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Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design†

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I show how, in the tradition of the dynamic labor supply literature, one can identify the moral hazard effects and liquidity effects of unemployment insurance (UI) using variations along the time profile of unemployment benefits. I use this strategy to investigate the anatomy of labor supply responses to UI. I identify the effect of benefit level and potential duration in the regression kink design using kinks in the schedule of benefits in the US. My results suggest that the response of search effort to UI benefits is driven as much by liquidity effects as by moral hazard effects. (*JEL* D82, J22, J65)

Most social insurance and transfer programs have time-varying benefits, in the sense that the benefits received are a function of time spent in the program. Changing the generosity of these programs therefore involves affecting the time profile of benefits. It is now well understood, in particular in the context of unemployment insurance (UI), that labor supply responses to such variations in the time profile of benefits consist of a combination of liquidity effects and "moral hazard" effects. And that the dichotomy between the moral hazard effect and the liquidity effect of benefits is critical to assess the welfare impact of such social insurance and transfer programs (Shimer and Werning 2008; Chetty 2008). But, to date, the dichotomy has been of little practical interest because of the difficulty to disentangle these two effects empirically.[1]

The contribution of this paper is to propose a new strategy to estimate liquidity and moral hazard effects in the context of unemployment insurance. I show how the dichotomy between liquidity effects and moral hazard effects can be reinterpreted in light of the more traditional literature on dynamic labor supply, and how the moral

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[1] Apart from Chetty (2008), using variations in severance payments, and also LaLumia (2013), using variations in the timing of EITC refunds, there has been very few attempts to empirically estimate the magnitude of liquidity effects of social insurance programs.

hazard effect of UI on search effort can be related to the Frisch elasticity concept (i.e., the response of search effort to a change in benefits keeping marginal utility of wealth constant). Following the methodology of MaCurdy (1981), which relies on exploiting (exogenous) variations in the wage profile, keeping marginal utility of wealth constant, I propose a similar method to identify the moral hazard effects of UI using variations along the time profile of benefits brought about by exogenous variations in the benefit level as well as the benefit duration. Importantly, this strategy only relies on exploiting individuals' first-order conditions and variations in the time profile of benefits. It is, in this sense, very general, and can be applied to any other transfer program with time-dependent benefits.

I implement empirically this identification strategy, identifying the effect of both benefit level and potential duration in the regression kink (RK) design, using kinks in the schedule of UI benefits, following Card et al. (2012). I use administrative data from the Continuous Wage and Benefit History Project (CWBH) on the universe of unemployment spells in five states in the United States from the late 1970s to 1984.[2] 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. I provide compelling graphical evidence and find significant responses of unemployment and nonemployment duration with respect to both benefit level and potential duration for all states and periods in the CWBH data. I provide various tests for the robustness of the RK design, and assess its validity to overcome the traditional issue of endogeneity in UI benefit variations on US data. These tests include graphical and regression-based tests of the identifying assumptions as well as placebo tests and kink-detection and kink-location tests. I also use variations in the location of the kink over time to implement a difference-in-differences RK strategy to check the robustness of the results.

Overall, replicating the RK design for all states and periods, my results suggest that a 10 percent increase in the benefit level increases the duration of UI claims by about 4 percent on average, and that increasing the potential duration of benefit by a week increases the duration of UI claims by about 0.3 to 0.4 week on average. These estimates are higher than estimates found in European countries using sharp RD designs, but are still lower than previous estimates on US data. My results also suggest that the ratio of liquidity to moral hazard effects in the response of labor supply to a variation in unemployment benefits is around 0.9. This confirms the existence of significant liquidity effects as found in Chetty (2008). But interestingly, the identification strategy for moral hazard and liquidity effects proposed in this paper only uses administrative UI data and the RK design, and can therefore deliver timely estimates of liquidity effects without the need for data on consumption or on assets. I finally use these estimates to calibrate the welfare benefits of UI.

The remainder of the paper is organized as follows. In Section I, I present a simple dynamic model to show how the moral hazard effect can be identified using

[2] Records begin in January 1976 for Idaho, in January 1979 for Louisiana, January 1978 for Missouri, April 1980 for New Mexico, and July 1979 for Washington.

variations in the time profile of UI benefits, that, in practice, come from variations in both benefit level and potential duration. In Section II, I present the RKD strategy, the data, and provide with institutional background on the functioning of UI rules. In Section III, I present the results of the labor supply effects of benefit level and potential duration, and I present various tests for the robustness of the RKD estimates. Finally, in Section IV, I estimate the liquidity to moral hazard ratio of the effect of UI, and calibrate the welfare benefits of UI using my RKD estimates.

I. Relating Moral Hazard to Estimable Behavioral Responses

I show in this section how the dichotomy between liquidity effects and moral hazard effects can be reinterpreted in light of the more traditional literature on dynamic labor supply, and how one can use the insights from this literature to back out moral hazard effects from comparing the behavioral response of current search effort to variations in benefits at different points in time.

In a standard dynamic labor supply model, with time-separability, a change in the net return to work today has two effects on current labor supply. First, there is an effect due to the manipulation of the current return to work keeping marginal utility of wealth constant: this effect relates to the concept of Frisch elasticity. Second, there is a wealth effect due to the change in the marginal utility of wealth.[3] The "MaCurdy critique" (MaCurdy 1981) formulated against static reduced-form labor supply studies using tax reform variation builds on this simple argument. A permanent tax change dt will shift the whole net-of-tax wage profile as shown on the left-hand side of Figure 1, panel A, and the effect of such a tax change on labor supply should therefore be interpreted as a mix of wealth effect and "Frisch" effects.

Another important point of the standard dynamic labor supply literature is that any variation in the future returns to work only affects current labor supply through the marginal utility of wealth. An obvious corollary is that you can back out the wealth effects and the Frisch elasticity component by comparing the effect on current labor supply of a marginal change in the return to effort today versus that of an equivalent marginal change in return to effort in the future. This is the principle of the methodology used in MaCurdy (1981), which relies on exploiting (exogenous) variations in the wage profile, keeping marginal utility of wealth constant as shown on the right-hand side of Figure 1, panel A.

In the context of unemployment benefits, most countries have two-tier UI benefits systems, giving benefits b for a maximum period of B weeks, at which point UI benefits exhaust, and UI benefits are zero afterward. A change in the benefit level db received by the unemployed for the first B periods can be interpreted as a full shift of the profile of the returns to search effort, as in the left-hand side of Figure 1, panel B. Most studies exploiting variations in the benefit level b across individuals to analyze the effect of UI benefits on search effort therefore estimate a mix of wealth effects and of distortionary "Frisch" effects (moral hazard effects). This is the point explicitly made by Chetty (2008). The idea developed here is that one can use, as has been

[3] See online Appendix C.1 for a simple exposition of a standard dynamic labor supply model without state dependence, and how Frisch elasticities can be identified using variations in the wage profiles.

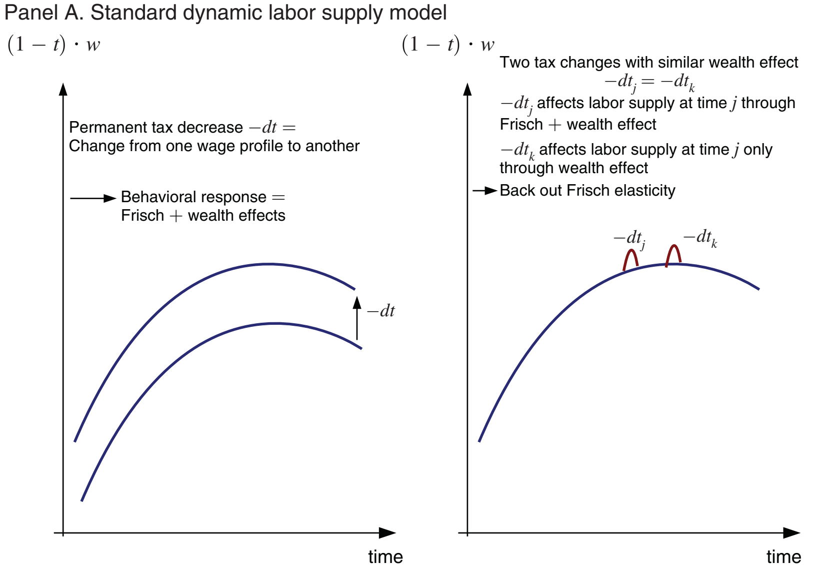

Figure 1. Backing Out Moral Hazard Effects in Dynamic Labor Supply Models

(*Continued*)

traditionally done in the dynamic labor supply literature, variations in the net return to search effort at different points in time in order to disentangle wealth effects from the moral hazard effects.[4] Such variation is brought about by variations in benefit level and in the potential duration of benefits as shown in the right-hand side of Figure 1, panel B. The only notable difference in the context of unemployment benefits is the presence of state dependence: search effort today affects in which state one ends up tomorrow. In other words, when increasing future benefits (through an increase in the potential duration B for instance), one only gets the higher benefits if still unemployed after B periods. Because of this, variations in future benefits do not only have an effect on current job search effort through the marginal utility of wealth, but also through the net return to search effort today.

To get the point across and explain the intuition of the main results, I only present a simplified two-period version of a partial equilibrium dynamic search model, a class of models that has been used extensively to analyze the welfare implications of UI benefits (Chetty 2008; Schmieder, von Wachter, and Bender 2012). Proofs and discussion for the multi-period model are in online Appendix C. The model describes the behavior of a worker who is laid-off and therefore becomes unemployed before the start of period zero. If the worker is unemployed at the start of period i, he exerts (endogenous) search effort s_i, which has a utility cost $\psi(s_i)$, with $\psi' \geq 0$ and $\psi'' \geq 0$. Search effort s_i translates into a probability to find a job[5] that

[4] Note also that if agents are totally credit constrained, or totally myopic, the dynamic dimension of the problem is irrelevant, and the effect of UI benefits is a mix of contemporaneous income effects and substitution effects, as in the static case. Identification of distortionary effects of UI would then simply require the use of contemporaneous income shocks to control for income effects.

[5] This captures the presence of search frictions in the labor market.

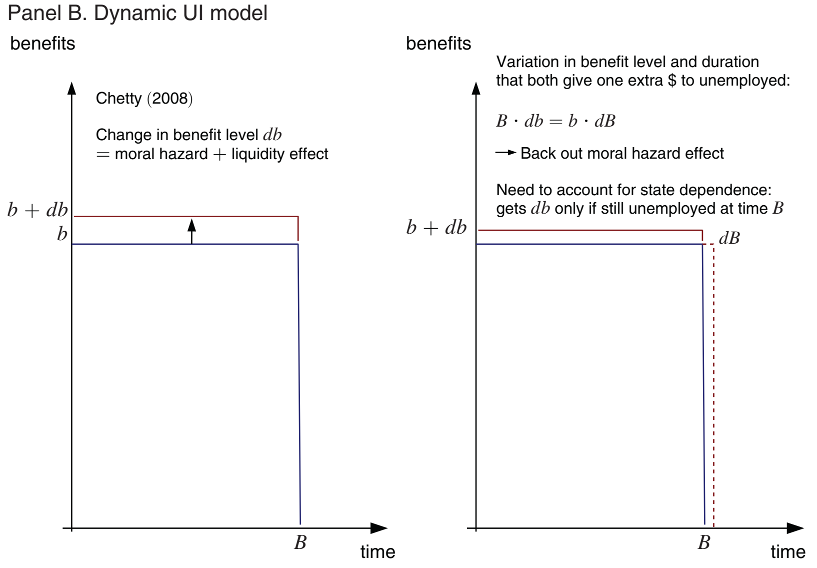

FIGURE 1. BACKING OUT MORAL HAZARD EFFECTS IN DYNAMIC LABOR SUPPLY MODELS (*Continued*)

Notes: The figure explains the decomposition of tax/UI benefits changes into wealth effects and moral hazard (or Frisch) effects, and the relationship between the "MaCurdy critique" (MaCurdy 1981) and the liquidity versus moral hazard decomposition of Chetty (2008). Panel A-left, shows the effect of a permanent tax change on the wage profile of an individual: the net return to work is affected every period, but so is the expected lifetime wealth of the individual. The behavioral response of labor supply to this tax change will be a mix of wealth and Frisch effects. In panel A-right, a marginal tax change at time j and a marginal tax change at time k will have a similar wealth effect on labor supply at time j, but the marginal tax change at time k will only affect labor supply at time j through the wealth effect. Comparing the effect of these two tax changes on labor supply at time j will therefore identify Frisch effects (MaCurdy 1981). Panel B plots a change in the benefit level db received by the unemployed for the first B periods in a two-tier UI benefits system. This change in benefit is a full shift of the profile of the return to search effort, as in panel A-left, and its effects on search effort will be a mix of wealth effects and of distortionary "Frisch" effects (or moral hazard effects, Chetty 2008). But the idea of exploiting variations in the net return to search effort at different points in time can also be implemented using variations in benefit level db and in the potential duration of benefits dB as shown in panel B-right. The only difference is the presence of state-dependence: search effort today affects in which state one will be tomorrow. When increasing potential duration dB, one only gets the higher benefits if still unemployed after B periods. Because of this, variations in future benefits do not only have an effect on current job search effort through the marginal utility of wealth, but also through the net return to search effort today. The difference in the effect of current and future benefits on search effort today only identifies the moral hazard effect up to a term that depends on the ex ante survival function, as shown in Proposition 1.

