Essays in Labor Economics

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Abstract

This dissertation consists of three independent essays. The first essay analyzes the relationship between Schumpeterian growth and subjective well-being. The second essay investigates whether more generous unemployment insurance leads job seekers to be more selective in the job they are looking for. It estimates the elasticity of the reservation wage and of other dimensions of job selectivity with respect to the potential duration of benefits. The third essay studies whether there remains a causal effect of job loss on health when unemployment insurance is generous, health insurance has universal coverage and active labor market policies are available. It estimates the effect of exogenous job losses in Denmark in the 2000s on purchases of prescription drugs, visits to the doctor, hospital diagnoses and mortality.

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Introduction

This dissertation consists of three independent essays. In the first chapter, co-authored with Philippe Aghion, Ufuk Akcigit and Angus Deaton and published in the *American Economic Review* in December 2016, we analyze the relationship between turnover-driven growth and subjective well-being. Our model of innovation-led growth and unemployment predicts that: (i) the effect of creative destruction on expected individual welfare should be unambiguously positive if we control for unemployment, less so if we do not; (ii) job creation has a positive and job destruction has a negative impact on well-being; (iii) job destruction has a less negative impact in US metropolitan statistical areas (MSA) within states with more generous unemployment insurance policies; and (iv) job creation has a more positive effect on individuals that are more forward-looking. The empirical analysis using cross-sectional MSA-level and individual-level data provide empirical support to these predictions.

The second chapter, which investigates whether more generous unemployment insurance leads job seekers to be more selective in their job search, is co-authored with Thomas Le Barbanchon and Roland Rathelot and has been accepted for publication in the *Journal of Public Economics*. Although the reservation wage plays a central role in job search models, empirical evidence on the determinants of reservation wages, including key policy variables such as unemployment insurance (UI), is scarce. In France, unemployed people must declare their reservation wage to the Public Employment Service when they register to claim UI benefits. We take advantage of these rich French administrative data and of a reform of UI rules to estimate the effect of the potential benefit duration (PBD) on reservation wages and on other dimensions of job selectivity, using a difference-in-difference strategy. We cannot reject that the elasticity of the reservation wage with respect to PBD is zero. Our results are precise and we can rule out elasticities larger than 0.006. Furthermore, we do not find any significant effects of PBD on the desired number of hours, duration of labor contract and commuting time/distance. The estimated elasticity of actual benefit duration with respect to PBD of 0.3 is in line with the consensus in the literature. Exploiting a regression discontinuity design as an alternative identification strategy, we find similar results.

The third and last chapter studies the causal effect of job loss on health. Job loss can affect health both through the income shock and through non-pecuniary channels like the loss of self-esteem or the loss of a structured schedule. I investigate whether there is still a causal effect of job loss on health in a setting where the unemployment risk is well-insured by policy through both generous unemployment insurance, active labor market policies and public health insurance with universal coverage. Using Danish administrative data and a difference-in-difference design, I compare the health of roughly 25,000 high tenure workers who are at an establishment that closes between 2001 and 2006 to that of a control group of workers matched on observables who do not experience a closure. I find that in such a setting job losses do not cause any large significant effect on health, whether looking at mental health proxies such as antidepressant purchases, severe physical health outcomes that require inpatient care or mortality. I can rule out effect on most health outcomes of the order of 1 or 2%. For mortality I can rule out effects of 15%. My results taken together with prior literature suggest that it is possible, presumably through an adequate set of policies, to make the causal effect of job loss on health negligible.

Chapter 1

Creative Destruction and Subjective Wellbeing[1]

1.1 Introduction

Does higher (per capita) GDP or GDP growth increase happiness? The existing empirical literature on happiness and income looks at how various measures of subjective wellbeing (SWB) relate to income or income growth, but without looking in further detail at what drives the growth process and at how the determinants of growth affect wellbeing. In this paper, we provide a first attempt at filling this gap.

More specifically, we look at how an important engine of growth, namely Schumpeterian creative destruction with its resulting flow of entry and exit of firms and jobs, affects SWB differently for different types of individuals and in different types of labor markets.

Thus, in the first part of the paper we develop a simple Schumpeterian model of growth and unemployment to organize our thoughts and generate predictions on the potential effects of turnover on life satisfaction. In this model, growth results from quality-improving innovations. Each time a new innovator enters a sector, the worker currently employed in that sector loses her job and the firm posts a new vacancy. Production in the sector

[1] Co-authored with Philippe Aghion, Ufuk Akcigit and Angus Deaton

resumes with the new technology only when the firm has found a new suitable worker. Life satisfaction is captured by the expected discounted valuation of an individual's future earnings. In the model a higher rate of turnover has both direct and indirect effects on life satisfaction. The direct effects are that, everything else equal, more turnover translates into both a higher probability of becoming unemployed for the employed, which reduces life satisfaction, and a higher probability for the unemployed to find a new job, which increases life satisfaction. The indirect effect is that a higher rate of turnover implies a higher growth externality and therefore a higher net present value of future earnings: this enhances life satisfaction. Overall, a first prediction of the model is that a higher turnover rate increases wellbeing more when controlling for aggregate unemployment than when not controlling for aggregate unemployment. A second prediction is that job creation increases and job destruction decreases wellbeing. A third prediction is that job destruction has a less negative effect on wellbeing, the more generous are unemployment benefits. A fourth prediction is that job creation increases future wellbeing more for more forward-looking individuals.[2]

In the second part of the paper we test the predictions of the model using cross-sectional Metropolitan Statistical Area (MSA)-level US data. To measure creative destruction we follow Davis *et al.* (1996) and use their measure of job turnover, defined as the job creation rate plus the job destruction rate.[3] The data come from the Census' Business Dynamics Statistics (BDS) and are at the MSA level. In addition, we also use the Longitudinal Employer-Household Dynamics (LEHD) data from the Census, which provide information on hires, separations, employment, and thus turnover, also at the MSA level. To measure SWB, we use the Cantril ladder of life from the Gallup Healthways Wellbeing Index (Gallup), which asks individuals about both current and future wellbeing. The Cantril ladder is based on the following questions: "Imagine a ladder with steps numbered from 0 at the bottom to 10 at the top;

[2]In the Appendix we characterize the transitional dynamics of the model, and also extend the analysis to the case where job destruction can be partly exogenous, or to the case where the turnover rate is endogenously determined by a free entry condition.

[3]We have also looked at firm turnover, namely the sum of the establishment entry rate and the establishment exit rate, with similar results.

the top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time? And which level of the ladder do you anticipate to achieve in five years?"[4]

We investigate whether Schumpeterian creative destruction affects these measures of wellbeing positively or negatively, by regressing our measures of SWB on our creative destruction variables. The empirical analysis using cross-sectional MSA-level data on SWB and job turnover vindicates the theoretical predictions: namely, we find that: (i) the effect of creative destruction on wellbeing is positive when we control for MSA-level unemployment and less so if we do not; (ii) the effects of job creation and job destruction on wellbeing are positive and negative, respectively, and; (iii) job destruction has less negative effect when unemployment benefits are higher. Moreover, we find some evidence that job creation has a more positive impact on future wellbeing for more forward-looking individuals when we use income, age, and education to proxy for patience. These results are not only consistent with the theory, but they are also remarkably robust. In particular they hold whether looking at wellbeing at MSA level or at individual level, or whether using the BDS or the LEHD data to construct our proxy for creative destruction.

The paper relates to two main strands of literature. First, to the literature on innovation-led growth, job turnover and unemployment [e.g., see Davis *et al.* (1996), Mortensen and Pissarides (1998), Aghion and Howitt (1994, 1998), and Aghion *et al.* (2014)]. In particular Aghion and Howitt (1994, 1998) and Mortensen and Pissarides (1998) develop Schumpeterian models of growth through creative destruction, where growth is driven by quality-improving innovations by new entrants that make existing firms and jobs become obsolete. In any sector where new entry occurs, the incumbent firm closes down, therefore the worker employed by the incumbent firm loses her job whereas the entering firm in the sector posts a new vacancy. Equilibrium unemployment results from assuming labor market frictions

[4] In Appendix A.2, we also check the robustness of our results to a SWB measure from another dataset: the Life Satisfaction question from the Behavioral Risk Factor and Surveillance System, which asks respondents "In general how satisfied are you with your life?"

in the form of a Poisson matching rate between new vacancies and workers looking for a new job. These two papers point to two opposite effects of growth on unemployment. One is a “capitalization" effect whereby more growth reduces the rate at which firms discount the future returns from creating a new vacancy: this effect pushes towards creating more vacancies and thus towards reducing the equilibrium unemployment. The counteracting effect is a “creative destruction" effect whereby more growth implies a higher rate of job destruction which in turn tends to increase the equilibrium level of unemployment. We contribute to this literature by looking at the counteracting effects of innovation-led growth on SWB.

