURATION OF INITIAL SPELL VS ASSIGNMENT VARIABLE IN LOUISIANA FOR 3 PERIODS



Notes: The graph shows for the three sub-periods of analysis of potential duration in Louisiana the mean values of the duration of initial spell in each bin of .05 of the assignment variable centered at the kink. The graph shows evidence of a kink in the evolution of the outcome at the kink. Formal estimates of the kink are displayed in table 7. The red lines display predicted values in the linear case allowing for a discontinuous shift at the kink.

Figure 17: Quantile Treatment Effect Estimates for the Effect of Potential Duration on Duration of Initial Spell, Louisiana



Notes: The graph shows in Louisiana for the three sub-periods of analysis the estimated quantile treatment effects of potential duration. The red line depicts the minimum percentage of censoring across all bins of the assignment variable in the estimation bandwidth.

Table 1: DESCRIPTIVE STATISTICS

	Idaho			Louisiana			Missouri			New Mexico			Washington		
	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N	Mean	s.d.	N
	Duration Outcomes (wks)														
Initial spell	13.9	12.4	33365	14	10.6	34077	12.2	10.9	28665	14	12.6	27004	17.6	15.4	41992
wks UI paid	11.7	10.7	33365	13.8	10.4	34077	12.5	11.3	28665	13.4	12.8	27004	16.2	14.8	41992
wks UI claim	15.8	12.2	33365	15.1	10.4	34077	15.4	11.8	28665	15.8	12.6	27004	18.9	15.4	41992
	Earnings and Benefits ($2010)														
bpw	25136	22164	33365	26993	19446	34077	23733	17334	28665	23334	17132	27004	31232	20380	41992
hqw	9827	16405	33365	9581	6441	34077	8211	5830	28665	8252	5382	27004	8982	5321	41992
wba	262.4	86.3	33365	304.8	117.1	34077	225	51.4	28665	230	69.5	27004	286.7	94.7	41992
potential duration Tier I	20	5.5	33365	25	4.4	34077	22.1	5.2	28665	25.7	1	27004	27	4.2	41992
	Covariates														
age	30.2	12.7	33361	34.6	12.7	33850	34.8	12.7	28651	33.7	11.4	26924	34.2	11.9	41955
male	.666	.471	33361	.683	.465	33624	.609	.488	28663	.651	.477	27002	.627	.484	41972
educ. (yrs)	12	2.2	17774	11.4	2.7	31272	11.3	2.2	1867	11.7	2.5	26482	12.4	2.4	41702
dependents	2	1.6	18781	2	1.6	17325	2	1.6	21746	2.2	1.7	25534	1.7	1.5	28834
censored	.165	.362	33365	.128	.323	34077	.151	.382	28665	.162	.336	27004	.107	.289	41992

Notes: The initial spell, as defined in Spiegelman et al. [1992], starts at the date the claim is filed and ends when there is a gap of at least two weeks in the receipt of UI benefits. The duration of paid UI corresponds to the number of weeks a claimant receives unemployment compensation. The duration of a UI claim is the number of weeks a claimant is observed in the administrative data for a given unemployment spell. . bpw is the base period earnings, and hqw is the highest quarter of earnings. wba is the weekly benefit amount of UI. Potential duration Tier I is the potential duration of the regular state UI program. In Missouri, information on education level is almost always unavailable.

Table 2: BASELINE RKD ESTIMATES OF THE EFFECT OF BENEFIT LEVEL, LOUISIANA JAN 1979 - DEC 1983

	(1) Duration of Initial Spell	(2) Duration UI Claimed	(3) Duration UI Paid	(4) Age	(5) Years of Education	(6) Male	(7) Dependents
	Period 1: Jan 1979- Sept 1981 U=7.0%						
α	.036	.041	.038	-.013	-.001	-.001	-.006
	(.009)	(.009)	(.009)	(.103)	(.023)	(.003)	(.004)
$\varepsilon_b = \frac{dY}{db} \cdot \frac{b}{Y}$	.382	.421	.366				
	(.095)	(.095)	(.087)				
p-value	.968	.917	.948	.188	.346	.394	.462
N	8073	8073	8073	8036	7449	7983	4814
Opt. Poly	1	1	1	3	3	3	1
	Period 2: Sept 1981- Dec 83 U=10.8%						
α	.063	.047	.072	.018	.007	-.008	-.004
	(.104)	(.01)	(.102)	(.098)	(.009)	(.005)	(.002)
$\varepsilon_b = \frac{dY}{db} \cdot \frac{b}{Y}$	.713	.552	.765				
	(1.18)	(.115)	(1.08)				
p-value	.291	.706	.38	.426	.085	.481	.414
N	6899	6899	6899	6852	6268	6807	3128
Opt. Poly	3	1	3	3	1	3	1