I normalize to s_i to simplify presentation.[6] If employed in period 0, the worker gets utility $u(c_0^e) = u(A_0 - A_1 + w_0 - \tau)$, where A_0 is the initial level of wealth and $u' \geq 0$; $u'' \leq 0$. w_0 is the wage rate (assumed exogenous) and τ is the payroll tax paid to finance UI benefits. If employed in period 1, the worker gets utility $u(c_1^e) = u(A_1 - \overline{A} + w_1 - \tau)$, where $\overline{A}$ is asset level at the end of period 1, subject

[6] We also assume that search effort is not observable from the social planner, and this is why we describe as "moral hazard" the distortions in search effort induced by UI benefits.

to the non-Ponzi condition $\overline{A} \geq 0$. We can also introduce liquidity constraints of the form $A_1 \geq L$, $\overline{A} \geq L$. If unemployed in period 0, the worker gets utility $u(c_0^u) = u(A_0 - A_1 + b_0)$, where b_0 are UI benefits in period 0. And if unemployed in period 1, the worker gets utility: $u(c_1^u) = u(A_1 - \overline{A} + b_1)$. Lifetime utility at the start of period 0 is given by

$$\mathcal{U} = s_0 u(c_0^e) + (1 - s_0)u(c_0^u) - \psi(s_0)$$
$$+ \beta\big(s_0 u(c_1^e) + (1 - s_0)\big(s_1 u(c_1^e) + (1 - s_1)u(c_1^u) - \psi(s_1)\big)\big),$$

where β is the discount factor, and we assume interest rates to be zero for simplicity. Maximizing utility with respect to search effort in period 0, s_0, yields the following first-order condition:

$$(1) \qquad \psi'(s_0) = \underbrace{u(c_0^e) + \beta u(c_1^e)}_{\text{Lifetime utility if employed in period 0}}$$
$$- \underbrace{\big(u(c_0^u) + \beta\big(s_1 u(c_1^e) + (1 - s_1)u(c_1^u) - \psi(s_1)\big)\big)}_{\text{Lifetime utility if unemployed in period 0}}.$$

This is the standard optimal intratemporal allocation rule where the marginal disutility of effort in period 0 equals the marginal return to effort in period 0, i.e., the lifetime utility of getting employment starting in period 0 minus the lifetime utility of staying unemployed in period 0.[7] From this intratemporal allocation rule we get that

$$(2) \qquad \frac{\partial s_0}{\partial b_0} = -\frac{u'(c_0^u)}{\psi''(s_0)} = \frac{\partial s_0}{\partial A_0} - \frac{\partial s_0}{\partial w_0}.$$

This decomposition, at the center of the argument in Chetty (2008) can be thought of as a standard dynamic decomposition of the effect of current returns to effort between a Frisch elasticity concept keeping marginal utility of wealth constant $\left(\frac{\partial s_0}{\partial w_0}\right)$, that from now on will be referred to as the moral hazard effect of UI benefits, and a wealth effect $\frac{\partial s_0}{\partial A_0}$.[8]

Individuals choose their consumption level every period once the result of the search process is realized. From their optimal choice we get the standard Euler conditions determining the optimal intertemporal allocation of consumption:

$$(3) \qquad u'(c_0^e) = \beta u'(c_1^e)$$

$$(4) \qquad u'(c_0^u) = \beta\big(s_1 u'(c_1^e) + (1 - s_1)u'(c_1^u)\big).$$

[7] In the absence of state-dependence (or in a static model), only $u(c_0^e)$ and $u(c_0^u)$ would appear in this first-order condition, and future wages would only affect current effort through the marginal utility of wealth (wealth effect). See online Appendix C for a simple example of a two-period labor supply model without state-dependence.

[8] I explain more in depth in online Appendix C.1 the comparison between this decomposition and the one obtained in a standard model without state dependence.

Using (1), (3), and (4), we can retrieve the simple relationship between the effect of current and future wages on current effort:

$$\frac{\partial s_0}{\partial w_1} = (1 - s_1) \cdot \frac{\partial s_0}{\partial w_0}. \tag{5}$$

The intuition for this relationship, which stems directly from the presence of state dependence, is simply that increasing wages tomorrow induces me to search more today to benefit from the extra consumption tomorrow if I am employed at the start of the period, but at the same time, I can delay search until tomorrow and find a job tomorrow with probability s_1 to benefit from the extra wages tomorrow. The effect of increasing the net reward from work tomorrow on search effort today is therefore s_1 percent smaller than the effect of increasing wages today on search effort today.[9] And if $s_1 = 1$, then I will be employed with certainty in period 1, irrespective of my search effort in period 0, therefore changes in the wage rate in period 1 will have no effect on my search effort in period 0 in this case.

Using (5), and Euler conditions (3) and (4), a change in b_1 can therefore be decomposed as:

$$\frac{\partial s_0}{\partial b_1} = -\beta \frac{(1 - s_1)u'(c_1^u)}{\psi''(s_0)} = \frac{\partial s_0}{\partial A_0} - (1 - s_1)\frac{\partial s_0}{\partial w_0}. \tag{6}$$

And therefore we have that

$$\frac{\partial s_0}{\partial b_0} - \frac{\partial s_0}{\partial b_1} = -s_1 \cdot \frac{\partial s_0}{\partial w_0}. \tag{7}$$

In a model with no state dependence, the effect of future benefits would give us the wealth effect directly, but here, because of state dependence, the effect of future benefits on current search effort is larger in absolute value than the pure wealth effect, as shown in equation (6), since the change in future benefits also affects the net return to effort in the current period. Then the difference between the effect of current and future returns, which would give us the Frisch elasticity directly as in MaCurdy (1981) in the absence of state dependence, here gives us s_1 times the

[9] The best way to understand this result is to rewrite lifetime budget constraint:

$$A_0 + s_0(w_0 - \tau) + (1 - s_0)b_0 + s_0(w_1 - \tau) + (1 - s_1)s_0(w_1 - \tau) + (1 - s_0)(1 - s_1)b_1 \geq \mathcal{C}_0 + \mathcal{C}_1$$
$$A_0 + b_0 + b_1 + s_0 \underbrace{[\Delta c_0 + (1 - s_1)\Delta c_1]}_{\text{Price of effort at time 0}} + s_1 \underbrace{[\Delta c_1]}_{\text{Price of effort at time 1}} \geq \mathcal{C}_0 + \mathcal{C}_1,$$

where $\Delta c_0 = (w_0 - \tau - b_0)$ and $\Delta c_1 = (w_1 - \tau - b_1)$. In other words, by exerting effort at time 0, your reward is the extra money Δc_0 you gain in period 0 compared to remaining unemployed plus the extra money you earn tomorrow $(1 - s_1)\Delta c_1$ because you will enter period 1 as employed. The reason your return for tomorrow is $(1 - s_1)\Delta c_1$ and not simply Δc_1 is because you could also have had Δc_1 by exerting effort tomorrow instead and therefore get Δc_1 with probability s_1. In other words, altering the total price of effort at time 0 by dw_0 or by $(1 - s_1)dw_1$ is equivalent, and should have the same effect on effort at time 0. Hence the result that $\frac{\partial s_0}{\partial w_1} = (1 - s_1) \cdot \frac{\partial s_0}{\partial w_0}$.

moral hazard, because the effect of benefits tomorrow also contains a moral hazard dimension; but we know that this moral hazard component is s_1 percent smaller than the moral hazard component of today's benefits. In other words, variations in search effort brought about by changes in the profile of benefits contains a lot of information, but one needs to take explicitly the state-dependence dimension of the dynamic problem to retrieve parameters that are meaningful for welfare analysis.

The strategy used in this paper to identify the moral hazard effects of UI relies on the use of variations along the time profile of benefits brought about by exogenous variations on both benefit levels and potential benefit duration in the UI system. Proposition 1 generalizes the insight of (7) to a multi-period case where variations in b_0 and b_1 from the two period model are now replaced by variations in benefit level b and potential duration B. As in the two-period model, a change in benefits today due to an increase in the benefit level b affects search effort today through a liquidity and a moral hazard effect. A change in benefits tomorrow because of a benefit extension also affects search effort today through a liquidity effect and through a moral hazard effect because of state dependence. As shown in Figure 1, panel B, a benefit-level increase or a benefit extension will give the same dollar increment in liquidity to unemployed individuals when $B\partial b = b\partial B$. This explains why, compared to (7), $\frac{\partial s_0}{\partial b_0}$ now becomes $\frac{1}{B}\frac{\partial s_0}{\partial b}$ in Proposition 1, and $\frac{\partial s_0}{\partial b_1}$ becomes $\frac{1}{b}\frac{\partial s_0}{\partial B}$. Proposition 1 simply uses the fact that the liquidity effects of the same dollar increment in a benefit-level increase and in a benefit extension are equal, so that the difference in the effects on search effort at time 0 of a benefit-level increase and of a benefit extension can identify the moral hazard effect.

PROPOSITION 1: *If the borrowing constraint does not bind after B periods, the moral hazard effect Θ_1 of providing UI benefits b for B periods is a linear combination of the effects on exit rate at the start of a spell of an increase in benefit duration $\left(\frac{\partial s_0}{\partial B}\right)$ and of an increase in benefit level $\left(\left.\frac{\partial s_0}{\partial b}\right|_B\right)$*

$$\frac{1}{B}\left.\frac{\partial s_0}{\partial b}\right|_B - \frac{1}{b}\frac{\partial s_0}{\partial B} = -\frac{\overline{S_1^B} - S_1(B)}{D_1^B}\cdot\Theta_1, \tag{8}$$

where $S_1(B)$ is the survival rate at time B conditional on being unemployed at period 1; *$\overline{S_1^B}$ is the average survival rate between time* 1 *and time B conditional on being unemployed at period* 1; *and D_1^B is the average duration of covered UI spells conditional on being unemployed at time* 1.

PROOF:
See online Appendix C.

To understand the intuition behind Proposition 1 it is useful to compare it to the standard dynamic labor supply. In this case, there is no state dependence, and giving one extra dollar of wealth today or tomorrow through an increase in the wage rate has the same wealth effect on labor supply today, so that the difference in the behavioral response of search effort today to a change in the wage rate today and tomorrow washes out the wealth effect, and only the moral hazard or Frisch effect remains. In the presence of state dependence, search effort today affects in which state one will be tomorrow. In other words, when increasing potential duration dB, one only gets the higher benefits if still unemployed after B periods. In this case, the difference in the effect of current and future benefits on search effort today only identifies the moral hazard effect up to a term that depends on the ex ante survival function, as shown in Proposition 1.