Second, the paper contributes to the literature on SWB. In spite of a now large literature on self-reported wellbeing,[5] there is no general consensus on how seriously these SWB measures should be taken, or on exactly what they mean. Indeed some of the most exciting recent work [e.g., see Benjamin *et al.* (2012, 2014)] is investigating these fundamental questions.[6] In this paper, we find that life satisfaction responds to the future growth prospects that are inherent in creative destruction, even in spite of the related short-run unemployment effects, and at the same time we provide some evidence of the validity and

[5]In particular, in his seminal work, Easterlin (1974) provides evidence to the effect that, within a given country, happiness is positively correlated with income across individuals but this correlation no longer holds within a given country over time. This Easterlin paradox is often explained by the idea that, at least past a certain income threshold, additional income enters life satisfaction only in a relative way; Clark *et al.* (2008) provided a review of this large literature of which Luttmer (2005), Clark and Senik (2010), and Card *et al.* (2012) are prominent examples. Recent work has found little evidence of thresholds and a good deal of evidence linking higher incomes to higher life satisfaction, both across countries and over time. Thus in his cross-country analysis of the Gallup World Poll, Deaton (2008) finds a relationship between log of per capita GDP and life satisfaction which is positive and close to linear, i.e., with a similar slope for poor and rich countries, and, if anything, steeper for rich countries. Stevenson and Wolfers (2013) provide both cross-country and within-country evidence of a log-linear relationship between per capita GDP and wellbeing and they also fail to find a critical “satiation" income threshold. Yet these issues remain far from settled, see for example the reviews by Frey and Stutzer (2002), Layard and Layard (2011), or Graham (2012).

[6]Benjamin *et al.* (2012) run three surveys to look at the extent to which, when facing two alternatives, individuals choose the alternative from which they anticipate the highest SWB (as measured by the Cantril ladder). They find that SWB and choice coincide 83% of the time.Benjamin *et al.* (2014) survey students from US medical schools who enroll in the National Residence Matching Program; they find that individuals’ actual choice of residence somewhat departs from individuals’ anticipated SWB rankings, and then they investigate possible sources of divergence between these SWB measures and revealed preferences. In the present paper, however, the predictions as to how creative destruction should affect individuals’ utility turn out to be fully mirrored by the empirical analysis using SWB measures and data.

usefulness of self-reported wellbeing as a measure of expected future material wellbeing. Such findings have not previously been documented in the wellbeing literature and they provide further evidence of the usefulness of these wellbeing measures.

The paper is organized as follows. Section 1.2 develops the model and generates predictions on the effects of turnover on SWB, and how these effects depend upon individual or local labor market characteristics. Section 1.3 describes the data, the approach underlying the empirical analysis, and presents the empirical results. Section 1.4 considers several robustness checks. Section 1.5 concludes the paper. The Appendix contains proofs, extensions to the baseline framework and extra tables.

1.2 Theoretical Analysis

1.2.1 A Toy Model

In this section, we offer a simple model to motivate our empirical analysis. The source of economic growth is Schumpeterian creative destruction which at the same time generates endogenous obsolescence of firms and jobs. The workers in the obsolete firms join the unemployment pool until they are matched to a new firm. Higher firm turnover has both a positive effect (by increasing economic growth and by increasing employment prospects of unemployed workers) and a negative effect (by increasing the probability of currently employed workers losing their job) on wellbeing. Which effect dominates will in turn depend upon both individual characteristics (e.g., discount rate and risk-aversion) and characteristics of the labor market (e.g., unemployment benefits). To keep the analysis tractable, in what follows we will consider a steady-state economy with exogenous entry, risk-neutral agents, and only endogenous job destruction. These assumptions will be relaxed in the Appendix: Section A.1.4 focuses on transitional dynamics, A.1.5 considers a model with exogenous job destruction, A.1.6 considers the implications of risk aversion, and A.1.7 endogenizes entry in the theoretical model.

Production Technology and Innovation

We consider a multi-sector Schumpeterian growth model in continuous time. The economy is populated by infinitely-lived and risk-neutral individuals of measure one, and they discount the future at rate ρ. Therefore the household Euler equation is simply

$$r = \rho, \tag{1.1}$$

where r is the interest rate of the economy.

The final good is produced using a continuum of intermediate inputs, according to the logarithmic production function:

$$\ln Y_t = \int_{j \in \mathcal{J}} \ln y_{jt} dj$$

where $\mathcal{J} \subset [0,1]$ is the set of active product lines. We will denote its measure by $J \in [0,1]$. The measure J is invariant in steady state.

Each intermediate firm produces using one unit of labor according to the following linear production function,

$$y_{jt} = A_{jt} l_{jt},$$

where $l_{jt} = 1$ is the labor employed by the firm, and is the same in all sectors. Thus the measure of inactive product lines is equal to the unemployment rate

$$u_t = 1 - J_t,$$

where u denotes the equilibrium unemployment rate. Our focus will be on balanced growth path equilibrium, therefore, when possible, we will drop time subscripts to save notation.

Innovation and Growth

An innovator in sector j at date t will move productivity in sector j from A_{jt-1} to

$$A_{jt} = \lambda A_{jt-1},$$

where $\lambda > 1$. The innovator is a new entrant, and entry occurs in each sector with Poisson arrival rate x which we assume to be exogenous. Upon entry in any sector, the previous incumbent firm becomes obsolete and its worker loses her job and the entering firm posts a new vacancy with an instantaneous cost cY.[7] Production in that sector resumes with the new technology when the firm has found a new suitable worker.

Labor Market and Job Matching

Following Pissarides (1990), we let

$$m(u_t, v_t) = u_t^{\alpha} v_t^{1-\alpha} \tag{1.2}$$

denote the arrival rate of new matches between firms and workers, where u_t denotes the number of unemployed at time t and v_t denotes the number of vacancies. Thus the flow probability for each unemployed worker to find a suitable firm is

$$m(u_t, v_t)/u_t,$$

whereas the probability for any new entrant firm to find a suitable new worker is

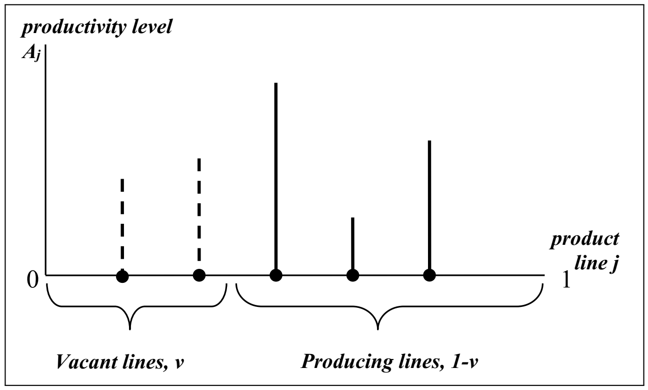
$$m(u_t, v_t)/v_t.$$

In steady state, there will be a constant fraction of product lines that are *vacant* (of measure v), and the remaining fraction will be *producing*. We illustrate this economy in Figure 1.1:

Finally, we assume that in each intermediate sector where a worker is currently employed, the worker appropriates fraction β of profits whereas the complementary fraction $(1-\beta)$ accrues to the employer.

[7] In Appendix A.1.10, we provide sufficient conditions under which the incumbent firm in any sector will choose to leave the market as soon as a new entrant shows up in that sector. The basic story is that, conditional upon a new entrant showing up, it becomes profitable for the incumbent firm to seek an alternative use of her assets.

Figure 1.1: *Model economy*



Valuations and Life Satisfaction

Life satisfaction is captured by the average present value of an individual employee, namely:

$$W_t = u_t U_t + (1 - u_t) E_t,$$

where U_t is the net present value of an individual who is currently unemployed, and E_t is the net present value of an individual who is currently employed.

The value of being currently employed, satisfies the asset equation:

$$\rho E_t - \dot{E}_t = w_t + x(U_t - E_t).$$

In words: the annuity value of being currently employed is equal to the capital gain $\dot{E}_t$ plus the wage rate w_t at time t and with arrival rate x the worker becomes unemployed as the incumbent firm is being displaced by a new entrant. Here we already see the negative effect of turnover on currently employed workers.

Similarly, the value of being unemployed satisfies the asset equation:

$$\rho U_t - \dot{U}_t = b_t + (m(u_t, v_t)/u_t)(E_t - U_t).$$

As before, the annuity value of being currently unemployed is equal to the capital gain $\dot{U}_t$

plus the benefit b_t accruing to an unemployed worker,[8] and with arrival rate $m(u_t, v_t)/u_t$ the unemployed worker escapes unemployment. For any given unemployment rate, turnover has a positive effect on the value of being unemployed because it creates job opportunities.