Notes: Duration outcomes are expressed in weeks. α is the RK estimate of the average treatment effect of benefit level on the outcome. Standard errors for the estimates of α are in parentheses. The elasticity of the three duration outcomes with respect to the UI benefit level $\varepsilon_b = \widehat{\alpha} \cdot \frac{b_{max}}{\overline{Y_1}}$, where $\overline{Y_1}$ is mean duration at the kink point, are also reported. P-values are from a test of joint significance of the coefficients of bin dummies in a model where bin dummies are added to the polynomial specification in equation 1. The optimal polynomial order is chosen based on the minimization of the Aikake Information Criterion.

Table 3: Estimates of the Effect of Benefit Level by Polynomial Order, Louisiana Sept 1981- Dec 83

	(1) Duration of Initial Spell	(2) Duration UI Claimed	(3) Duration UI Paid	(4) Age	(5) Years of Education	(6) Male	(7) Dependents
				Poly Order=1			
α	.053	.047	.048	-.032	.007	.004	-.004
	(.01)	(.01)	(.01)	(.01)	(.002)	(0)	(.002)
p-value	.396	.706	.442	.205	.085	.006	.414
N	6899	6899	6899	6852	6268	6807	3128
AIC	53847.4	53323.4	53555.8	52744.3	30284.0	6384.7	11700.5
				Poly Order=2			
α	.092	.075	.091	-.045	.005	-.001	-.01
	(.041)	(.039)	(.04)	(.039)	(.01)	(.001)	(.008)
p-value	.478	.729	.549	.492	.079	.192	.416
N	6899	6899	6899	6852	6268	6807	3128
AIC	53849.5	53326.5	53558.1	52742.2	30287.8	6367.2	11703.9
				Poly Order=3			
α	.063	.074	.072	.018	.017	-.008	-.005
	(.104)	(.1)	(.102)	(.098)	(.026)	(.003)	(.02)
p-value	.291	.551	.38	.426	.072	.481	.378
N	6899	6899	6899	6852	6268	6807	3128
AIC	53845.1	53324.0	53553.9	52741.5	30286.0	6361.9	11703.56

Notes: The table explores the sensitivity of the results to the choice of the polynomial order for the regression specification in equation 1. α is the RK estimate of the average treatment effect of benefit level on the outcome. Standard errors for the estimates of α are in parentheses. P-values are from a test of joint significance of the coefficients of bin dummies in a model where bin dummies are added to the polynomial specification in equation 1. AIC is the Aikake Information Criterion.

Table 4: BASELINE RKD ESTIMATES OF THE EFFECT OF BENEFIT LEVEL FOR DIFFERENT BANDWIDTH LEVELS, LOUISIANA SEPT 1981- DEC 83

	(1) Duration of Initial Spell	(2) Duration UI Claimed	(3) Duration UI Paid	(4) Age	(5) Years of Education	(6) Male	(7) Dependents
			Bandwidth=1500				
α	.063	.05	.162	-.07	.012	-.009	-.003
	(.022)	(.021)	(.224)	(.212)	(.005)	(.007)	(.004)
p-value	.405	.61	.277	.922	.084	.546	.109
N	3972	3972	3972	3948	3598	3922	1816
Opt. Poly	1	1	3	3	1	3	1
			Bandwidth=2500				
α	.063	.047	.072	.018	.007	-.008	-.004
	(.104)	(.01)	(.102)	(.098)	(.009)	(.005)	(.002)
p-value	.291	.706	.38	.426	.085	.481	.414
N	6899	6899	6899	6852	6268	6807	3128
Opt. Poly	3	1	3	3	1	3	1
			Bandwidth=4500				
α	.099	.076	.094	-.074	.005	-.003	-.004
	(.047)	(.046)	(.046)	(.048)	(.012)	(.001)	(.001)
p-value	.2	.363	.208	.002	0	0	.321
N	10024	10024	10024	9963	9145	9900	4569
Opt. Poly	3	3	3	3	3	3	1

Notes: The table explores the sensitivity of the results to the choice of the bandwidth for the regression specification in equation 1. α is the RK estimate of the average treatment effect of benefit level on the outcome. Standard errors for the estimates of α are in parentheses. P-values are from a test of joint significance of the coefficients of bin dummies in a model where bin dummies are added to the polynomial specification in equation 1. The optimal polynomial order is chosen to minimize the Aikake Information Criterion.