Heterogeneity: An interesting aspect of Proposition 1 is that it can be generalized to allow for the presence of heterogeneity. The reason for this generalizability is that proposition 1 is only making use of individual optimality conditions. Suppose the economy has N individuals, indexed by i, and, for simplicity, let us focus back on the two-period case. Denote $E\left[\frac{\partial s_0}{\partial b_0}\right] = \frac{1}{N}\sum_{i=1}^{N}\frac{\partial s_0^i}{\partial b_0}$ the mean response of search effort in period 0 to a change in benefit at time 0, and $E\left[\frac{\partial s_0}{\partial b_1}\right] = \frac{1}{N}\sum_{i=1}^{N}\frac{\partial s_0^i}{\partial b_1}$ the mean response of search effort in period 0 to a change in benefit at time 1. Then, $E\left[\frac{\partial s_0}{\partial b_0}\right] - E\left[\frac{\partial s_0}{\partial b_1}\right] = E\left[\frac{\partial s_0}{\partial b_0} - \frac{\partial s_0}{\partial b_1}\right] = E\left[s_1\frac{\partial s_0}{\partial w_0}\right]$, where we only use individual first-order conditions regarding consumption and search effort. If heterogeneity is such that the distribution of optimal effort s^i and $\frac{\partial s_0^i}{\partial w_0}$ are independent, then we have $E\left[\frac{\partial s_0}{\partial b_0}\right] - E\left[\frac{\partial s_0}{\partial b_1}\right] = \bar{s}_1 \cdot E\left[\frac{\partial s_0}{\partial w_0}\right]$, where $\bar{s}_1 = \sum_{i=1}^{N}\frac{s_1^i}{N}$ is the average hazard rate in period 1. Note, however, that the independence of the optimal effort level and the marginal effect of w_0 on optimal effort can actually be a fairly strong assumption depending on the type of heterogeneity one considers. If heterogeneity was in parameters related to risk preferences, for example, this would most certainly not be true, and a covariance term would kick in that would also need to be estimated.[10]

Empirically, this means that the difference between the average behavioral response of search effort of the unemployed in period 0 to a change in benefits in period 0 versus a change in benefits in period 1 can be related to the average moral hazard effect of UI benefits in period 0 $E\left[\frac{\partial s_0}{\partial w_0}\right]$, and by extension, to the average liquidity effect of UI benefits $E\left[\frac{\partial s_0}{\partial A_0}\right]$. And as shown in Chetty (2008), the ratio of the average moral hazard effect to the average liquidity effect is a sufficient statistic for the optimal level of UI benefit in the presence of heterogeneity. In other words, even in the presence of heterogeneity, the difference between the average behavioral responses of search effort to variations in UI benefits at a different point in time reveals all the relevant information for the Baily formula.

Stochastic Wage Offers: The result of Proposition 1 can also be extended to the presence of stochastic wage offers, whereby an agent's hazard rate out of

[10]Note that Andrews and Miller (2014) have a similar discussion on heterogeneity and sufficient statistics in the context of UI.

unemployment would depend both on her search effort and her reservation wage. Suppose that in period t with probability s_t (controlled by search intensity) the agent is offered a wage $w \sim \hat{w} + F(w)$, and assume independent and identically distributed wage draws across periods. In such a framework (McCall 1970), the agent follows a reservation-wage policy: in each period, there is a cutoff R_t such that the agent accepts a job only if the wage $w > R_t$. I show in online Appendix C.6 that the result of Proposition 1 remains unchanged in this context because the agent is setting her reservation wage profile optimally, so that the envelope theorem applies and there is no first-order effect of a change in reservation-wage policy on the agent's expected utility. In the two-period case, formula (7) becomes

$$\frac{\partial s_0}{\partial b_0} - \frac{\partial s_0}{\partial b_1} = -h_1 \frac{\partial s_0}{\partial w_0}, \tag{9}$$

where $h_1 = s_1 P[w \geq R_1]$ is the hazard rate out of unemployment[11] in period 1, and $P[w \geq R_1]$ is the probability that the wage offered in period 1 is larger than the reservation wage in period 1 R_1.

Relationship with Optimal UI Formula: The importance of isolating moral hazard from liquidity effects lies in the fact that they reveal critical information about the consumption smoothing benefits of UI, and as a consequence about the welfare effects of UI. The ratio of moral hazard to liquidity effects is actually directly proportional to the risk aversion parameter $\left(c \cdot \frac{u''}{u'}\right)$ and therefore to the consumption smoothing benefits of UI. The intuition for this is the following. First, the moral hazard effect of UI (ds/dw) is proportional to u': the larger the marginal benefit of a dollar, the more the agent's search effort will react to a $1 increase in her wage rate. Second, the liquidity effects (ds/dA) are proportional to u'': when u'' is large, if wealth falls, u' rises sharply, and individuals will exert a lot of effort to find a job. Therefore, the consumption smoothing benefits of UI, which constitute the left-hand side of the traditional Baily formula can be recast in terms of the ratio of moral hazard to liquidity effects. Chetty (2008) shows how to obtain this modified Baily formula to calibrate the optimal benefit level for a constant duration, and I show in online Appendix C that a similar formula can be obtained to calibrate the optimal duration of benefit for a given benefit level. Armed with these modified formulas for the optimal benefit level and optimal benefit duration, and using Proposition 1, it becomes possible to evaluate the welfare impact of local policy reforms using only responses of search effort to variations in the time profile of unemployment benefits, and without estimation of the full underlying structural model.

To fully implement the proposed strategy, and calibrate optimal formula for UI level (resp. benefit duration) I need to estimate three statistics: the elasticity of the duration of the paid unemployment spell with respect to benefit level (resp. benefit

[11] The only difficulty lies in defining the empirical counterparts for the implementation of formula 9, as changes in empirically observed job finding hazards cannot be directly used to infer the relevant changes in search intensity because part of the change in job finding hazards comes from changes in the reservation wage. I give two options for empirical implementation in online Appendix C.6.

duration), the elasticity of the duration of the total nonemployment spell with respect to benefit level (resp. benefit duration), and the ratio of the liquidity effect to moral hazard effect of an increase in benefit level (resp. benefit duration). In the empirical implementation, I begin by estimating the two elasticities. To estimate the ratio of moral hazard to liquidity effects, I estimate the effect of a change in benefit level on the hazard rate at the start of the spell $\left.\frac{\partial s_0}{\partial b}\right|_B$ and the effect of a change in potential duration on the hazard rate at the start of the spell $\left.\frac{\partial s_0}{\partial B}\right|_b$. I then use Proposition 1 to get the moral hazard effect Θ_1 of providing UI benefits b for B periods. Finally, I use the fact that the behavioral response $\left.\frac{\partial s_0}{\partial b}\right|_B$ is the sum of the liquidity effect $\left(\left.\frac{\partial s_0}{\partial a}\right|_B\right)$ and of the moral hazard effect Θ_1 (see online Appendix C for details) to back out the liquidity effect and compute the ratio of liquidity to moral hazard effects.

Pros and Cons of the Proposed Method: The obvious advantage of the proposed method to estimate moral hazard and liquidity effects is that it can be done from estimation of search responses only. Proposition 1 relates the structural approach of dynamic models to behavioral responses of search effort that can be estimated in reduced-form using credibly exogenous variations in both benefit levels and potential durations for the same individuals. And as a consequence, welfare effects of UI can be assessed without any direct estimation of the consumption smoothing benefits of UI from consumption data, which can prove arduous. Given the "local"[12] nature of the Baily-Chetty formula, the components of the welfare formula need to be statistics that can be easily estimable, and preferably at high frequency, to be able to make readily available policy recommendation. The interest of the proposed method is that, as will become apparent in the empirical sections of the paper, all the relevant statistics for welfare analysis are estimable with administrative UI data at high frequency using the regression kink design.

The method of Proposition 1 to uncover the moral hazard component of behavioral responses relies on individuals' optimality conditions, and in particular on the Euler equations. A key advantage of this approach is that it does not require any knowledge about individuals' risk aversion or discount factors. In practice though, it is therefore important to test the assumption that the credit constraint is not yet binding after B periods so that the Euler equations actually hold. In Section A.8, I provide a simple test of this assumption using post-exhaustion behavior with administrative data. More fundamentally, the method proposed here to identify moral hazard and liquidity effects relies on the assumption that the unemployed are rational and forward-looking. If individuals were perfectly myopic for instance, the Euler equation would not hold. The test about the slackness of the liquidity constraint seems to indicate a certain degree of consumption smoothing over time, ruling out perfect myopia. But evidence in the labor market (see for instance DellaVigna and Paserman 2005) indicates that job seekers may exhibit a lot of impatience. Even though our identification strategy is valid independently of the value of the discount factor,

[12] Local here means in the neighborhood of the actual policy parameters, where the statistics entering the formula are estimated.

it rules out the possibility of forms of impatience, such as hyperbolic (beta-delta) discounting.

My identification strategy also necessitates that individuals have very precise information about their benefit level and potential duration of UI. This seems to be the case nowadays, unemployed individuals receiving, in most states at the beginning of their claim, a summary of their rights, with the amount of their weekly benefits and total duration of benefits in weeks.[13] Finally, my identification strategy postulates that unemployed individuals are able to form rational expectations about their survival rates and expected duration of unemployment at the start of a spell. Evidence in the labor market also suggests that unemployed individuals may actually exhibit biased perceptions about their unemployment risks (Spinnewijn 2010). It is unfortunately difficult to know to what extent such biased beliefs are likely to affect my estimates, since the moral hazard estimate is at the same time an increasing function of the expected duration of unemployment and a decreasing function of the expected survival rate at exhaustion. In other words, biased beliefs would not affect my estimate if the bias is a simple shifter of the survival curve. If this is not the case, one would need to compare the full (biased) expected survival curve to the true survival curve to know how these biased perceptions affect the moral hazard and liquidity estimates.

II. Empirical Implementation

The empirical challenge in applying the formula of Proposition 1 lies in the difficulty to find credibly exogenous and time invariant sources of variations in UI benefits. Most sources of variations used in the literature on US data come from changes in state legislation over time,[14] with the issue that these changes might be endogenous to labor market conditions. In this paper, I use the presence in most US states of kinked schedules in the relationship between previous earnings and both benefit level and benefit duration to estimate the responses of labor supply to UI benefits, using administrative data on UI recipients. This strategy has several important advantages. First, in contrast to studies using regional or time variation in UI benefits, the RK design holds market-level factors constant, such that I identify changes in the actual behavioral response, net of any market-level factors that may change over time or across regions. Second, the RK design allows me to identify behavioral responses with respect to both benefit level and potential duration for the same workers in the same labor markets. Finally, my empirical strategy, based on the use of administrative data, delivers high-frequency estimates of behavioral responses without the need for quasi-experimental policy reforms, which is critical for welfare recommendations based on sufficient statistics formula.

[13] Unfortunately, I was not able to find a copy of the UI benefit summary for the period covered by the CWBH, and could not confirm that such information was already present at the time.

[14] See for instance Meyer (1990) or Card and Levine (2000).

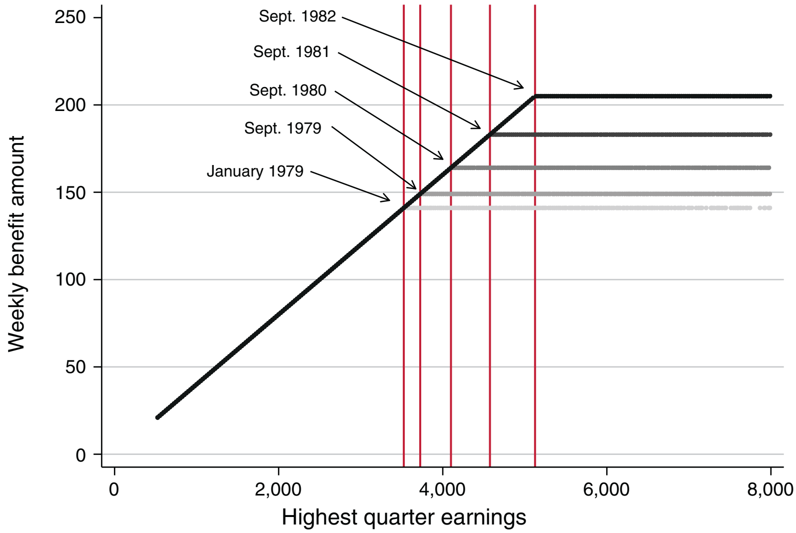

FIGURE 2. LOUISIANA: SCHEDULE OF UI WEEKLY BENEFIT AMOUNT, JAN. 1979–DEC. 1983

Notes: The graph shows the evolution of the schedule of the weekly benefit amount (WBA) in nominal terms 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.

Source: Louisiana Revised Statutes RS 23:1592 and yearly *Significant Provisions of State Unemployment Insurance Laws* 1976 to 1984, Department of Labor, Employment, and Training Administration

A. *Institutional Background: Kinks in UI Schedules*

In all US states, the weekly benefit amount b received by a compensated unemployed is a fixed fraction τ_1 of her highest earning quarter (hqw) in the base period (the last four completed calendar quarters immediately preceding the start of the claim)[15] 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 used in this paper. Note that the maximum benefit amount has been increased several times in Louisiana, partly to adjust to high inflation rates during the period.[16] 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

[15] Some states, such as Washington, use the average of the two highest earning quarters in the base period.