1.2.2 Solving the Model

We now proceed to solve the model for equilibrium production and profits, the equilibrium steady-state unemployment rate, the steady-state growth rate, and the equilibrium value of life satisfaction.

Static Production Decision and Equilibrium Profits

Let w_t denote the wage rate at date t. The logarithmic technology for final good production implies that the final good producer spends the same amount Y_t on each variety j. As a result, the final good production function generates a unit elastic demand with respect to each variety: $y_{jt} = Y_t / p_{jt}$.

Note that the cost of production is simply w_{jt} which is the firm-specific wage rate. Then the profit is simply

$$\pi_{jt} = p_{jt} y_{jt} - w_{jt} = Y_t - w_{jt}. \tag{1.3}$$

Next, the above sharing rule between wage and profits implies that $w_{jt} = \beta \left(Y_t - w_{jt} \right)$, hence

$$w_{jt} = w_t = \frac{\beta}{1+\beta} Y_t, \text{ and } \pi_{jt} = \frac{1}{1+\beta} Y_t = \pi Y.$$

Clearly, β determines the allocation of income in the economy, with a higher β shifting the income distribution towards workers.

[8] Think of this benefit term as being the sum of a (monetary) unemployment benefit and of a private utility (or disutility) of being currently unemployed. In Appendix A.1.9 we analyze the case where b corresponds to unemployment benefits financed through taxing labor. There we show that the conclusion that “the negative impact of creative destruction on wellbeing is mitigated by the unemployment benefit", continues to hold as long as the unemployment benefit is not financed completely by workers.

Steady-state Equilibrium Unemployment

Our focus is on a steady-state equilibrium in which all aggregate variables (Y_t, w_t, U_t, E_t) grow at the same constant rate g, and where the measure of unemployed u and the number of vacancies and the interest rate remains constant over time.[9] Henceforth, we will drop the time index t, when it causes no confusion.

In steady state, the flow out of unemployment must equal the flow into unemployment. Namely:

$$m(u, v) = (1 - u)x. \tag{1.4}$$

The left-hand side is the flow out of unemployment, the right-hand side is the flow into unemployment, equal to the number of active sectors $(1 - u)$ time the turnover rate x.

In addition, the number of sectors without an employed worker is equal to the number of sectors with an open vacancy, $u = v$. Combining this fact with the matching technology (1.2), we get:

$$m = u = v. \tag{1.5}$$

Putting equations (1.4) and (1.5) together, we obtain the equilibrium unemployment rate $u = (1 - u)x$, or equivalently

$$u = \frac{x}{1 + x}. \tag{1.6}$$

That the numerator of u is increasing in x reflects the job destruction effect of turnover on currently employed workers; that the denominator is also increasing in x reflects the positive effect a higher turnover rate has on the job finding rate of currently unemployment workers.

The first effect dominates here, with the equilibrium unemployment rate increasing in the turnover rate x. However this very much hinges on the fact that innovative turnover is the only source of job destruction in this baseline model. In Appendix A.1.5, we introduce the possibility of exogenous job destruction on top of innovation-driven job destruction. Then we show that the higher the exogenous rate of job destruction, the more the innovation

[9] In Appendix A.1.4., we discuss the transitional dynamics of this model. We show that following that increase in the entry rate convergence to the steady-state is fast.

rate x contributes to reducing unemployment, and therefore the more positive the overall effect of x on equilibrium wellbeing.

Now we can express the growth rate of the economy.

Lemma 1. *The balanced growth path growth rate of the economy is equal to*

$$g = m \ln \lambda,$$

where m denotes the flow of sectors in which a new innovation is being implemented (i.e., the rate at which new firm-worker matches occur).

Proof. See Appendix A.1.1. □

Then, using the fact that in steady-state equilibrium we have $m = u = \frac{x}{1+x}$,we get the equilibrium growth rate as,

$$g = \frac{x}{1+x} \ln \lambda. \tag{1.7}$$

As expected, the growth rate is increasing in the turnover rate x and with the innovation step size λ.

Equilibrium Valuations and Life Satisfaction

Recall that life satisfaction is the average welfare of an individual employee, namely:[10]

$$W = uU + (1-u)E,$$

where:

$$\begin{aligned} rE - \dot{E} &= \beta\pi Y + x(U - E), \text{ and} && (1.8) \\ rU - \dot{U} &= bY + (m(u,v)/u)(E - U). && (1.9) \end{aligned}$$

[10]Note that in our analysis, life satisfaction is not necessarily equal to the present discounted value of income for at least two reasons. First, even though we labelled b as the unemployment benefit, the interpretation of it is much more general and it can embody in reality the private disutility associated with being unemployed or opportunity cost of not working. Second, our results also hold for the case of risk aversion as we illustrate in Appendix A.1.6, in which case income and life satisfaction are distinct objects.

Now, after substituting for E and U in the expression for the steady-state value of W, and using the fact that in steady state $\dot{E} = gE$ and $\dot{U} = gU$, and that in equilibrium [see equation (1.5)] $m = u = x/(1+x)$, we get the following expression for life satisfaction:[11]

$$W = \frac{Y}{r-g}\left[\beta\pi - \frac{xB}{1+x}\right] \tag{1.10}$$

where

$$g = \frac{x}{1+x}\ln\lambda \text{ and } B \equiv \beta\pi - b. \tag{1.11}$$

From the above expression for W, we see three effects of turnover on life satisfaction. First, for given growth rate g, more turnover increases the probability of an employed worker losing her current job (numerator in $\frac{xB}{1+x}$) which reduces life satisfaction; second, for given growth rate g, more turnover increases the probability of an unemployed worker finding a new job (denominator in $\frac{xB}{1+x}$) which increases life satisfaction; third, higher turnover increases the growth rate g which in turns acts favorably on life satisfaction: this is the *capitalization* effect mentioned in the introduction. The overall effect of turnover on life satisfaction is ambiguous.[12]

Comparative Statics and Additional Discussions

In this section, we discuss the implications of our model.

Unemployment vs Capitalization Effect If we look at the effect of turnover on life satisfaction controlling for unemployment, this effect is unambiguously positive. To see this, after some straightforward algebra we reexpress equilibrium wellbeing W as:

$$W = \frac{Y}{r-g}[ub + (1-u)\beta\pi] \tag{1.12}$$

[11] See Appendix A.1.2 for the detailed derivation of (1.10).

[12] Using the fact that: $\frac{\partial W}{\partial x} = \frac{Y[\beta\pi\ln\lambda - B\rho]}{[(1+x)(\rho-\ln\lambda)+\ln\lambda]^2}$, we see that $\frac{\partial W}{\partial x} > 0$ if and only if $\rho < \frac{\beta\pi\ln\lambda}{B}$.

which for given u is increasing in x since it is increasing in g and g is increasing in x (capitalization effect).[13]

Taking the derivative of (1.10) with respect to x and substituting (1.11) we get:

$$\frac{\partial W}{\partial x} = \frac{Y\left[\beta\pi \ln\lambda - B\rho\right]}{\left[(1+x)\left(\rho - \ln\lambda\right) + \ln\lambda\right]^2},$$

which is clearly positive when $\rho < \frac{\beta\pi \ln\lambda}{B}$, i.e., when capitalization effect dominates the negative unemployment effect. Note also that life satisfaction increases more with turnover x the more generous unemployment benefits are:[14]

$$\frac{\partial^2 W}{\partial x \partial b} > 0.$$

We summarize the above discussion in the following proposition:

Proposition 1. *(i) A higher turnover rate x increases life satisfaction W unambiguously once we control for the unemployment rate, not otherwise; (ii) life satisfaction increases more with turnover x the more generous unemployment benefits are.*

Job Creation vs Job Destruction So far, we have proxied job turnover using a single parameter x. However, we can also write (1.12) in terms of *job creation* and *job destruction rates*. Note that in our model, job creation happens through new matches, which happen at the rate m $(= \mathit{job\_creation})$ and job destruction happens as incumbent firms are replaced by new entrants at the rate x $(= \mathit{job\_destruction})$. Hence we can express (1.12) as

$$W = \frac{Y}{\rho - \ln\lambda \times \mathit{job\_creation}}\left[\beta\pi - B\frac{\mathit{job\_destruction}}{1+\mathit{job\_destruction}}\right] \tag{1.13}$$

Clearly, we obtain the following immediate comparative statics:

$$\frac{\partial W}{\partial \mathit{job\_creation}} > 0, \; \frac{\partial W}{\partial \mathit{job\_destruction}} < 0, \text{ and } \frac{\partial^2 W}{\partial \mathit{job\_destruction}\, \partial b} > 0.$$

[13] See Appendix A.1.3 for the detailed derivation of equation 1.12.