Table 5: BASELINE RKD ESTIMATES OF THE EFFECT OF BENEFIT LEVEL WITH MORE SUB-PERIODS, LOUISIANA JAN 1979 - DEC 1983

		(1) Duration of Initial Spell	(2) Duration UI Claimed	(3) Duration UI Paid	(4) Age	(5) Years of Education	(6) Male	(7) Dependents
Jan-Sep 79	α	.024	.028	.026	-.061	.008	.007	-.007
		(.018)	(.019)	(.018)	(.022)	(.005)	(.001)	(.003)
	p-value	.19	.146	.264	0	.603	.047	.027
	N	1898	1898	1898	1889	1730	1878	1314
Sep 79-Sep 80	α	.043	.048	.043	-.066	.013	.006	-.003
		(.015)	(.015)	(.015)	(.016)	(.003)	(0)	(.003)
	p-value	.224	.104	.166	.257	.59	.021	.644
	N	3399	3399	3399	3387	3131	3368	1847
Sep 80-Sep 81	α	.035	.038	.037	-.032	.011	.004	-.009
		(.015)	(.015)	(.014)	(.017)	(.004)	(.001)	(.003)
	p-value	.049	.023	.035	.376	.138	.123	.84
	N	2776	2776	2776	2760	2588	2737	1653
Sep 81-Sep 82	α	.051	.04	.05	-.05	.01	.004	-.003
		(.018)	(.017)	(.017)	(.015)	(.004)	(0)	(.003)
	p-value	.108	.19	.176	.022	.148	.115	.127
	N	2905	2905	2905	2887	2654	2862	1031
Sep 82-Dec 83	α	.055	.052	.047	-.019	.005	.004	-.005
		(.012)	(.012)	(.012)	(.012)	(.003)	(0)	(.002)
	p-value	.597	.739	.513	.189	.165	.069	.926
	N	3994	3994	3994	3965	3614	3945	2097

Notes: The table explores the sensitivity of the results to the number of sub-periods of analysis. α is the RK estimate of the average treatment effect of benefit level on the outcome. Standard errors for the estimates of α are in parentheses. P-values are from a test of joint significance of the coefficients of bin dummies in a model where bin dummies are added to the polynomial specification in equation 1. The displayed estimates are for the optimal polynomial order chosen to minimize the Aikake Information Criterion.

Table 6: CYCLICAL BEHAVIOR OF THE RKD ESTIMATES OF THE EFFECT OF BENEFIT LEVEL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Average Treatment Effects							Quantile Treatment Effects			Hazard
	ε_b	ε_b	ε_b	$\varepsilon_{\Delta C}$	α	ε_b	ε_b	α_τ	α_τ	$\varepsilon_{b,\tau}$	$(\partial\lambda/\partial b)\times 1000$
U	-0.0103	0.0337	0.0250	-0.000944	-0.00839		0.0564				
	(0.0356)	(0.0283)	(0.0352)	(0.0386)	(0.0250)		(0.0255)				
$\frac{\dot{U}}{U}$						-0.00342					
						(0.140)					
τ								0.114***	0.174***	1.556*	
								(0.0140)	(0.0329)	(0.806)	
$\tau \times U$									-0.00742**	-0.114	
									(0.00369)	(0.0906)	
t											0.124
											(0.0756)
$t \times U$											-0.00939
											(0.00851)
age							-0.145**				
							(0.0333)				
male							0.561				
							(0.360)				
dependents							-1.461*				
							(0.528)				
education							-0.606**				
							(0.190)				
State F-E			×	×	×	×		×	×	×	×
Inverse s-e weights		×	×	×	×	×	×	×	×	×	×
N	12	12	12	12	12	12	9	91	91	91	216