[16] Inflation was 13.3 percent in 1979, 12.5 percent in 1980, 8.9 percent in 1981, 3.8 percent in 1982 (source: BLS CPI data).

equal to $1/25$ which guarantees a constant replacement ratio of 52 percent of the highest earning quarter up to the kink, where the replacement ratio decreases.

The potential duration of benefits (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[17]

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

Duration is thus also a deterministic kinked function of previous earnings,[18] as shown in Figure 3. All the details on the rules pertaining to the kinks in potential duration are described in online Appendix D.7. 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 potential duration of UI benefits. The first program is the permanent standby Extended Benefit (EB) program, federally mandated but administered at the state level (Tier II). On top of the EB program, federal extensions are usually enacted during recessions (Tier III). These extensions may change the location and size of the kink in the relationship between previous earnings and benefit duration as shown in Figure 3 in the case of Louisinia. Most importantly, benefit extensions create nonstationarity in the

[17] Idaho is the only state in the CWBH data with different rules for the determination of benefit duration.

[18] To give a concrete example, an unemployed individual in Louisiana during the period 1979 to 1983 will hit the maximum duration whenever her ratio of base period earnings to highest quarter of earnings is larger than 2.8. An individual with a highest quarter of earnings of \$3,725 in 1979, for instance, who is therefore hitting the maximum benefit amount ceiling will see her potential duration increase by roughly 0.25 week for each additional \$100 of base period earnings, up to the point where her base period earnings is larger than \$10,430, at which point she will be hitting the maximum duration ceiling of 28 weeks. Note also that the schedule of benefit level and benefit duration are related. In particular, if $\frac{bpw}{min\left(hqw, \frac{b_{max}}{\tau_1}\right)} \leq D_{max} \cdot \frac{\tau_1}{\tau_2}$, then $D = \tau_2 \cdot \frac{bpw}{min(\tau_1 \cdot hqw, b_{max})}$, so that 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. To deal with the issue, I always get rid of all individuals with $\frac{bpw}{min\left(hqw, \frac{b_{max}}{\tau_1}\right)} \leq D_{max} \cdot \frac{\tau_1}{\tau_2}$ when estimating the effect of benefit level, to avoid the correlation between the location of the two kinks. I explain in detail in online Appendix D.7 how to deal with the correlation between the two schedules, for all the various subcases.

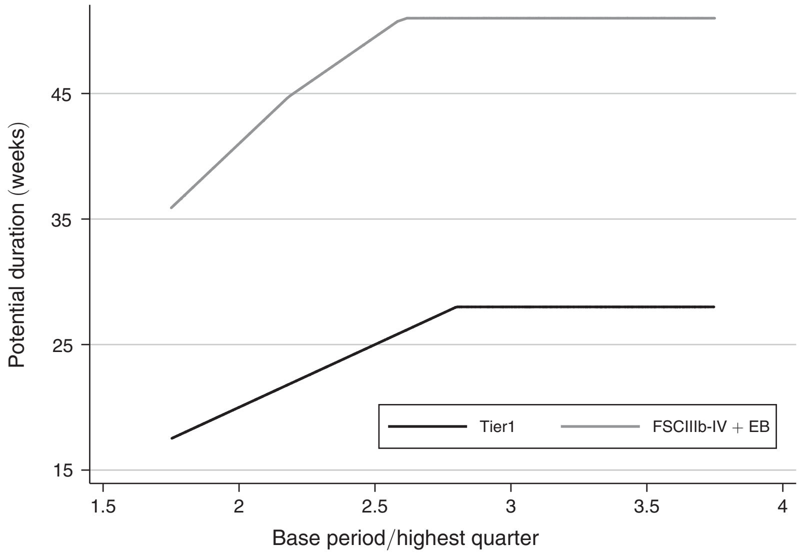

Figure 3. Louisiana: Schedule of UI Potential Duration, Jan. 1979–Dec. 1983

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. In normal times, the potential duration is determined by the regular state UI program (Tier 1). During recessions, and conditional on states' labor market conditions, two additional UI programs (Extended Benefit program and Federal extensions) may extend the potential duration over the maximum duration of Tier 1 which may affect the size and location of the kink. The graph shows for instance the schedule applying during most of 1983 when both the EB and Federal extensions (FSC-III and FSC-IV) were in place in Louisiana.

Source: Louisiana Revised Statutes RS 23:1592 and weekly state trigger notice reports

potential duration of benefits over the duration of a spell, which creates an additional challenge for inference in the RK design, as I discuss in Section IIIB.

B. *Data*

The data used is from Continuous Wage and Benefit History (CWBH) UI records.[19] This is the most comprehensive, publicly available administrative UI dataset for the United States. CWBH data contains the universe of unemployment spells and wage records for five US states from the late 1970s to 1984. Records begin in January 1976 for Idaho, in January 1979 for Louisiana, January 1978 for Missouri, April 1980 for New Mexico, and July 1979 for Washington.[20] This enables me to replicate and successfully test for the validity of the RK design in many different settings and labor market conditions. Two important advantages of the data are worth noting. First, CWBH data provides accurate information on the level of benefits, potential duration, previous earnings, and work history over time. Given

[19] I am especially grateful to Bruce Meyer and Patricia M. Anderson for letting me access the CWBH data.
[20] For all details on the CWBH dataset, see for instance Moffitt (1985a).

the large degree of measurement error found in survey data, administrative data like the CWBH are the only reliable source to implement identification strategies such as the regression kink design.[21] Second, 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.

I report in Table 1 descriptive statistics for the CWBH sample used in my RKD strategy for all five states. In terms of duration outcomes,[22] I focus on four main outcomes: the duration of paid unemployment, the duration of claimed unemployment, the duration of the initial spell as defined in Spiegelman, O'Leary, and Kline (1992),[23] and the duration of total nonemployment. Note that the latter can only be properly computed in Washington, which is the only state where the wage records, matched to the UI records, contain information about reemployment dates.

Table 1 also reveals large variation in the generosity of UI benefits across states. The average weekly benefit level (in \$2010) varies from \$225 in Missouri to \$305 in Louisiana, while the average potential duration varies from 20 weeks in Idaho to 27 weeks in Washington. These differences are due to variations in the parameters of the schedule (the maximum benefit amount, τ_1, etc.). For the purpose of the RKD estimation, this has the advantage of creating substantial variation in the location of the kink (relative to the distribution of earnings) across states: the ratio of the kink point to the average *hqw* varies from 0.98 in Missouri to 1.65 in Louisiana, with a fraction of unemployed at the maximum benefit amount varying from 0.64 to 0.35. This mitigates the concern that RKD estimates are just picking a functional form dependence between the outcome of interest and the running variable that would be consistent across states.

In terms of external validity, it is interesting to note that the overall structure of the UI system has remained almost unchanged since the period covered by the CWBH. The slope of the UI schedule has remained the same in almost all US states over the past 30 years. The generosity of the UI system has only been affected by the evolution of the other parameters of the schedule, and in particular of the maximum benefit amount. Some states, such as Louisiana, are less generous today than they are in the CWBH data: the average replacement rate is 0.47 in the CWBH data, while it is around 0.395 in 2012.[24] But overall, with average replacement rates ranging between 0.43 and 0.47 across states, the generosity of UI benefits in the CWBH data is very similar to today's, with an average replacement rate of 0.466

[21] Administrative data was also supplemented by a questionnaire given to new claimants in most states participating in the CWBH project, 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.

[22] UI claims are observed at weekly frequencies in the administrative data so that all duration outcomes are measured and expressed in weeks.

[23] 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, and second, because a lot of spells exhibit interruptions in payment with the claimant not collecting any check for a certain number of weeks without being observed in the wage records. The initial spell, as defined in Spiegelman, O'Leary, and Kline (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.

[24] The replacement rate is defined as the weekly benefit amount divided by the weekly wage in the highest quarter of earnings. The figures for recent state UI replacement rates come from the Department of Labor and can be found at http://workforcesecurity.doleta.gov/unemploy/ui_replacement_rates.asp.

Table 1—Descriptive Statistics for Full CWBH Sample

	Idaho			Louisiana		
	Mean	SD	Observations	Mean	SD	Observations
	Duration outcomes (weeks)					
Initial spell	13.8	12.3	33,125	17.5	14.1	44,702
Wks UI paid	11.7	10.7	33,125	17.3	13.8	44,702
Wks UI claim	15.8	12.2	33,125	18.7	13.7	44,702
	Earnings and benefits ($2010)					
bpw	25,163	22,227	33,125	26,894	19,029	44,702
hqw	9,835	16,463	33,125	9,538	6,228	44,702
wba	261.8	86.3	33,125	305.2	115.8	44,702
Pot. duration Tier I	20	5.5	33,125	24.9	4.3	44,702
Kink/avg. *hqw*	1.44	0.9	33,125	1.65	1.35	44,702
Percent with max. *b*	0.37	0.48	33,125	0.35	0.48	44,702
Percent with max. *D*	0.31	0.46	33,125	0.64	0.48	44,702
Average repl. rate	0.44	0.12	33,125	0.47	0.09	44,702
Exhaustion rate	0.11	0.29	33,125	0.13	0.31	44,702
	Covariates					
Age	30.2	12.8	33,121	34.6	12.6	44,373
Male	0.67	0.47	33,121	0.7	0.46	44,058
Educ. (yrs)	12	2.2	17,627	11.4	2.7	41,308
Dependents	2	1.6	18,688	2.1	1.6	22,525
	Missouri			New Mexico		
	Mean	SD	Observations	Mean	SD	Observations
	Duration outcomes (weeks)					
Initial spell	12.2	10.9	28,599	14	12.6	27,004
Wks UI paid	12.5	11.4	28,599	13.4	12.8	27,004
Wks UI claim	15.4	11.8	28,599	15.8	12.6	27,004
	Earnings and benefits ($2010)					
bpw	23,756	17,346	28,599	23,334	17,132	27,004
hqw	8,218	5,835	28,599	8,252	5,382	27,004
wba	224.9	51.4	28,599	230	69.5	27,004
Pot. duration Tier I	22.1	5.2	28,599	25.7	1	27,004
Kink/avg. *hqw*	0.98	0.74	28,599	1.3	0.8	27,004
Percent with max. *b*	0.64	0.48	28,599	0.43	0.5	27,004
Percent with max. *D*	0.52	0.5	28,599	0.92	0.27	27,004
Average repl. rate	0.45	0.16	28,599	0.43	0.11	27,004
Exhaustion rate	0.18	0.37	28,599	0.14	0.32	27,004
	Covariates					
Age	34.8	12.7	28,585	33.7	11.4	26,924
Male	0.61	0.49	28,597	0.65	0.48	27,002
Educ. (yrs)	11.3	2.2	1,852	11.7	2.5	26,482
Dependents	2	1.6	21,701	2.2	1.7	25,534

(*Continued*)

in the United States in 2012. This means that the location of the kink in the distribution of earnings is roughly similar today to that in the CWBH data. The only notable difference concerns the tax status of UI benefits. Prior to 1979, UI benefits were not subject to Federal income taxation, but in 1979 they became taxable for

Table 1—Descriptive Statistics for Full CWBH Sample (*Continued*)

	Washington		
	Mean	SD	Observations
	Duration outcomes (weeks)		
Initial spell	17.6	15.4	41,992
Wks UI paid	16.2	14.8	41,992
Wks UI claim	18.9	15.4	41,992
Wks nonemployed	27.9	16.3	38,035
	Earnings and benefits ($2010)		
bpw	31,232	20,380	41,992
hqw	8,982	5,321	41,992
wba	286.7	94.7	41,992
Pot. duration Tier I	27	4.2	41,992
Kink/avg. *hqw*	1.49	1.2	41,992
Percent with max. *b*	0.37	0.48	41,992
Percent with max. *D*	0.56	0.5	41,992
Average repl. rate	0.47	0.21	41,992
Exhaustion rate	0.12	0.31	41,992
	Covariates		
Age	34.2	11.9	41,955
Male	0.627	0.484	41,972
Educ. (yrs)	12.4	2.4	41,702
Dependents	1.7	1.5	28,834

Notes: The initial spell, as defined in Spiegelman, O'Leary, and Kline (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.

high-income individuals, and in 1987 benefits became taxable for all recipients. It is finally interesting to note that the composition of the UI recipients in the CWBH is relatively close to that of UI recipients during the Great Recession as can be seen for instance from table 2.1 in Krueger and Mueller (2014).