[14] Indeed:

$$\frac{\partial^2 W}{\partial x \partial b} = \frac{Y\rho}{\left[(1+x)\left(\rho - \ln\lambda\right) + \ln\lambda\right]^2} > 0.$$

Next, the following proposition follows.

Proposition 2. *(i) A higher job creation rate increases, whereas a higher job destruction rate decreases life satisfaction W; (ii) life satisfaction decreases less with job destruction the more generous the unemployment benefits.*

Current vs Future Wellbeing and Transitional Dynamics In this section, we discuss briefly the transitional dynamics and its impact on wellbeing. Consider a sudden unexpected increase in the rate of creative destruction from x^{old} to x^{new} such that $x^{new} > x^{old}$. This generates a transition from the old steady state to a new steady state. During this transition, the path of the growth rate is summarized in the following lemma.

Lemma 2. *Consider an initial steady state with a creative destruction rate of x^{old}. Assume that at time $t = 0$, the creative destruction rate becomes x^{new}. Then the growth rate during the transition can be expressed as*

$$g_t = g_{ss}^{new} - e^{-tx^{new}} \left[g_{ss}^{new} - g_{ss}^{old} \right] \tag{1.14}$$

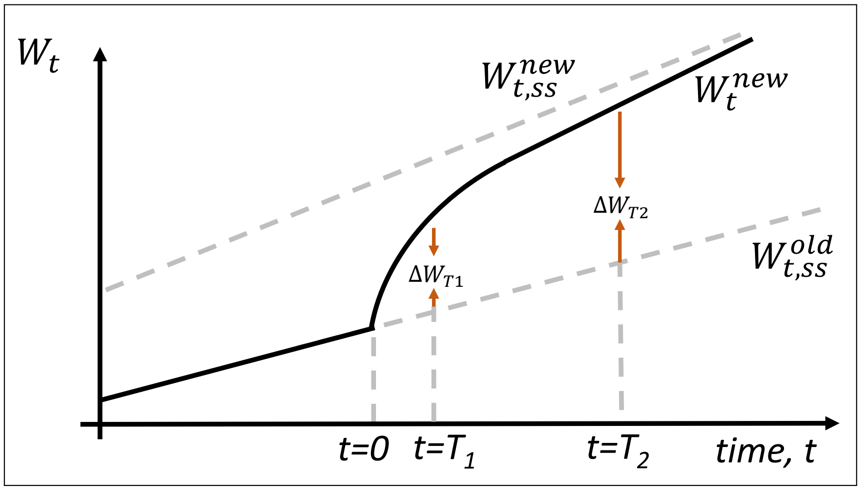
where g^{new} and g^{old} are the new and old steady-state growth rates, respectively and $g^{new} > g^{old}$.

Proof. See Appendix A.1.4. □

Let us denote the wellbeing at time t under x^{old} and x^{new} by W_t^{old} and W_t^{new}, respectively. Moreover, let us denote their respective steady-state trajectories by $W_{t,ss}^{old}$ and $W_{t,ss}^{new}$. Expression (1.14) makes it clear that when there is an increase in creative destruction from x^{old} to $x^{new} > x^{old}$, the growth rate will monotonically converge toward its new level. The impact of this change is illustrated in Figure 1.2. Before time 0, i.e., at $t < 0$, wellbeing is increasing on its trajectory $W_{t,ss}^{old}$. When the turnover rate increases from x^{old} to x^{new} when $t = 0$, wellbeing accelerates and starts to evolve toward its new trajectory $W_t^{new} \to W_{t,ss}^{new}$.

The important point to note here is that the gap between the new trajectory and the old trajectory widens over time. For instance, the gap between $W_t^{new} - W_{t,ss}^{old}$ at time $t = T_1$ is smaller than the gap at time $t = T_2$. This implies that any change in the turnover rate has a bigger impact in the future wellbeing than the current wellbeing. Hence $\Delta W_t \equiv W_t^{new} - W_{t,ss}^{old}$

Figure 1.2: *Wellbeing during transition*



is increasing over time. In this economy, a given individual's expected period-T future wellbeing from a time-zero perspective can be expressed as

$$future\_wellbeing\,(T) = e^{-\rho T} W_T.$$

Clearly, an increase in turnover that increases future wellbeing will be perceived more highly by more patient individuals (with lower discount rate ρ). In Appendix A.1.4, we show that the transition in our model happens very fast. Motivated by this fact, for any given wellbeing path W_t, if we approximate it with its steady-state value $W_T \approx W_{ss}$, we can also show this formally:

$$\begin{aligned} \frac{\partial future\_wellbeing\,(T)}{\partial x} &\approx e^{-\rho T}\frac{\partial W_{ss}}{\partial x} > 0, \text{ and} \\ \frac{\partial^2 future\_wellbeing\,(T)}{\partial x \partial \rho} &\approx -Te^{-\rho T}\frac{\partial W_{ss}}{\partial x} + e^{-\rho T}\frac{\partial^2 W_{ss}}{\partial x \partial \rho} < 0. \end{aligned}$$

In words, future wellbeing increases in creative destruction, and more so for more patient individuals. A nice feature of our wellbeing data is that individuals are asked about their expectation about their future wellbeing as well. This will allow us to directly test this prediction of our model using the "future wellbeing" measure.

Summary and Main Predictions

In the empirical analysis below, we will use cross-MSA data on wellbeing and job turnover to test the following predictions from the model:

Prediction 1: A higher turnover rate increases wellbeing more when controlling for aggregate unemployment than when not controlling for aggregate unemployment.

Prediction 2: A higher job creation rate increases wellbeing, whereas a higher job destruction rate decreases wellbeing.

Prediction 3: A higher turnover rate increases wellbeing more, whereas a higher job destruction rate decreases wellbeing less the more generous the unemployment benefits.

Prediction 4: A higher turnover rate increases future wellbeing more for more forward-looking individuals.

1.3 Empirical Analysis

1.3.1 Data

The data on creative destruction come from the BDS, which provide, at the metropolitan level (MSA), information on job creation and destruction rates. The job creation (destruction) rate is the sum of all employment gains (losses) from expanding (contracting) establishments from year $t-1$ to year t including establishment creations (destructions), divided by the average employment between years t and $t-1$. These rates are computed from the whole universe of firms as described in the Census Longitudinal Business Database. Our main measure of creative destruction is the "job turnover rate", defined as the sum of the job creation and job destruction rates. We also analyze the role of creation rates and destruction rates separately. For our panel analysis, we use an alternative data source, the LEHD constructed by the Census bureau. This dataset varies at the quarterly level, whereas the BDS data vary only at the annual level. The LEHD dataset also allows for a sectoral breakdown, which we take advantage of to construct a predicted Bartik-like measure of turnover that we use as a robustness check. The job creation rate in the LEHD is defined as the estimated number of workers who start a new job in a given quarter divided by the average employment in that quarter. The job destruction rate is defined as the estimated number of workers whose job ended in a given quarter divided by the average employment in that quarter.

The main data source on SWB is the Gallup Healthways Wellbeing Index, which collects data on 1,000 randomly selected Americans each day through phone interviews. The period covered is 2008-2011. Subjective wellbeing in Gallup is assessed through various questions aimed at capturing different dimensions of wellbeing. We focus on the “Cantril ladder of life" questions which are intended to measure the individual’s *evaluation* of her life. Each individual is asked: “Please imagine a ladder with steps numbered from 0 at the bottom to 10 at the top; the top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you; on which step of the ladder would you say you personally feel you stand at this time?"; and then “which level of the ladder do you anticipate to achieve in five years?". We refer to answers to the first question as the “current ladder" and to the second one as the “future ladder". The distinction between current and future ladder measures is particularly interesting, as we recall that some of the predictions, especially Prediction 4, rely mainly on future wellbeing.

To test the robustness of our main results to an alternative measure of wellbeing, we use the life satisfaction measure from the Behavioral Risk Factor and Surveillance System (BRFSS).[15] The sample size is roughly similar to that of Gallup but the BRFSS does not distinguish between current and future wellbeing. Further details on these data are provided in the robustness section.

Additional data sources used are: the Local Area Unemployment Statistics from the Bureau of Labor Statistics for the MSA-level unemployment rate; the FBI Crime Statistics for the MSA crime rates; the Bureau of Economic Analysis for population levels; and the

[15] We prefer to use the life satisfaction measure from BRFSS rather than additional wellbeing measures from Gallup because the latter are destined to capture *emotional* wellbeing (as opposed to *evaluative* wellbeing), whereas the life satisfaction measure, as the Cantril ladder of life, seems better suited to capture our theoretical notion of welfare.