Standard errors in parentheses, * p<0.10, ** p<0.05, *** p<0.01

Notes: U is the average monthly state unemployment rate from CPS and $\frac{\dot{U}}{U}$ is the average monthly growth rate of unemployment in the state. Both are expressed in percentage points, so that the results in column (1) for instance should be interpreted as follows: a 1 percentage point increase in the unemployment rate is associated with a .01 percentage point decrease in the estimated elasticity. $\tau \in [0,1]$ is the percentile of the distribution of unemployment duration at which the quantile treatment effect α_τ is estimated. Results in column (8) to (10) should be interpreted as follows: between the x-th and the $(x+10)$-th percentile of the distribution the estimated quantile treatment effect increases by .0114.

Table 7: BASELINE RKD ESTIMATES OF THE EFFECT OF POTENTIAL DURATION, LOUISIANA

	(1) Duration of Initial Spell	(2) Duration UI Claimed	(3) Duration UI Paid	(4) Age	(5) Years of Education	(6) Male	(7) Dependents
	Period 1: Jan 1979 - Jan 1980						
β	.216	.185	.222	-.107	.014	.004	-.013
	(.119)	(.12)	(.117)	(.167)	(.032)	(.006)	(.026)
p-value	.685	.596	.65	.163	.123	.519	.072
N	3107	3107	3107	3091	2839	3078	1952
Opt. Poly	1	1	1	1	1	1	1
	Period 2: Sep 1981 - Apr 1982						
β	.3	.299	.272	.071	.013	-.007	-.016
	(.103)	(.099)	(.099)	(.113)	(.024)	(.004)	(.025)
p-value	.593	.546	.488	.416	.118	.31	.427
N	2659	2659	2659	2644	2415	2624	951
Opt. Poly	1	1	1	1	1	1	1
	Period 3: Jun 1983 - Dec 1983						
β	.502	.456	.457	-.004	-.003	-.028	-.092
	(.087)	(.081)	(.084)	(.096)	(.025)	(.017)	(.082)
p-value	.746	.837	.747	.837	.492	.234	.264
N	1750	1750	1750	1738	1586	1731	935
Opt. Poly	1	1	1	1	1	2	2

Notes: Duration outcomes are expressed in weeks. β is the RK estimate of the average treatment effect of potential duration on the outcome. Standard errors for the estimates of α are in parentheses. P-values are from a test of joint significance of the coefficients of bin dummies in a model where bin dummies are added to the polynomial specification in equation 1. The optimal polynomial order is chosen based on the minimization of the Aikake Information Criterion.

Table 8: CYCLICAL BEHAVIOR OF THE RKD ESTIMATES OF THE EFFECT OF POTENTIAL DURATION

	(1)	(2)	(3)	(4)	(5)	(6)
	QUANTILE TREATMENT EFFECTS					
	β_τ	β_τ	β_τ	β_τ	$\varepsilon_{D,\tau}$	β_τ
τ	1.055***	0.950***				
	(0.103)	(0.109)				
$\tau \leq .2$			0.0706			
			(0.0658)			
$.2 < \tau \leq .5$			0.0751			
			(0.0594)			
$\tau > .5$			0.657***			
			(0.0430)			
$\tau \times (U > 7.5\%)$				0.819***	-0.464	
				(0.185)	(1.271)	
$\tau \times (U \leq 7.5\%)$				1.006***	2.766***	
				(0.128)	(0.909)	
$\tau \times (\frac{\dot{U}}{U} > .8\%)$						0.942***
						(0.117)
$\tau \times (\frac{\dot{U}}{U} \leq .8\%)$						0.965***
						(0.289)
State F-E		×	×	×	×	×
Inverse s-e weights	×	×	×	×	×	×
N	57	57	57	57	55	57

Standard errors in parentheses, * p<0.10, ** p<0.05, *** p<0.01

Notes: U is the average monthly state unemployment rate from CPS and $\frac{\dot{U}}{U}$ is the average monthly growth rate of unemployment in the state. $\tau \in [0,1]$ is the percentile of the distribution of unemployment duration at which the quantile treatment effect α_τ is estimated. Results in column (8) to (10) should be interpreted as follows: between the x-th and the $(x+10)$-th percentile of the distribution the estimated quantile treatment effect increases by .1055.