C. *Regression Kink Design*

To identify the effect of UI benefit level and UI potential duration on search outcomes, I use the kinks in the schedule of UI benefits following a *sharp* RK design.[25] Identification relies on two assumptions. First, the direct marginal effect of the assignment variable on the outcome should be smooth. Second, density of the unobserved heterogeneity should evolve smoothly with the assignment variable at the kink. This local random assignment condition seems credible in the context of UI as few people may know the schedule of UI benefits while still employed.[26]

[25] Recently there has been a considerable interest for RK designs in the applied economics literature. References include Nielsen, Sørensen, and Taber (2010); Card et al. (2012); Dong (2010); or Simonsen, Skipper, and Skipper (2010). The term *sharp* RK design means that everyone is a complier and obeys the same treatment assignment rule.

[26] Unfortunately, apart from anecdotal evidence, there is very little data on individuals' information on UI schedules in order to fully substantiate this point.

Moreover, to be able to perfectly manipulate 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,[27] and to optimize continuously not only one's highest earning quarter but also the ratio of base period earnings to the highest earning quarter. In the next section, I provide further empirical evidence in support of the RKD assumptions.

As explained in Card et al. (2012), the denominator of the RKD estimand is deterministic,[28] so that RKD estimation only relies on the estimation of the numerator of the estimand, which is the change in the slope of the conditional expectation function of the outcome given the assignment variable at the kink. This can be done by running parametric polynomial models of the form

$$E[Y|W = w] = \mu_0 + \left[\sum_{p=1}^{\bar{p}} \gamma_p (w-k)^p + \nu_p (w-k)^p \cdot D\right] \quad \text{where} \quad |w-k| \leq h, \tag{10}$$

where W is the assignment variable; $D = \mathbb{1}[W \geq k]$ 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.

Note that the United States is characterized by relatively low take-up rates of UI. Incomplete take-up may affect the validity of RK design if it causes the random local assignment assumption to be violated. The RKD requires that the presence of incomplete take-up does not generate a non-smooth relationship between the assignment variable and unobserved heterogeneity at the kink point. This requirement is more likely to be met if some components of take-up are orthogonal to the assignment variable. Empirical evidence from the CWBH period partly supports this assumption. Blank and Card (1991) for instance show that unionization had a large impact on take-up, which suggests that lack of information/ignorance stories played an important role in take-up behaviors in the 1980s. Note also that because we only observe individuals who take-up UI in the CWBH data, the RKD estimates should be interpreted as a treatment effect on the treated and not as an Intention-To-Treat effect, in the sense that a change in the generosity of the schedule may affect the selection of individuals in the CWBH sample.

III. Effect of UI Benefits on Unemployment Duration

In this section, I present results of the estimation of the effect on unemployment duration of both UI benefit level and UI potential duration. The objective of this section is also to assess the validity of the RK design to estimate these elasticities. I propose and run several tests aimed at assessing both the validity of the identifying assumptions, and the robustness of the RK estimates.

[27] As shown in Figures 2 and 3, the schedule changes rather frequently.

[28] It is the change in the slope of the schedule at the kink.

A. *Benefit Level*

In the baseline analysis, I divide for each state all the unemployment spells in subperiods corresponding to stable UI schedules. In Figures 4, 5, and 6, and in the robustness analysis of Table A1, I group unemployment spells over all periods, which has the advantage of providing a larger number of observations at the kink for statistical power. For exposition purposes, I focus mainly on the case of Louisiana but all the results for all states and periods are displayed in online Appendix B.

Graphical Evidence: I begin by showing graphical evidence in support of the RKD assumptions. First, I plot the probability density function of the assignment variable in order to detect potential manipulation of the assignment variable at the kink point. Figure 4, panel A shows the number of spells observed in each bin of the highest quarter of earnings normalized by the kink point[29] in Louisiana. The graph shows 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 tests as is standard in the Regression Discontinuity Design literature. The estimate for the log change in height and its bootstrapped standard error are displayed directly on the graph and confirm that we cannot detect a lack of continuity at the kink. I also extend the spirit of the McCrary test to test the assumption of continuity of the derivative of the p.d.f, as done in Card et al. (2012). The idea is to regress the number of observations N_i in each bin on polynomials of the average highest quarter of earnings in each bin (centered at the kink) $(w - k)$ and the interaction term $(w - k) \cdot \mathbb{1}[W \geq k]$. The coefficient on the interaction term for the first-order polynomial (testing for a change in slope of the p.d.f) reported on panel A of Figure 4 is insignificant, which supports the assumption 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 the assignment variable as done in Figure 5 in Louisiana. Panels A, B, C, and D of Figure 5 all suggest that the covariates evolve smoothly at the kink, in support of the identification assumptions of the RK design. In panel C, I investigate whether differences in ex ante savings behaviors may affect the local random assignment assumption of the RK design. To do so, I exploit the information available in the CWBH survey, which contains a reported measure of capital income and interests. Although this is not a perfect measure of liquidity, this is a good proxy for the availability of savings. Figure 5 panel C displays the relationship between the probability of having positive capital income and the assignment variable, which does not exhibit any nonlinearity at the kink. Formal tests for all covariates can also be performed by running polynomial regressions of the form described in equation (10). Results are described in the next subsection.

[29] 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 0.05 is the largest that passes the test for all states and outcomes of interest.

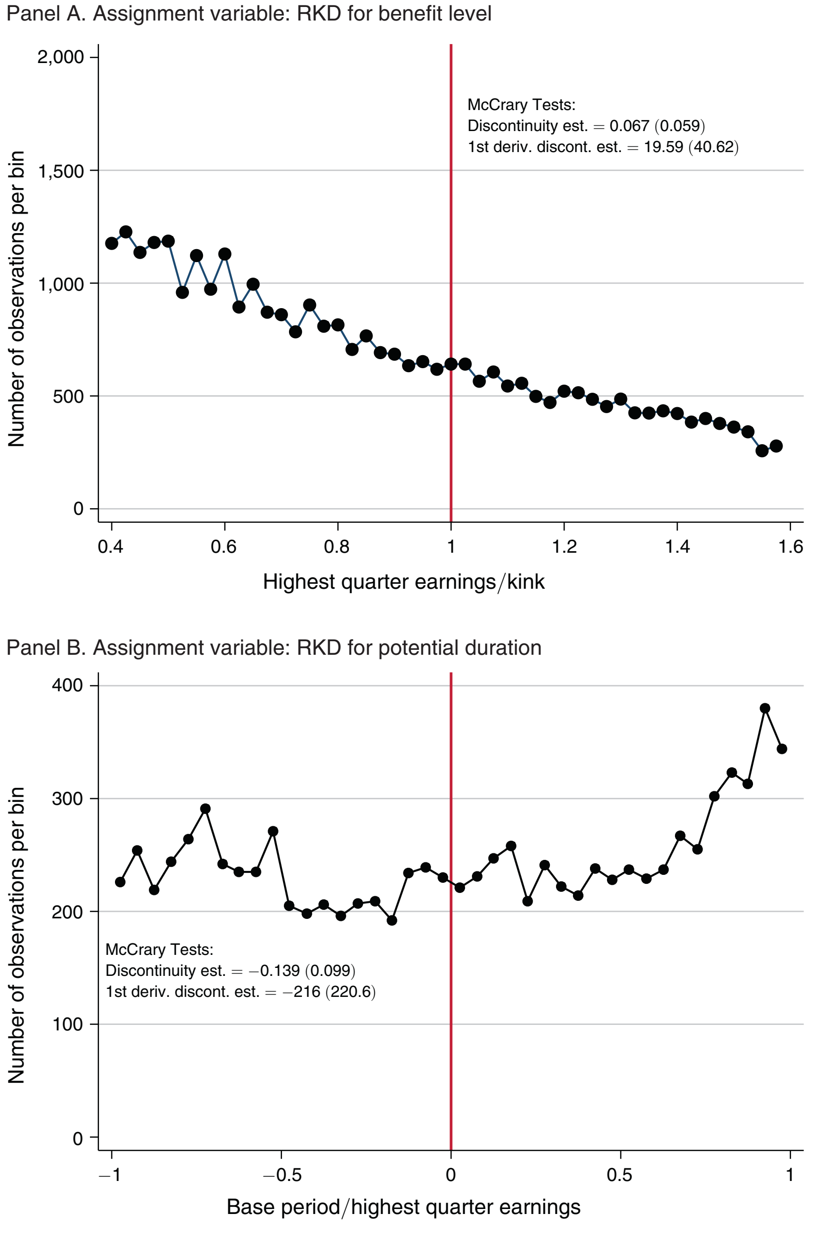

Figure 4. RKD Graphical Evidence of the Effect of Unemployment Benefits: Duration of UI Claims

Notes: The graph assesses the validity of the assumptions of the RK design by testing graphically the smoothness of the distribution of the assignment variable at the kink point in the UI schedules. Panel A shows the probability density function of the assignment variable for the schedule of UI benefit level, normalized at the kink point. Panel B shows the probability density function of the assignment variable for the schedule of UI potential duration, centered at the kink point. I also display two tests of the identifying assumptions of the RKD. The first is a standard McCrary test of the discontinuity of the p.d.f of the assignment variable. I report here the log difference in height of the p.d.f at the kink. The second is a test for the continuity of the first derivative of the p.d.f. I report here the coefficient estimate of the change in slope of the p.d.f in a regression of the number of individuals in each bin on polynomials of the assignment variable interacted with a dummy for being above the kink. See text for details.

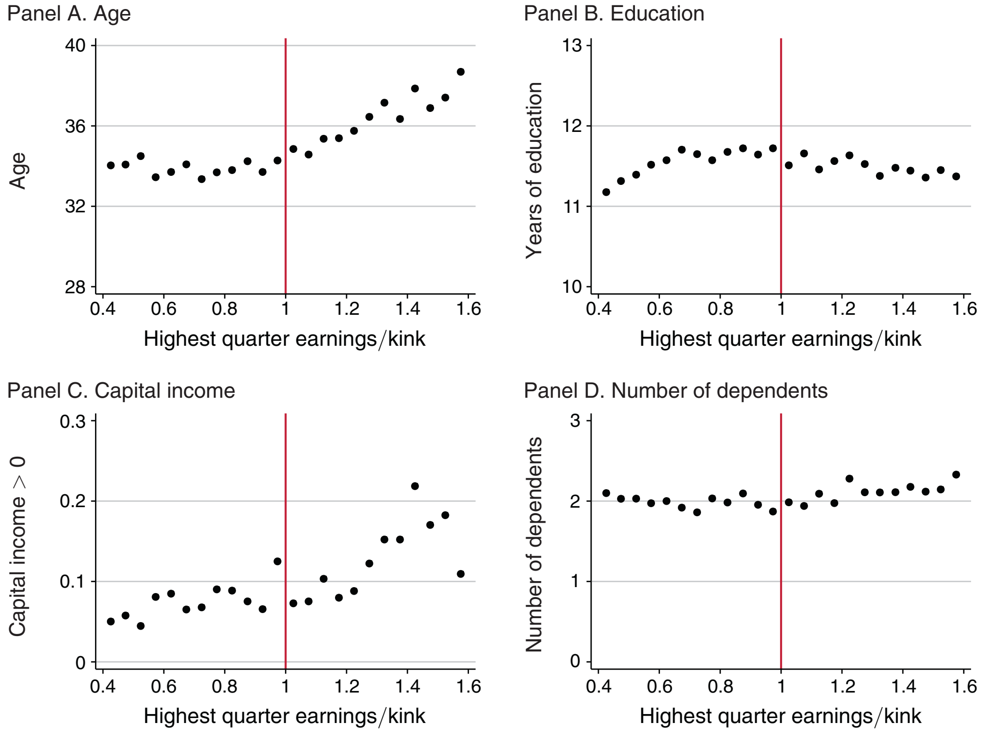

Figure 5. Distribution of Highest Quarter Earnings and Covariates, Louisiana

Notes: The graphs test the validity of the smoothness assumptions of the RK design (for the first subperiod of analysis in Louisiana). For all four panels, highest quarter of earnings, which is the assignment variable in the RK design for the estimation of the effect of benefit level, is normalized by the kink point. The binsize is 0.05 and passes the test of excess smoothing recommended in Lee and Lemieux (2010). Each panel shows the mean values of a different covariate in each bin of the assignment variable. 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.