Department of Labor for states' unemployment insurance policies.

The descriptive statistics of our main data can be found in Table 1.1.

Table 1.1: *Summary statistics*

	Observations	Mean	Standard deviation	Min	Max
MSA-level 2008-2011 Averages (Used in Panel A of Table 1.2, 1.3, and 1.4)					
Current ladder	363	6.724	0.192	6.059	7.431
Future ladder	363	7.950	0.195	7.363	8.571
Job turnover rate	363	0.261	0.036	0.165	0.409
Job creation rate	363	0.125	0.018	0.082	0.215
Job destruction rate	363	0.136	0.021	0.082	0.225
Unemployment rate	363	0.083	0.025	0.035	0.275
Log of income	363	8.127	0.164	7.517	8.605
Share African Americans	363	0.102	0.102	0	0.454
Population (in thousands)	355	726.5	1,621	55.24	19,533
Crime rate(/100,000 inhab.)	352	401.4	176.9	65.33	1085
Unemp. insurance generosity	363	1.759	0.520	0.624	2.931
Individual-level Data, 2008-2011(Used in Panel B of Table 1.2, 1.3, and 1.4, and Table A.11)					
Current ladder	556,719	6.722	1.950	0	10
Future ladder	544,620	8.032	1.983	0	10
Female	556,719	0.491	0.500	0	1
Age	556,719	39.83	11.91	18	60
Black	556,719	0.126	0.332	0	1
Asian	556,719	0.026	0.158	0	1
White	556,719	0.725	0.447	0	1
Married or living with partner	556,719	0.592	0.492	0	1
Average years of schooling	556,719	14.18	2.346	10	18
Monthly household income	556,719	5,709	5,033	347.1	16,483
Log income	556,719	8.234	0.984	5.850	9.710
Quarterly Panel Data (Used in Panel C of Table 1.2 and 1.3)					
Job turnover rate	5,704	0.326	0.093	0.141	1.815
Job creation rate	5,704	0.146	0.045	0.053	0.894
Job destruction rate	5,704	0.180	0.053	0.080	0.929

1.3.2 Estimation Framework

Our measure of creative destruction varies at the MSA level, thus we estimate MSA-level regressions. However, in order to take advantage of our micro-level data on SWB, we also perform individual-level regressions that allow us to have a richer and more meaningful set of controls. Individual characteristics such as marital status do not vary much if we aggregate them at the MSA level, yet they are important determinants of wellbeing at the individual level. In all cases, regressions are OLS. We restrict the analysis to working age individuals (18-60 years old) to be closer to the model in which individuals are either employed or unemployed.[16,17]

At the MSA level, we look at purely cross-sectional regressions, where we average our SWB data at the MSA level and across our sample years.[18] In all specifications we control for MSA-level averages of the Gallup respondents' income. Income is measured in terms of household income brackets. We calculate the midpoints of these brackets assuming that income is log-normally distributed and we then average at the MSA level these log midpoints.[19] In our regressions, we also explore what happens when we add MSA-level potential confounders such as crime rate, the share of African Americans, and population.

At the individual level, we perform regressions where we control for individual characteristics such as education, income, and ethnicity, as well as gender, marital status, and age. Our specification is as follows:

$$SWB_{i,m,t} = \alpha \times X_{m,t} + \beta \times Y_{m,t} + \delta \times Z_{i,t} + T_t + \epsilon_{i,t}, \tag{1.15}$$

where $SWB_{m,t}$ is SWB for individual i who lives in MSA m in year t. This measure is derived through the current ladder question or the future ladder question in the Gallup survey. $X_{m,t}$ is either the job turnover rate and the unemployment rate in MSA m in year t (Prediction 1), or the job creation and the job destruction rates introduced separately (Prediction 2).

[16] However, we performed all the regressions for the whole population as well, which yields very similar results, though with slightly smaller coefficients.

[17] We cannot run separate regressions for the employed and the unemployed as we do not have access to consistent measures of employment and unemployment either in Gallup or in the BRFSS.

[18] Sample years are 2008-2011 for the main analysis using Gallup data, and 2005-2010 when using the BRFSS data in the Appendix, which we then also decompose into 2005-2007 and 2008-2010

[19] We also checked that our results are unchanged when using the log of MSA-level income per capita as measured by the Bureau of Economic Analysis and averaged over the relevant period.

$Y_{m,t}$ are MSA-level controls, such as the population level in year t, the crime rate, and the share of African-Americans. $Z_{i,t}$ are individual-level controls: gender, age, age square, 4 race dummies, 6 education dummies, 6 family status dummies, and 9 dummies for income brackets. T_t are year and month fixed effects. Finally, $\epsilon_{m,t}$ is the error term. A constant is also included and standard errors are clustered at the MSA level. When testing Prediction 3, we interact our creative destruction proxies with a measure of the generosity of the state's unemployment insurance. When testing Prediction 4, we interact the job creation and the job destruction rates with proxies for the individual discount rate (age, education, and income). Robustness checks are discussed below in Section 1.4.2.

1.3.3 Testing Prediction 1

In this section, we test Prediction 1: *A higher turnover rate increases wellbeing more when controlling for aggregate unemployment than when not controlling for aggregate unemployment.*

Recall that the model highlights two opposite forces whereby creative destruction impacts SWB: the negative effect that comes from the higher risk of unemployment through job destruction and the positive growth effect through new job creation. Controlling for the unemployment rate should capture part of the negative force of creative destruction and thus lead to a more positive coefficient of creative destruction on wellbeing than without the control for unemployment.

MSA-level Results Before displaying the regression results, we show two scattered plots where one observation corresponds to an MSA. Figure 1.3 plots the MSA's average life satisfaction on MSA-level job turnover. We then regress these MSA-level life satisfaction and job turnover variables on the MSA's unemployment rate and plot the residuals in Figure 1.4. We see that wellbeing is more strongly positively associated with job turnover, once we control for the unemployment rate.

Now moving to the regression results, Table 1.2 Panel A shows the results from baseline OLS regressions at the MSA level. We see that the job turnover has a positive but statisti-

Figure 1.3: *Simple scatter plot*

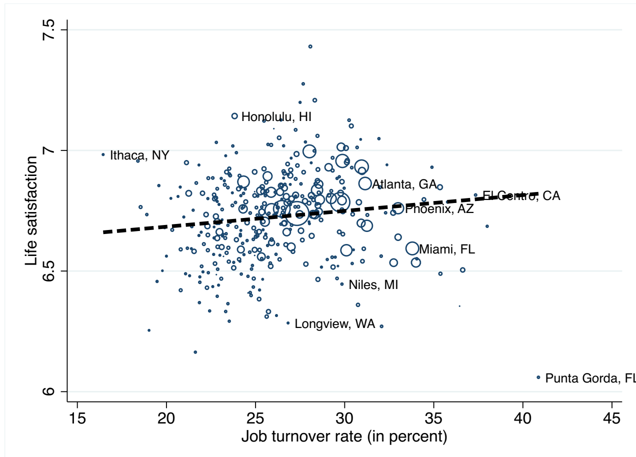
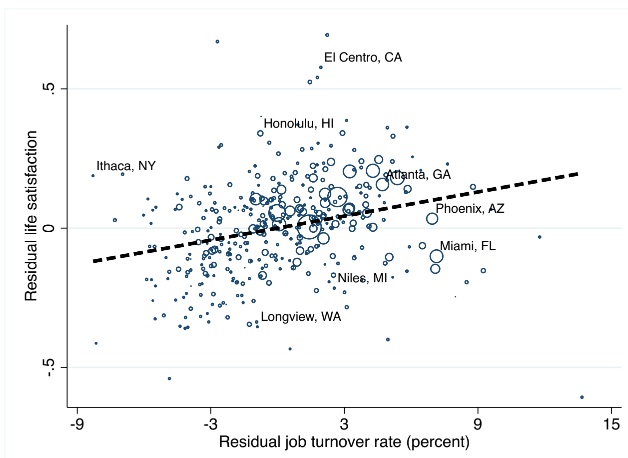


Figure 1.4: *Residual scatter plot*



cally insignificant effect on current wellbeing. Column 2 shows that once we control for unemployment, job turnover has an effect on wellbeing that is more than twice as large and strongly statistically significant. This is in line with our model which predicts that, controlling for unemployment, turnover should have a more positive effect on wellbeing as it implies higher growth and a higher probability for currently unemployed workers of finding a new job.[20] In Column 3, we add additional MSA controls: population, crime

[20]The correlation between the MSA-level average job turnover rates over the period 2008-2011 and the

rate, and share of African Americans.[21] We see that these potential confounders do not significantly affect the coefficients of the creative destruction variable.