The pattern for the outcome variables offers a striking contrast with that of covariates, as shown in Figure 6, panel A, which displays the evolution of the relationship between the duration of UI claims and the assignment variable normalized at the kink. There is a sharp visible change in the slope of the relationship between the duration of UI claims and the assignment variable at the kink point of the benefit schedule. Figure 7 replicates the same graphical diagnosis for all five states.[30] This provides supportive evidence for the identification of an effect of benefit level on unemployment duration in the RK design.

Estimation Results: Table 2 shows the results for the baseline specification of equation (10) in the linear case for Louisiana for all five subperiods. In each column, I report the estimate of the weighted average treatment effect $\hat{\alpha} = -\frac{\hat{\nu}_1}{\tau_1}$, where $\hat{\nu}_1$ is the estimated change in slope in the relationship between the outcome and the

[30] Results for the other duration outcomes of interest are displayed in online Appendix Figures B2 and B3 and reveal the exact same patterns.

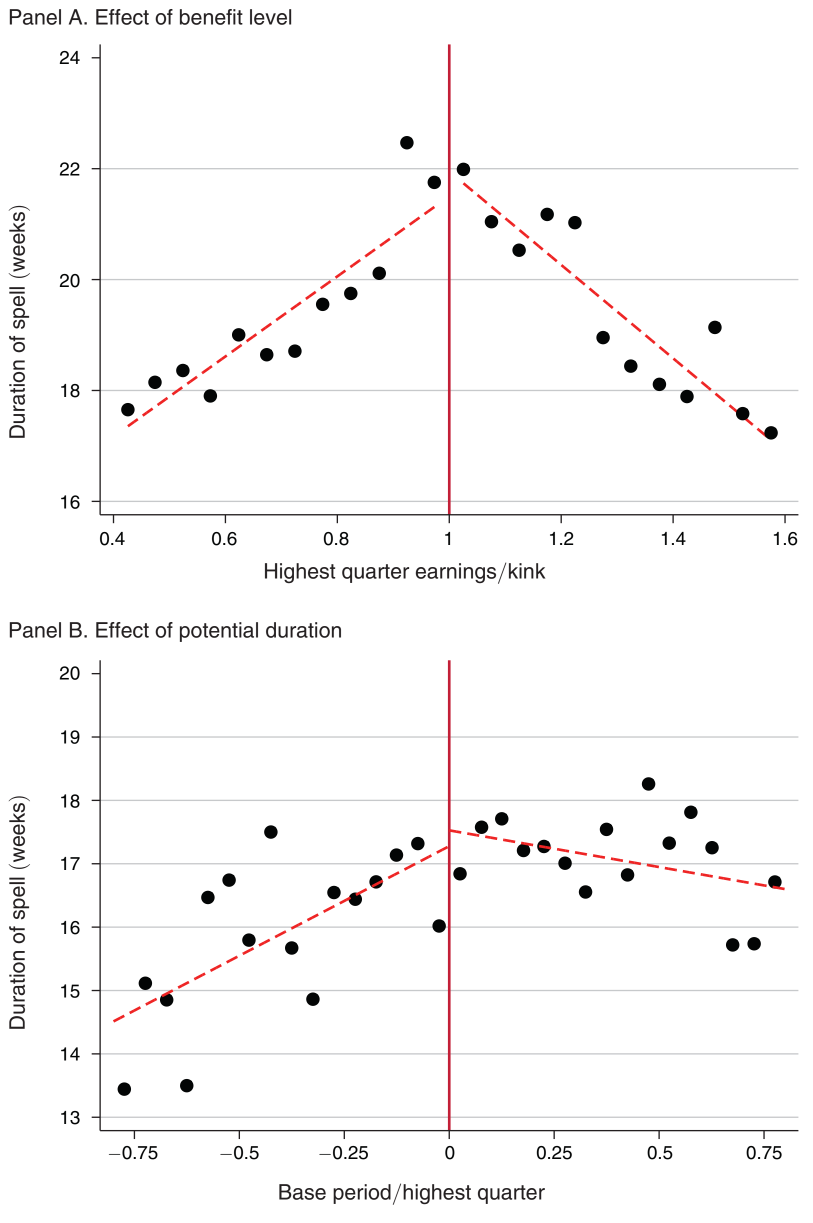

FIGURE 6. RKD GRAPHICAL EVIDENCE OF THE EFFECT OF UNEMPLOYMENT BENEFITS: DURATION OF UI CLAIMS, LOUISIANA 1979–1984

Notes: Panel A shows for the first subperiod of analysis in each state the mean values of the duration of UI claims in each bin of highest quarter of earnings normalized at the kink point in the schedule of the weekly benefit amount. 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 (10) are displayed in Table 2. The dashed lines display predicted values of the regressions in the linear case allowing for a discontinuous shift at the kink. Panel B shows the mean values of the duration of initial spell in each bin of the ratio of base period earnings (*bpw*) divided by highest quarter earnings (*hqw*), which is the assignment variable in the schedule of potential UI duration, and centered at the kink point in the schedule. 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 3. The dashed lines display predicted values in the linear case allowing for a discontinuous shift at the kink.

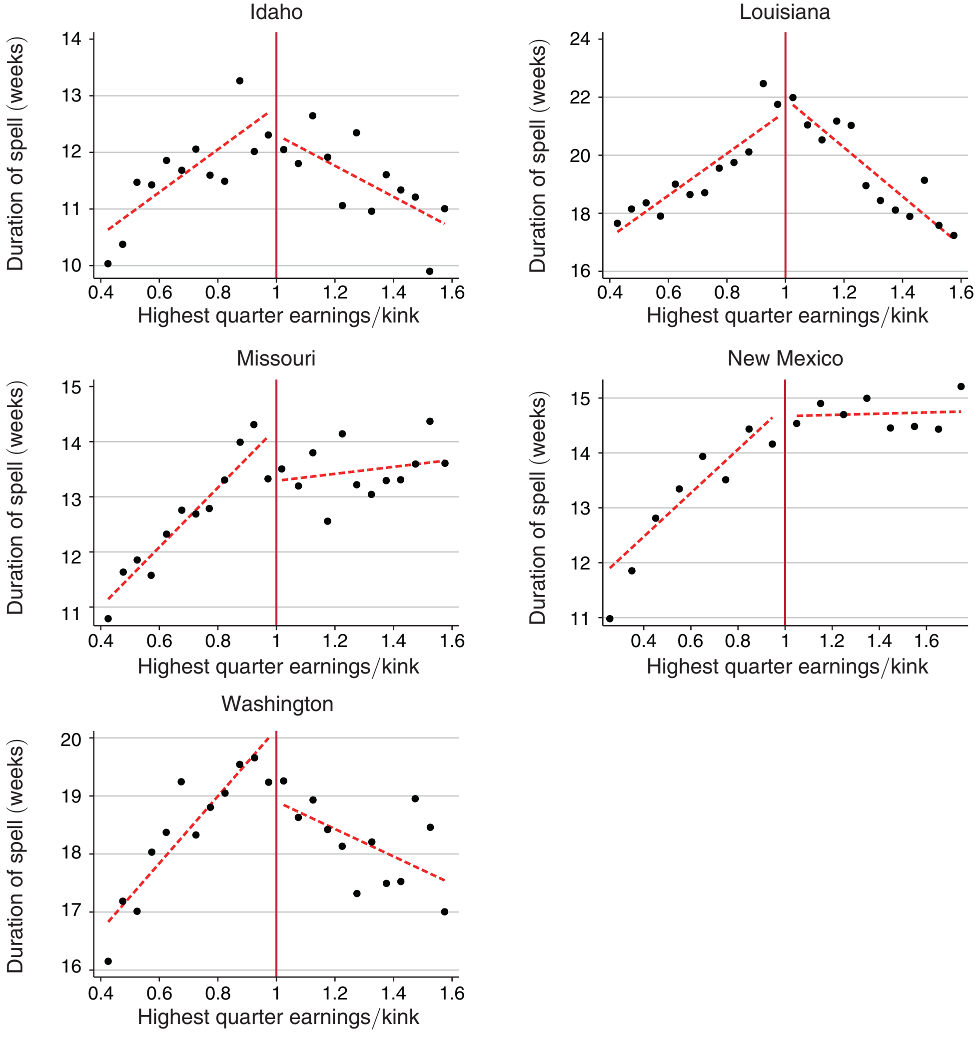

Figure 7. RKD Evidence of the Effect of Benefit Level: Duration of UI Claims versus Highest Quarter Earnings for All Five States

Notes: The graph shows in each state the mean values of the duration of UI claims in each bin of highest quarter of earnings normalized by the kink point in the schedule of the UI benefit level. 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 (10) are displayed in Table 2 and online Appendix Tables B2 to B5. The dashed lines display predicted values of the regressions in the linear case allowing for a discontinuous shift at the kink.

assignment variable at the kink point from specification (10) and τ_1 is the deterministic change in slope in the schedule of UI benefits at the kink point. Each estimate is done using nominal schedules, but the $\hat{\alpha}$ are rescaled to 2010 dollars and they should be interpreted as the effect of an extra \$1 in weekly benefit amount on the average duration (in weeks) of the outcome.[31] The coefficient estimate of 0.04 (Table

[31] The marginal effect $\hat{\alpha}$ estimated in the RK design is of course a local estimate for individuals at the kink and might differ from the average treatment effect (ATE) for the whole population in the presence of heterogeneity. $\hat{\alpha}$ is, to be precise, an average treatment effect weighted by the ex ante probability of being at the kink given the distribution of unobserved heterogeneity across individuals.

Table 2—RKD Estimates of the Effect of Benefit Level, Louisiana 1979–1983

	Duration of initial spell (1)	Duration UI claimed (2)	Duration UI paid (3)	Age (4)	Male (5)	Years of education (6)	Number of dependents (7)
				January–September 1979			
α	0.006	0.007	0.006	−0.121	0	0.002	0.004
	(0.006)	(0.006)	(0.006)	(0.069)	(0.002)	(0.014)	(0.01)
ε_b	0.183	0.228	0.186				
	(0.183)	(0.184)	(0.165)				
p-value	0.216	0.274	0.283	0.116	0.506	0.056	0.262
Observations	2,129	2,129	2,129	2,117	2,106	1,953	1,479
				September 1979–September 1980			
α	0.018	0.019	0.018	0.052	−0.001	0.003	−0.001
	(0.005)	(0.005)	(0.005)	(0.056)	(0.002)	(0.012)	(0.001)
ε_b	0.484	0.518	0.45				
	(0.143)	(0.142)	(0.131)				
p-value	0.589	0.389	0.499	0.659	0.041	0.735	0.742
Observations	3,765	3,765	3,765	3,752	3,723	3,483	2,042
				September 1980–September 1981			
α	0.018	0.019	0.018	0.054	0.002	−0.025	−0.026
	(0.006)	(0.006)	(0.006)	(0.069)	(0)	(0.016)	(0.01)
ε_b	0.455	0.467	0.422				
	(0.147)	(0.148)	(0.135)				
p-value	0.007	0.003	0.006	0.509	0.064	0.992	0.908
Observations	3,133	3,133	3,133	3,116	3,089	2,932	1,849
				September 1981–September 1982			
α	0.042	0.038	0.04	0.051	−0.001	0	0
	(0.009)	(0.009)	(0.009)	(0.059)	(0.002)	(0.014)	(0.014)
ε_b	0.708	0.665	0.644				
	(0.154)	(0.154)	(0.142)				
p-value	0.091	0.178	0.108	0.43	0.595	0.314	0.28
Observations	3,845	3,845	3,845	3,823	3,786	3,553	1,351
				September 1982–December 1983			
α	0.047	0.046	0.042	−0.013	−0.001	0.001	−0.001
	(0.006)	(0.006)	(0.006)	(0.005)	(0.001)	(0.001)	(0.001)
ε_b	0.757	0.763	0.667				
	(0.103)	(0.105)	(0.098)				
p-value	0.199	0.175	0.084	0.64	0.508	0.261	0.843
Observations	6,602	6,602	6,602	6,558	6,520	6,078	3,531

Notes: Duration outcomes are expressed in weeks. α is the RK estimate of the average treatment effect of benefit level on the outcome. Robust standard errors for the estimates of α are in parentheses. The elasticity of the three duration outcomes with respect to the UI benefit level $\varepsilon_b = \hat{\alpha} \cdot \frac{b_{max}}{\overline{Y_1}}$, 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 (10). All estimates are for the linear case. Each period corresponds to a stable schedule for the benefit level (cf. Figure 2).

2, column 3, Sept. 1981 to Sept. 1982) for instance suggests that a \$1 increase in weekly benefits leads to a 0.04 week increase in the duration of paid unemployment.

I also report the elasticity with respect to the benefit level $\left(\varepsilon_b = \hat{\alpha} \cdot \frac{b_{max}}{\overline{Y_1}}\right.$, where $\overline{Y_1}$ is mean duration at the kink point$\left.\right)$ 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 0.2 and 0.7 depending on the subperiod of interest. These estimates suggest that a 10 percent increase in the average weekly benefit amount increases, on average, by 2 to 7 percent the duration of unemployment. 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 five periods. In online Appendix Table B5, I display estimates of the elasticity of all duration outcomes, including the duration of total nonemployment, in Washington, the only state for which we observe reemployment dates from wage records in the CWBH data. Interestingly, the marginal effect of a change in benefit level on the duration of nonemployment is very similar to the effect on the duration of UI claims or on the duration of paid UI. But the duration of nonemployment being usually quite longer than the duration of paid UI, the elasticity of nonemployment duration is relatively lower than the elasticity of paid UI spells.

I provide various tests for the robustness of the RKD estimates. For the sake of brevity, most of the details of these tests are given in online Appendix A. In Table A1, panel A, I begin by analyzing the sensitivity of the results to the choice of the polynomial order. The estimates for α are of very similar magnitude for the linear, the quadratic, and the cubic specification. 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 A1, panel B 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. Overall though, it should be noted that the RKD does pretty poorly with small samples, and therefore is quite demanding in terms of bandwidth size compared to a regression discontinuity design.

I then provide two tests to deal with the issue of functional dependence between the forcing variable and the outcome of interest. A key identifying assumption of the RK design is that, conditional on b, this relationship is smooth at the kink. But in practice, it could be that the relationship between the forcing variable and the outcome (in the absence of a kink in the schedule of b) is either kinked or simply quadratic. Then, the RKD estimates are likely to be picking up this functional dependence between y and w_1 instead of the true effect of b on y. One way to control for this type of issue would be to compare two groups of similar individuals with different UI schedules, so that kinks would be at different points of support of the forcing variable. As shown in online Appendix A.3, under the assumption that the functional dependence between y and w_1 is the same for the two groups, the average treatment effect can be identified and estimated in a "double-difference regression kink design."

To implement this strategy, the idea is to use the presence of variations in the maximum benefit amount over time, that shift the position of the kink across the distribution of the forcing variable (as shown in Figure 2). The problem though is that, taken separately, each variation in max_b is too small to give enough statistical power to detect changes in slopes because the bandwidths are too small, and as previously pointed out, the drawback of the RKD is to be quite demanding in terms of bandwidth size. The idea therefore is to compare periods that are further away in time.[32] Figure A2 in online Appendix A shows the relationship between the duration of paid unemployment and the forcing variable in 1979 and 1982. Interestingly, there is a kink in this relationship in 1979 at the level of the 1979 kink in the schedule, and this kink disappears in 1982, when a new kink appears right at the level of the 1982 kink. Furthermore, in the interval between the 1979 and 1982 kinks, there is a change in slope in the relationship between the duration of unemployment and the forcing variable. This evidence is strongly supportive of the validity of the RK design. Table A2 reports the double-difference RKD estimates of the effect of benefit level corresponding to the evidence of Figure A2. The point estimates are perfectly in line with the baseline RKD estimates of Table 2. The DD-RKD strategy being a lot more demanding, the precision of the estimates is nevertheless quite reduced compared to the baseline RKD strategy.

Another way to test for the functional dependence between earnings and the outcome is to run RKD estimates using as the forcing variable a placebo, i.e., a proxy for previous earnings, that would not be too correlated with the highest quarter of earnings. In the CWBH data, the variable that is best suited for this strategy is the reemployment wage. Online Appendix Table A3 explores the robustness of the RKD results using the post unemployment wage as a placebo forcing variable instead of the pre-unemployment highest quarter of earnings. Results show that we cannot detect any effect in these placebo specifications.[33]

I finally conduct a semi-parametric test inspired by the literature on the detection of structural breakpoints in time series analysis, following for instance Bai and Perron (2003). The principle of the test is to try to nonparametrically detect the

[32] The obvious drawback of this option is that the identifying assumption is less likely to hold as one compares periods that are further away in time. In particular, one may worry about the high inflation rates during this period. It is important to note here that the maximum benefit amount increased in Louisiana a lot faster than inflation (40 percent between September 1979 and September 1982 and total inflation was less than 20 percent during that period), so that there is a clear and important change in the schedule in *real* terms. To further alleviate this concern, I also control for quadratic in *real* highest quarter of earnings in the DD-RKD specifications and find similar results.

[33] Ganong and Jäger (2014) propose a clever alternative test for curvature in the relationship between expected duration and previous earnings. The principle of the test is to use four part linear splines (therefore with two placebo kinks) instead of a two part linear spline. Using all 26 state $\times$ period estimates, it is possible to look at the distribution of estimates at the true kink and at 2 placebo kinks (one at \$1,000 and the other at $-$\$1,000) in the 4 part linear splines. For the placebo kink at \$1,000, the median point estimate is zero but not for the placebo kink at $-$\$1,000 kink which suggest some curvature of expected duration with respect to earnings that may not be fully reflected in the conventional standard errors reported in my estimates.

location of the kink by looking for the kink point that would minimize the residual sum of squares or equivalently maximize the R^2. Details of the test are given in online Appendix A.5. I report in Figure A3 the evolution of the R^2 as I change the location of the kink point in specification (10). The evolution of the R^2 as one varies the location of the kink points provides evidence in support of the validity of the RKD design. The R^2 increases sharply as one moves closer to the actual kink point and then decreases sharply, supportive of the existence of a kink around 0.

Comparison to Other Studies: I replicate the RKD estimation procedure for all states and periods. All the estimates are displayed in online Appendix B. Overall, estimates of the elasticity of unemployment duration with respect to the benefit level are consistently between 0.1 and 0.7. The average elasticity of the duration of initial spell for all 5 states and periods is 0.32 (standard deviation is 0.2), where each period of analysis is defined as the entire period for which the benefit schedule is left unchanged and which represents a total of 26 different estimates. To get a sense of the validity of the RK design, it is useful to compare the RKD estimates to existing estimates in the literature. My estimates are on the lower end of the spectrum when compared to traditional benchmarks in the literature on US data. Estimation of the effect of UI benefit level in this literature has, however, always been struggling with the endogeneity issue due to the joint determination of UI benefits and previous earnings. Most empirical studies on US data therefore use proportional hazard models and add controls for previous earnings.[34] In Table A4 in online Appendix A.6, I report the estimates of Cox proportional hazard models on the CWBH data,[35] which enables me to compare my results to the widely cited benchmark of Meyer (1990), who used a smaller sample of the same CWBH records. Online Appendix Table A4 shows that the estimates of Meyer (1990), who found an elasticity of 0.56,[36] can be fully replicated using his specification. The drawback of these estimates is that they may not fully address the endogeneity issue due to the joint determination of UI benefits and previous earnings. Meyer (1990) only controls for previous wages using the log of the base period earnings. Interestingly, if one adds a richer set of non-parametric controls for previous earnings to mitigate the concern of endogeneity, and fully controls for variations across labor markets by adding time fixed effects interacted with state fixed effects, the results converge to the RKD estimates and the elasticity goes down to around 0.3. The reason is that, as one controls more efficiently for the functional dependence between unemployment duration and previous earnings, the only identifying variation in benefit level that is left comes from the kink in the benefit schedule, and the model naturally converges to the

[34] See, for instance, estimates in Chetty (2008); Kroft and Notowidigdo (2011); or Spinnewijn (2010), and surveys in Holmlund (1998) or Krueger and Meyer (2002).

[35] All the details of the estimation procedure are given in online Appendix A.6.

[36] See Meyer (1990, table VI, column 7). Coefficient estimates for $\log(b)$ in the proportional hazard models of Table A4 can be interpreted as the elasticity of the hazard rate s with respect to the weekly benefit level. However, under the assumption that the hazard rate is somewhat constant, these elasticities can be easily compared to the RKD elasticities of unemployment duration, since $D \approx 1/s$ so that $\varepsilon_D \approx -\varepsilon_s$.

identification strategy of the RKD. Taken together, the results from these multiple robustness checks strongly support the validity of the RK design.

B. *Benefit Duration*

The existence of unemployment insurance extensions due to the EB program and the federal FSC program during the period covered by the CWBH creates frequent changes in the schedule of potential duration.[37] The schedule for potential duration applies based on the date of the week of certified unemployment so that changes in the schedule do usually affect ongoing spells. This complicates the estimation of the effect of potential duration in the CWBH sample because a fundamental requirement of the RK design is that the unemployed anticipate the stationarity of the schedule during the whole duration of their 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 for estimating the effect of potential duration on actual unemployment duration. 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 three subperiods.[38] Because of these constraints, the number of estimates for the effect of potential duration is more limited than for the effect of benefit level.

The ratio of base period earnings (bpw) divided by highest quarter earnings (hqw) is the assignment variable in the schedule of potential UI duration as explained in Section IIA and plotted in Figure 3. Figure 6, panel B plots the mean values of the duration of UI claims in each bin of bpw/hqw and centered at the kink in the schedule of potential duration. The graph provides evidence of a kink in the relationship between the assignment variable and the duration of UI claims at the kink in the schedule of potential duration. 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 Figure 6.

Table 3 presents the results for the average treatment effect $\hat{\beta}$ of a one week increase in potential duration with robust standard errors for Louisiana. For each of the three subperiods with stable schedules, 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 0.2 to 0.4 for all duration outcomes and subperiods 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 4–8, 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. Note that the average duration of UI claims when benefit exhaust after B weeks and $S(t)$ is the survival rate at time t is $D_B = \sum_{t=0}^{B-1} S(t)$. The effect of a one week increase in the potential duration of

[37] In Louisiana for instance the schedule changed 11 times between January 1979 and December 1983.

[38] The first subperiod 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 subperiod contains all spells beginning after 06/19/1983 to 31/12/1983.

Table 3—Baseline RKD Estimates of the Effect of Potential Duration, Louisiana

	Duration of initial spell (1)	Duration UI claimed (2)	Duration UI paid (3)	Age (4)	Years of education (5)	Male (6)	Dependents (7)
	Period 1: January 1979–January 1980						
β	0.21	0.184	0.211	−0.277	0.013	0.006	−0.027
	(0.113)	(0.114)	(0.111)	(1.609)	(0.03)	(0.006)	(0.024)
ε_B	0.413	0.363	0.38				
	(0.223)	(0.225)	(0.2)				
Opt. poly	1	1	1	3	1	1	1
p-value	0.557	0.484	0.471	0.338	0.087	0.511	0.022
Observations	3,497	3,497	3,497	3,476	3,216	3,465	2,208
	Period 2: September 1981–April 1982						
β	0.349	0.352	0.335	−0.251	0.005	0.002	−0.023
	(0.141)	(0.138)	(0.136)	(0.135)	(0.029)	(0.005)	(0.03)
ε_B	0.793	0.804	0.71				
	(0.32)	(0.315)	(0.289)				
Opt. poly	1	1	1	1	1	1	1
p-value	0.133	0.149	0.107	0.486	0.493	0.842	0.388
Observations	2,165	2,165	2,165	2,148	1,959	2,138	888
	Period 3: June 1983–December 1983						
β	0.387	0.363	0.334	−0.061	−0.014	0.006	−0.125
	(0.088)	(0.086)	(0.085)	(0.079)	(0.019)	(0.003)	(0.061)
ε_B	0.854	0.851	0.708				
	(0.194)	(0.201)	(0.181)				
Opt. poly	1	1	1	1	1	1	2
p-value	0.675	0.751	0.742	0.624	0.898	0.493	0.754
Observations	2,936	2,936	2,936	2,917	2,720	2,904	1,601

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 β 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 (10). The optimal polynomial order is chosen based on the minimization of the Aikake Information Criterion.

unemployment benefits dB on the average duration of UI claims is $\frac{dD_B}{dB} = \sum_{t=0}^{B-1} \frac{dS(t)}{dB} + S(B)$, which is the sum of a behavioral response $\sum_{t=0}^{B-1} \frac{dS(t)}{dB}$ and of the mechanical effect $S(B)$ of truncating nonemployment durations one week later. The average exhaustion rate for all UI tiers $S(B)$ is between 11 percent and 18 percent as shown in Table 1. This suggests that the 0.2–0.4 week estimated response is not entirely driven by the mechanical effect, but that only a half to two-thirds of the estimated response can be attributed to the behavioral response.