Table 1.2: *Test of prediction 1*

	(1)	(2)	(3)	(4)
VARIABLES		Current ladder		Future ladder
Panel A: MSA-level Analysis				
Job turnover rate	0.599	1.288	1.322	1.726
	(0.361)	(0.410)	(0.424)	(0.306)
Unemployment rate		-2.727	-2.581	-0.930
		(0.786)	(0.823)	(0.507)
Log of income	0.342	0.195	0.225	0.297
	(0.0839)	(0.088)	(0.106)	(0.079)
Additional MSA controls			x	x
Observations	363	363	344	344
R-squared	0.100	0.198	0.217	0.459
p-value Job Turnover [1]= Job Turnover [2]		0.000		
Panel B: Individual-level Analysis				
Job turnover rate	0.0676	0.521	0.611	0.984
	(0.236)	(0.237)	(0.285)	(0.148)
Unemployment rate		-2.299	-2.168	-0.0857
		(0.443)	(0.502)	(0.298)
MSA-level log of income	-0.187	-0.285	-0.263	-0.0424
	(0.048)	(0.046)	(0.051)	(0.038)
Additional MSA controls			x	x
Individual controls (incl. income)	x	x	x	x
Year and Month F.E.	x	x	x	x
Observations	556,300	556,300	461,054	450,908
R-squared	0.103	0.104	0.103	0.095
p-value Job Turnover [1]= Job Turnover [2]		0.000		

MSA-level average unemployment rates over the same period, is equal to 0.344.

[21] The share of African Americans is a weighted average of the number of respondents in the surveys that report being black in the race question. Weights use to compute the weighted average are those attached to the respondent by Gallup.

Table 1.2 continued

Panel C: Panel Analysis				
Job turnover rate	0.678	0.787	0.249	0.234
	(0.0970)	(0.105)	(0.142)	(0.140)
Unemployment rate		-2.238	-1.743	-1.074
		(0.301)	(1.054)	(1.057)
Log of income	0.410	0.360	0.412	0.192
	(0.0301)	(0.0307)	(0.0373)	(0.0390)
MSA F.E.			x	x
Additional MSA controls	x	x	x	x
Year and quarter F.E	x	x	x	x
Observations	4,884	4,884	4,884	4,884
R-squared	0.189	0.203	0.325	0.256
p-value Job Turnover [1]= Job Turnover [2]		0.041		

Note : The dependent variables are SWB measures from Gallup: columns (1) through (3) use the *current* Cantril ladder of life whereas column (4) uses the *future* ladder of life. Sample years are 2008-2011 and the sample is restricted to working-age respondents.
Column (1) regresses SWB on the job turnover rate, which comes in Panel A and B, from the BDS, and in panel C, from the LEHD which provide data at the quarterly level. Column (2) adds a control for the MSA-level unemployment rate. Columns (3) and (4) further add some MSA-level controls: population (in levels), crime rates, and share of African-Americans. All specifications control for income at the MSA-level. All data sources and variables definitions are in the main text.
Panel A carries a cross-sectional analysis at the MSA-level, where the variables are averaged across years within each MSA. The SWB measures are averaged using the weights attached by Gallup to each respondent.
Panel B carries a repeated cross-section analysis at the individual-level. Month and Year fixed effects are added to the specification as well as individual controls: age, age square, a dummy for being female, 6 dummies for family status, 6 dummies for education, and 4 race dummies (black, asian, white, other or missing), as well as 9 dummies for household income brackets. Regressions are weighted by individual weights attached by Gallup to each respondent.
Panel C shows the results of a quarterly panel analysis at the MSA-level. The SWB measures are averaged at the MSA-quarter level using the weights attached by Gallup to each respondent. All regressions include year and quarter fixed effects, quarterly controls for the MSA's average log of income, and the share of African Americans in the MSA, as well as annual controls for population levels and crime rates. Columns (3) and (4) add MSA fixed effects.

The difference between the estimates on creative destruction in Column 1 versus 2 and 3 is statistically significant at the 1% level. The last row in each Panel of Table 1.2 reports the p-value associated with a Wald test of the hypothesis that $\alpha_{\text{CD;column1}} = \alpha_{\text{CD;column2}}$

Column 4 repeats the same specification as column 3 but with future wellbeing as a dependent variable. We first see that job turnover has a stronger effect on the future ladder than on the current Cantril ladder. This in turn points to the notion that individuals disentangle the short-run losses from becoming unemployed as a result of job turnover from the long-term gains associated with higher growth and more new job opportunities in the future.

The magnitude of the effect of creative destruction on current life satisfaction is in the same ballpark as that of the effect of the unemployment rate. In particular, moving from an MSA which is at the 25th percentile in terms of its level of creative destruction (i.e., with a job creation rate + destruction rate at 23.5%) to an MSA at the 75th percentile (i.e., with a job creation rate plus job destruction rate at 28.3%) is associated with an increase in the current ladder of life of 0.06 points (Column 2 in Table 1.2, Panel A). As a benchmark, looking at the same regression, moving from the 75th to the 25th percentile in terms of the unemployment rate (that is, from a 9.4% to a 6.7% unemployment rate) is associated with an increase in life satisfaction of 0.07 points. Another way to put it is that a one standard deviation increase in job turnover increases the current ladder by 0.25 standard deviation: that effect is equivalent to a 0.7 ($= (0.036 \times 1.288/2.727)/0.025$) standard deviation decrease in the MSA-level unemployment rate.

Individual-level Results In Table 1.2 Panel B we perform individual-level regressions and find qualitatively similar results as in Panel A. The difference is that we now also control for individual-level characteristics and for year and month fixed effects. We thus control for household income brackets and we keep the control for the MSA-level log of income. Note that the MSA-level income has a negative impact on wellbeing once we control for individual-level income, which suggests that wellbeing depends on relative income of an individual. The creative destruction variable now varies at the MSA-year level.

We can still reject at the 1% level that the coefficient of job turnover on wellbeing is the same whether or not we control for the unemployment rate. We also see that the effect of turnover is stronger on the future ladder than on the current ladder.

The magnitude of the creative destruction effect is smaller to that displayed at the MSA level. A one standard deviation increase in job turnover has an effect on the current ladder of life which is equivalent to a 0.3 standard deviation increase in the MSA-level unemployment rate ($= (0.036 \times 0.521/2.299)/0.025$) .

Panel Results In Panel C, we show results from quarterly panel regressions with year and quarter fixed effects, with and without MSA fixed effects. However, we want to stress why we have a preference for the cross-sectional analysis. The theoretical concept of creative destruction is being proxied in our empirical analysis by a job turnover variable. So we are proxying $x(t)$ by $x^*(t)$ which is equal to $x(t)$ plus some measurement (or proxy) error $\epsilon(t)$. Adding MSA fixed effects into the regression changes in an unfavorable way the relative variances of the signal, the variance of $x(t)$, and the noise, the variance of $\epsilon(t)$. If the job destruction variable changes only slowly over time within each MSA, which is the case here, looking at the deviation of job destruction from its MSA time-mean is going to be problematic, as more of that deviation is going to come from the proxy error, not from the variable itself. Hence our predictions are better captured by cross-sectional regressions than by panel regressions that cover such short time periods.

Because our sample period is very short, we use a quarterly frequency to look at panel specifications. Thus we use the LEHD dataset constructed by the Census Bureau, based on the Quarterly Census of Employment and Wages and other administrative and survey data. Indeed these data contain information on employment, earnings, and job flows at the MSA and quarterly level. In terms of creative destruction: rather than job creations and destructions, the data give us the number of hires and separations. To compute the turnover rates, we divide these hires or separations by the average stock of employment in that quarter. The results are reported in Table 1.2 Panel C.

If we compare column 1 to column 2, we see again that the coefficient for job turnover is higher when we control for the MSA-level unemployment rate than when we do not. The difference is significant at the 5% level. These two columns are without MSA fixed effects. When we add MSA fixed effects (Column 3), the coefficient of job turnover is still significantly positive although of a smaller magnitude. Note that all the specifications in Panel C control for time-varying potential MSA-level controls: population levels, crime rates, share of African Americans.

1.3.4 Testing Prediction 2

In this section, we test Prediction 2: *A higher job creation rate increases wellbeing, whereas a higher job destruction rate decreases wellbeing.*

MSA-level Results Table 1.3 Panel A shows the results from the baseline OLS regressions at the MSA level. The first two columns use current ladder whereas the last two columns use the future ladder as dependent variables. In the first and third columns, we see the positive effect of job creation and the negative effect of job destruction on current and future wellbeing which are very much in line with Prediction 2. Columns 2 and 4 introduce additional MSA-level confounders: the MSA's average population level over the period, its average crime rate and its average share of African Americans. These controls do not change the pattern: overall, job creation and destruction have opposite effects on wellbeing, as the theory predicted.