The estimates of an increase of 0.2 to 0.4 weeks of unemployment with each additional week of UI, which translates into an elasticity of unemployment claims with respect to potential duration of 0.4 to 0.8, are in line with previous estimates in the United States such as Moffitt (1985b); Card and Levine (2000); and Katz and Meyer (1990). They are higher than existing estimates in Europe using RD designs such as Schmieder, von Wachter, and Bender (2012) for Germany. This could be due to much longer baseline durations in European UI systems. In Schmieder, von Wachter, and Bender (2012) 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 United States. They are also larger than the estimates of Rothstein (2011), who finds very small effects of UI extensions during the Great Recession. His identification strategies, however, might be picking up equilibrium effects in the labor market, which might be lower during recessions in the presence of negative job search externalities as suggested in Landais, Michaillat, and Saez (2010).

IV. Moral Hazard, Liquidity, and Welfare Calibrations

A. *Liquidity Effects and Calibrations*

To calibrate the welfare effects of UI following the (transformed) Baily-Chetty formula of Chetty (2008), I need estimates of the elasticities of paid unemployment duration and of total nonemployment duration, as well as estimates of the liquidity to moral hazard ratio. In the CWBH data, Washington is the only state for which information on total nonemployment duration is available through the matched UI records-wage records. I therefore now restrict interest to Washington. To compute the liquidity to moral hazard ratio, one needs to estimate at the same time the effect of benefit level and that of potential duration. I therefore focus on the longest period (July 1980 to July 1981) for which we have a stationary schedule in Washington for both benefit level and potential duration. In Table 4, I give in column 1 and 2 RKD estimates of the elasticities for the period of interest in Washington.

Estimation of Liquidity and Moral Hazard Effects: The estimation of liquidity and moral hazard effects follows from the application of the result of Proposition 1. The result of Proposition 1 relies on the assumption that the liquidity constraint is not yet binding at the exhaustion point B. In online Appendix A.8, I provide a simple test for this assumption. The intuition for the test is the following. If the liquidity constraint is binding, it means that the unemployed can no longer deplete their asset; they are hand-to-mouth, and therefore, benefits that they have received in the past do not have any effect on their future behavior. If to the contrary, exit rates after the exhaustion point are affected by benefits received before exhaustion, it means that agents can still transfer part of their consumption across time periods. Results, reported in the online Appendix, show that one additional dollar of UI before 39 weeks reduces the exit rate of unemployment after exhaustion, between 40 weeks and 60 weeks, by a statistically significant 0.2 percentage point. These estimates suggest that the Euler equation holds and that variations in benefits prior to exhaustion affect exit rate of unemployment after the exhaustion point.

In practice, to implement the result of Proposition 1, I estimate separately in the regression kink design the effect of an increase in benefit level $\left(\left.\frac{\partial s_0}{\partial b}\right|_B\right)$ and of an increase in potential duration $\left(\frac{\partial s_0}{\partial B}\right)$ on the hazard rate out of unemployment

Table 4—RKD Estimates of Behavioral Responses to UI and Liquidity and Moral Hazard Effect Estimates, Washington, July 1980–July 1981

	Effect of benefit level (1)	Effect of potential duration (2)	Liquidity and moral hazard estimates (3)
ε_{D_B}	0.730	1.348	
	(0.110)	(0.685)	
	[0.814]	[0.388]	
ε_D	0.291	0.330	
	(0.071)	(0.425)	
	[0.392]	[0.474]	
$\left(\frac{1}{B}\frac{\partial s_0}{\partial b}\Big\vert_B - \frac{1}{b}\frac{\partial s_0}{\partial B}\right) \times 10^3$			−0.042
			(0.01)
Moral hazard:			
Θ_1			0.0014
			(0.0001)
Liquidity to moral hazard:			
ρ_1			0.876
			(0.022)
Observations	6,061	2,049	9,471

Notes: For all columns, standard errors for the estimates are in parentheses. p-values are reported between brackets and 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 (10). Results are obtained from a linear specification. The bandwidth for the RK estimate of benefit level is 2,500 (assignment variable: highest quarter of earnings) and 0.75 for the RK estimate of the potential duration (assignment variable: ratio of base period to highest quarter of earnings). This table shows how to use the RKD to estimate all the statistics needed to calibrate the welfare effects of UI. Column 1 reports the RKD estimate of the elasticity of UI duration $\left(\varepsilon_{D_B}\right)$ and of the elasticity of nonemployment duration (ε_D) with respect to benefit level. Column 2 reports the RKD estimate of the same elasticities with respect to potential duration. Column 3 reports the liquidity and moral hazard effect estimates following the strategy detailed in Proposition 1. $\left(\frac{1}{B}\frac{\partial s_0}{\partial b}\Big\vert_B - \frac{1}{b}\frac{\partial s_0}{\partial B}\right)$ is the difference between the RKD estimate of the effect of benefit level (divided by the potential duration) and the RKD estimate of the effect of potential duration (divided by the benefit level) on s_0, defined as the exit rate out of unemployment in the first four weeks of unemployment. To ensure that the characteristics of individuals at both kinks (in benefit level and potential duration) are the same, I use a reweighing approach described in online Appendix B. Following Proposition 1, this difference is then used to compute the moral hazard effect Θ_1 of an increase in benefit level and the ratio of liquidity to moral hazard ρ_1 in the effect of an increase in benefit level. For the three statistics of column 3, bootstrapped standard errors with 50 replications are in parentheses. See text for additional details.

at the beginning of a spell.[39] Proposition 1 requires that we estimate the effect of benefit level and potential duration for the same individuals. To ensure that the characteristics of individuals at both kinks (in benefit level and potential duration) are the same, I use a re-weighting approach described in online Appendix A.10. Column 3 of Table 4 reports $\left(\frac{1}{B}\frac{\partial s_0}{\partial b}\Big\vert_B - \frac{1}{b}\frac{\partial s_0}{\partial B}\right)$, the difference between the RKD estimate of the effect of benefit level (divided by the potential duration) and the RKD estimate of the effect of potential duration (divided by the benefit level) on s_0. Standard errors

[39] To increase the precision of the estimates, I define s_0 as the probability of exiting unemployment in the first four weeks. Shorter definitions for period 0 yield similar results, but the standard errors on the estimates of the effect of potential duration increase sharply.

for all statistics in column 3 are bootstrapped with 50 replications.[40] By a simple application of Proposition 1, this difference is then divided by $\Phi_1 = -\frac{\overline{S_1^B} - S_1(B)}{D_1^B}$ to compute the moral hazard effect Θ_1 of an increase in benefit level and the ratio of liquidity to moral hazard ρ_1 in the effect of an increase in benefit level. I use the observed average survival rates and durations for the full period July 1980 to July 1981 in Washington and for individuals at the kink of benefit level in order to compute Φ_1.

The estimate reported in column 3 suggests the existence of substantial liquidity effects, with a ratio of liquidity effect to moral hazard effect of 88 percent. This estimate is, however, smaller than the figures reported in Chetty (2008), who finds a ratio of roughly 1.5 using data on severance payments. The great advantage of the RKD strategy is to be able to estimate liquidity effects from administrative UI data directly, without the need for information on severance payments or for consumption data.

Calibrations: I now use these estimates to calibrate the welfare effects of UI. The optimal UI formulas expressed in terms of ratio of liquidity to moral hazard are presented, derived, and explained in online Appendix C.4 and C.5. To calibrate the Insured Unemployment Rate $D_B/(T - D)$, I use the total number of paid unemployed divided by the total number of employees paying payroll taxes in the wage records in Washington for the period July 1980 to July 1981. This yields $D_B/(T - D) \approx 3.9\%$. Similarly, I calibrate $D/T - D \approx 8.5\%$ as the average unemployment rate in Washington during the period computed from CPS.[41] $\omega_1 = \frac{B}{D_B - s_0(B - 1)} - 1 \approx 17$ is calibrated directly from the CWBH data in Washington. Plugging the estimated elasticities of column 2 of Table 4 into formula (31) of the online Appendix yields the right-hand side of the optimal formula $\omega_1 \frac{D_B}{T - D}\left(1 + \varepsilon_{D_B} + \varepsilon_D \frac{D}{T - D}\right) \approx 1.14$. With a ratio of liquidity to moral hazard $\rho_1 \approx 0.88$, it means that the left-hand side of the formula $(1 + \rho_1 \approx 1.88)$ is greater than the right-hand side. This indicates that increasing the benefit level from its current level would be welfare increasing.[42] Similarly, one can calibrate the formula for the welfare effects of the potential duration of UI derived in online Appendix C.5. Under the approximation that $\rho_2 \approx \rho_1$, and given that in the CWBH data, $\omega_2/B \approx 14.2$, the right-hand side of equation (33) is approximately equal to 1.29, which is again lower than the left-hand side of the formula. Once again, the

[40] To be precise, I merge observations from both samples, the one at the benefit level kink and the one at the potential duration kink, and draw with replacement 50 different samples from that merged sample. I then replicate the full estimation procedure from these 50 samples to compute the standard errors on $\left(\frac{1}{B}\frac{\partial s_0}{\partial b}\Big|_B - \frac{1}{b}\frac{\partial s_0}{\partial B}\right)$, Θ_1, and ρ_1.

[41] The way I calibrate the ratios $D_B/(T - D)$ and $D/T - D$ relies on the assumption, implicit in the model, that each state UI agency balances its own budget every period. This assumption is somewhat restrictive, since the federal government subsidizes state UI agencies in practice. In particular, half of the cost of EB extensions is paid by the federal budget.

[42] Note that the Baily formula focuses on the optimal UI benefit level net of all taxes. The switch operated in the 1980s toward making UI benefit part of the income tax base may have reduced the net-of-tax benefit level even further from the optimal benefit level b obtained from my calibration.

result of this calibration suggest that a small increase in the potential duration of UI would be welfare increasing.

V. Conclusions

This paper has shown how, in the tradition of the dynamic labor supply literature, one can identify the moral hazard and liquidity effects of UI using variations along the time profile of UI benefits brought about by exogenous variations in the benefit level as well as in the benefit duration. My strategy only relies on exploiting individuals' first-order conditions and variations in the time profile of benefits, which makes it easily generalizable and applicable to any other transfer program with time-dependent benefits.

I have implemented this strategy using variations in UI benefit level and UI benefit duration in the RK design. Overall, my results confirm the evidence in Chetty (2008) that liquidity effects are substantial, and that an increase in the replacement rate and duration of UI might be welfare increasing.[43] The advantage of calibrating the welfare formula using the regression kink design as described in this paper, is that the formula can technically be tested in real time, so that any UI administration could easily estimate the welfare effects of the small adjustments that are usually done in UI legislation such as a change in the maximum benefit amount.

Yet, the calibrations presented here are obtained in a very stylized version of the labor market.[44] Models in the tradition of Baily (1978) and Chetty (2006), such as the one presented here, take a pure partial equilibrium view of the labor market, with an infinitely elastic labor demand. The unemployment problem is represented as a pure labor supply story, with no effect of UI on labor market equilibrium through labor demand effects. As shown in Landais, Michaillat, and Saez (2010), in equilibrium search-and-matching models of the labor market, partial equilibrium labor supply responses to UI are no longer sufficient to compute the optimal trade-off between insurance and moral hazard, and one needs to estimate equilibrium employment responses as well.

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[43] It is important however to remember that these policy recommendations are only valid locally, at the value of the policy parameters at which the statistics entering the formula are estimated. Extrapolating the optimal level of benefit and duration of UI from these statistics would require the implausible assumption that all statistics would remain unchanged if we were to modify the policy parameters.

[44] Note for instance that calibrations have assumed perfect take-up of UI. As shown in Kroft (2008), in the presence of responses to UI at the extensive margin with endogenous take-up costs, social multiplier effects arise and the optimal replacement rates can be substantially higher than in traditional models with responses only along the intensive margin.

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