Table 1.3: *Test of prediction 2*

	(1)	(2)	(3)	(4)
VARIABLES	Current ladder		Future ladder	
Panel A: MSA-level Analysis				
Job creation rate	5.486	5.567	3.588	3.103
	(0.978)	(1.015)	(0.825)	(0.682)
Job destruction rate	-3.586	-3.433	-0.158	0.144
	(0.838)	(0.870)	(0.702)	(0.668)
Log of income	0.277	0.293	0.221	0.324
	(0.077)	(0.094)	(0.061)	(0.073)
Additional MSA controls		x		x
Observations	363	344	363	344
R-squared	0.218	0.246	0.149	0.460

Table 1.3 continued

Panel B: Individual-level Analysis

Job creation rate	1.098	1.274	1.068	0.944
	(0.395)	(0.445)	(0.206)	(0.220)
Job destruction rate	-0.791	-0.702	0.926	0.987
	(0.274)	(0.306)	(0.197)	(0.225)
MSA log of income	-0.197	-0.173	-0.0408	-0.0382
	(0.046)	(0.048)	(0.031)	(0.038)
Additional MSA controls		x		x
Individual controls (incl. income)	x	x	x	x
Year and Month F.E.	x	x	x	x
Observations	556,300	461,054	544,228	450,908
R-squared	0.103	0.103	0.094	0.095

Panel C: Panel Analysis

Job creation rate	2.276	1.213	1.690	1.155
	(0.316)	(0.357)	(0.293)	(0.349)
Job destruction rate	-0.617	-0.466	-0.647	-0.460
	(0.274)	(0.314)	(0.249)	(0.285)
Log of income	0.416	0.416	0.266	0.195
	(0.0299)	(0.0371)	(0.0304)	(0.0389)
MSA F.E.		x		x
Additional MSA controls	x	x	x	x
Year and quarter F.E	x	x	x	x
Observations	4,884	4,884	4,884	4,884
R-squared	0.195	0.325	0.145	0.257

Note: The dependent variables are SWB measures from Gallup: columns (1) and (2) use the *current* Cantril ladder of life whereas columns (3) and (4) use the *future* ladder of life. Sample years are 2008-2011 and the sample is restricted to working-age respondents. Columns (1) and (3) regress these life satisfaction measures on the job creation and the job destruction rates, which come, in Panel A and B, from the BDS, and in panel C, from the LEHD which provide data at the quarterly level. All specifications control for income at the MSA-level. Columns (2) and (4) add some MSA-level controls: the unemployment rate, population (in levels), crime rates, and share of African-Americans. All data sources and variables definitions are in the main text.
Panel A carries a cross-sectional analysis at the MSA-level, where the variables are averaged across years within each MSA. The SWB measures are averaged using the weights attached by Gallup to each respondent.
Panel B carries a repeated cross-section analysis at the individual-level. Month and Year fixed effects are added to the specification as well as individual controls: age, age square, a dummy for being female, 6 dummies for family status, 6 dummies for education, and 4 race dummies (black, asian, white, other or missing), as well as 9 dummies for household income brackets. Regressions are weighted by individual weights attached by Gallup to each respondent.
Panel C shows the results of a quarterly panel analysis at the MSA level. The SWB measures are averaged at the MSA-quarter level using the weights attached by Gallup to each respondent. All regressions include year and quarter fixed effects, quarterly controls for the MSA's average log of income, and the share of African Americans in the MSA, as well as annual controls for population levels and crime rates. Columns (2) and (4) add MSA fixed effects.

Now, consider the magnitudes of the various effects. A one standard deviation increase in the job creation rate is associated with an increase in the current ladder of life of slightly more than half a standard deviation ($= 0.018 \times 5.567/0.192$). A one standard deviation increase in the job destruction rate is associated with a decrease in the current ladder of life of 0.4 ($= 0.021 \times 3.433/0.192$) standard deviations.

Individual-level Results In Table 1.3 Panel B we perform individual-level regressions and find qualitatively similar results as in Panel A. Again, we control for many demographic characteristics as well as income brackets. All specifications include year and month fixed effects as well as a control for the MSA's log of income. The job creation and destruction rates vary at the MSA-year level. Standard errors are clustered at the MSA level. Again, columns 2 and 4 are similar to columns 1 and 3 except that additional MSA-level potential confounders are added. We see that these additional controls barely change the coefficient on the job creation and destruction rates. Similar to Prediction 1, the magnitudes are smaller than for the MSA-level results.

Panel Results In Panel C, we show results of panel specifications using quarterly data on job creation and destruction rates coming from the LEHD, as in Panel C of Table 1.2. Columns 1 and 3 are without MSA fixed effects, whereas columns 2 and 4 are with MSA fixed effects. All specifications include year and quarter fixed effects as well as MSA-level potential confounders such as share of African Americans, population level, and crime rate. Prediction 2 remains verified in panel analysis, with a positive effect of the job creation rate on SWB and a negative effect of the job destruction rate.

1.3.5 Testing Prediction 3

In this section, we test Prediction 3: *A higher turnover rate increases wellbeing more, whereas a higher job destruction rate decreases wellbeing less, the more generous the unemployment benefits.*

The generosity of unemployment insurance (UI) varies at the state level. To avoid the endogeneity of the total number of benefits claimed, we use, as is standard in the literature, the maximum weekly benefit amount as a measure of the state's UI generosity. We normalize it by the average taxable wage. Our results are robust to whether or not we do this normalization. Panel A of Table 1.4 carries the analysis at the MSA-level whereas Panel B shows the results when using individual level regressions, controlling for the same individual characteristics used in Panels B of Table 1.2 and 1.3. The coefficients of interest are that of the interaction term between job turnover and UI generosity, as well as that of the interaction term between job destruction and UI generosity. Indeed we expect the effect of UI generosity to alleviate the negative effect of the job destruction rate by making the risk of unemployment less costly. On the contrary, there is no clear prediction on how UI generosity should interact with the job creation rate. Thus we do not report the interaction terms and main effect of the job creation rate although these variables are included in all the specifications that feature the job destruction rate (i.e., columns 3 and 4). Note that we demean our measure of the state's UI generosity such that the coefficients for the main effect of the job turnover or the job destruction rate show the effect for MSAs located in a state where the UI generosity is at its mean value.

MSA-level Results We see that the effect of the job turnover rate on SWB at the mean value of UI generosity is positive and that the coefficient of the interaction term between job turnover and the generosity of UI is significantly positive and economically significant (column 1, Table 1.4 Panel A). A one standard deviation increase in UI generosity increases the positive effect of job turnover by almost 100% $(= 0.520 \times 0.989/0.524)$. This remains true in column 2 when we control for the same potential MSA-level confounders as in Table 1.2 and 1.3. This more positive effect of turnover on SWB in MSA located in states with more generous UI is driven by a much less negative effect of the job destruction rate. Indeed we see in column 3 that the direct effect of job destruction on wellbeing is negative but the interaction term between the job destruction rate and the generosity of UI is significantly positive. A one standard deviation increase in our measure of UI generosity reduces the negative effect of the job destruction rate by 33% ($= 0.52 \times 2.357/3.661$). Thus the effect of

UI generosity is not only statistically significant but also economically so.

Table 1.4: *Test of prediction 3*

	(1)	(2)	(3)	(4)
VARIABLES		Current ladder		
		Panel A: MSA-level Analysis		
Job turnover rate	0.524	0.662		
	(0.362)	(0.378)		
Job turnover × UI generosity	0.989	0.897		
	(0.422)	(0.416)		
Job destruction rate			-3.661	-3.536
			(0.789)	(0.816)
Job destruction × UI generosity			2.357	2.369
			(1.105)	(1.113)
UI generosity	-0.288	-0.253	-0.167	-0.137
	(0.114)	(0.113)	(0.128)	(0.128)
Additional MSA controls		x		x
Observations	363	344	363	344
R-squared	0.116	0.136	0.237	0.262
		Panel B: Individual-level Analysis		
Job turnover rate	0.0845	0.209		
	(0.230)	(0.262)		
Job turnover × UI generosity	0.675	0.670		
	(0.310)	(0.357)		
Job destruction rate			-0.794	-0.720
			(0.272)	(0.300)
Job destruction × UI generosity			0.620	0.673
			(0.329)	(0.372)
UI generosity	-0.198	-0.183	-0.212	-0.200
	(0.085)	(0.096)	(0.083)	(0.094)
Individual controls (incl. income)	x	x	x	x
Year and Month F.E.	x	x	x	x
Additional MSA controls		x		x
Observations	556,300	461,054	556,300	461,054
R-squared	0.103	0.103	0.104	0.103

Note: Panel A carries a cross-sectional analysis at the MSA-level, where the variables are averaged across years within each MSA, whereas Panel B carries a repeated cross-section analysis at the individual-level. The first two columns are similar to the columns (1) and (3) of Table 1.2, and the last two columns are similar to the first two columns of Table 1.3, except that the creative destruction variables (job turnover, job creation, and destruction rates) are interacted with state-level UI generosity. UI generosity is measured by the average maximum weekly benefit amount over the period 2008-2011 normalized by the average taxable wage in covered employment. The variable is demeaned. We don't report the interaction coefficient for job creation as the interaction of interest is the job destruction one (see main text).

Individual-level Results The individual-level results of Panel B shows the exact same pattern as the MSA-level results of Panel A. We still have a significantly positive coefficient for the interaction terms of both job turnover and UI generosity (columns 1 and 2) as well as job destruction and UI generosity (columns 3 and 4). Results are barely affected by the addition of potential MSA-level confounders. And the magnitude of the interaction effect is roughly similar to that displayed at the MSA-level: For instance in column 3, a one standard deviation increase in our measure of UI generosity reduces the negative effect of the job destruction rate by 40% $(= 0.52 \times 0.62/0.794)$.

1.3.6 Testing Prediction 4

In the Appendix A.2.1, we test Prediction 4: *A higher turnover rate increases future wellbeing more for more forward-looking individuals.*

It is hard to find a direct measure of the discount rate. The literature on this subject found that old individuals, educated individuals, and rich individuals tend to be more patient [Gilman (1976), Black (1983), Lawrance (1991), Warner and Pleeter (2001)]. In the Appendix Table A.11 we use age, education, and income to proxy for individuals with different patience levels. This exercise should be seen as a first step, as our data does not provide a measure of patience which is fully convincing on its own. Further tests of this prediction are left to future work to be conducted with better measures of patience.

1.4 Robustness Checks and Extensions

In this section, we discuss various theoretical and empirical robustness checks and extensions.

1.4.1 Theory

Transitional Dynamics In Appendix A.1.4, we consider the transitional dynamics. More specifically, we look at the dynamic impact of a sudden increase in the entry rate. In particular we show that: (i) following such an increase in the entry rate convergence to the steady-state is fast; (ii) the big change in welfare occurs at the time of the increase in entry rate, and: (iii) the comparative statics on this change are quite similar to the comparative statics on steady-state welfare stated in Proposition 1. Thus there is no loss of insight in restricting attention to the steady state in our analysis in general.

Exogenous Job Destruction In the current model, the creative destruction rate x generates both job destruction for the workers in incumbent firms and job creation for currently unemployed workers. When the job destruction is fully endogenous, as it currently is, the former effect dominates and the equilibrium unemployment rate $u(x)$ is increases in x as in (1.6). In Appendix A.1.5, we extend the model so as to also allow for exogenous job destruction. There we show that the higher the exogenous rate of job destruction, the more will the innovation rate x contribute to reducing unemployment (the latter effect dominates), and therefore the more positive the overall effect of x on equilibrium wellbeing.

Risk Aversion The analysis in this section can be straightforwardly extended to the case where individuals are risk averse. In Appendix A.1.6 we show that when agents are risk averse, job loss is perceived more detrimentally than when they are risk neutral. Consequently, there is a range of unemployment benefits for which higher turnover reduces life satisfaction for risk-averse individuals with log preferences whereas it would increase life satisfaction for risk-neutral individuals.

Endogenous Entry In Appendix A.1.7, we extend the model to endogenize entry. This in turn enriches our analysis of the relationship between wellbeing and the determinants of creative destruction. In particular, a lower entry cost will have the same effects on wellbeing as the effects of an increase in x, but an increase in the size of innovations will enhance both the growth effect for given x and the creative destruction effect (it will foster x).

Matching Efficiency In Appendix A.1.8, we generalize the model by introducing a multiplicative parameter which reflects the efficiency of the matching process. This allows us to look at what happens when the costs of unemployment are incurred over longer expected time periods. We find that as the cost of unemployment has a longer impact (the productivity of matching declines), the negative impact of innovation on wellbeing through unemployment is amplified.

Taxing Labor to Finance UI Benefits In Appendix A.1.9, we consider a generalized version of our setting where the unemployment benefit is financed by raising taxes on labor income and corporate profits. We show that our results on unemployment benefit is robust to this generalization.

1.4.2 Empirics

We perform several robustness checks to confirm the validity of our empirical results on the main predictions. More details are provided in the Appendix.

BRFSS Data First we look at what happens when we use an alternative measure of SWB. Table A.2 and A.3 use the life satisfaction measure from the BRFSS. In BRFSS, life satisfaction is measured using the question: “In general how satisfied are you with your life?" The possible answers are: “Very satisfied"; “Satisfied"; “Dissatisfied"; “Very dissatisfied". We recode these answers so that “Very dissatisfied" corresponds to grade 1 and “Very satisfied" to grade 4.

Table A.2 tests for Prediction 1 and shows that, even when using the BRFSS measure of SWB, the effect of MSA job turnover on wellbeing is more positive when we control for the unemployment rate than when we do not. The difference is statistically significant at the 1% level. Table A.3 tests for Prediction 2 and confirms a significant positive effect of job creation on life satisfaction and negative effect of job destruction, also with this alternative measure of our left-hand side variable.

Effect of the 2008 Crisis In the BRFSS, the life satisfaction measure starts in 2005 instead of 2008 for the Gallup variables. Thus BRFSS allows us to check that our results are robust to restricting attention to different subperiods. In particular, to deal with the concern that the period post 2008 is a period following a major recession, where all kinds of other things were going on in developed economies, in Table A.4 and A.5 we decompose the overall BRFSS sample period into the subperiods 2005-2007 and 2008-2010. Table A.4 tests for Prediction 1 and Table A.5 tests for Prediction 2.

In Table A.4 we find that the effect of turnover on life satisfaction is less positive during the crisis years than in the period before, but this difference disappears when controlling for unemployment. In Table A.5 we find that job creation (resp. job destruction) has a more positive (resp. negative) effect on wellbeing before the crises years.

Alternative Definition of Creative Destruction We then look at what happens when we use an alternative measure of creative destruction. In Tables A.6 and A.7 we use the measure that comes from the LEHD, which we also use in our main panel analysis because of its quarterly nature. Table A.6 reproduces the baseline MSA cross-sectional specification of Panel A Table 1.2 (testing for Prediction 1) but using this LEHD measure of job turnover, whereas Table A.7 reproduces the baseline MSA cross-sectional specification of Panel A Table 1.3 (testing for Prediction 2) but using the LEHD measure of job creation and job destruction. We see that both Predictions 1 and 2 are still verified in cross-section when we use these other measures of creative destruction. Indeed the effect of job turnover as measured by the LEHD on the current ladder of life is more positive when we control for the unemployment rate than when we do not and the difference is statistically significant at the 5% level. The effect on wellbeing of the job creation rate as measured by LEHD is significantly positive and that of the destruction rate is significantly negative.

Non-linearity of Unemployment In Table A.8, we show that Prediction 1 is robust to controlling non-linearly for the unemployment rate. Indeed we introduce a cubic polynomial of the unemployment rate instead of just the unemployment rate to the baseline specification of Panel A Table 1.2. We still have that the effect of job turnover in column 2, i.e., when controlling for the unemployment rate, here non-linearly, is statistically different at the 1% level from the effect of job turnover in column 1, when we do not control for unemployment. Since the unemployment rate does not play any role in Prediction 2, we only test the robustness to non-linear control for the unemployment rate for Prediction 1.

Bartik Analysis The last and important robustness check we perform, in Tables A.9 and A.10, aims at alleviating a potential endogeneity concern. Indeed, to abstract from the effects of local changes in industry composition, or from the effects of purely local shocks that could get mixed up with variations in local turnover, we construct a "predicted measure" (or Bartik-type measure) of creative destruction as follows:

$$\widehat{CD}_{m,t} = \sum_{j} \omega_{j,m,2004} \times CD_{j,m,USA,t}$$

For each MSA m in quarter-year t : (i) the predicted level of Creative Destruction, $\widehat{CD}_{m,t}$, is computed by taking a weighted average of countrywide sectoral turnover measures in quarter-year t; (ii) $CD_{j,m,USA,t}$ is the country-wide average creative destruction in sector j leaving out MSA m; (iii) the weights $\omega_{j,m,2004}$ are determined by the sectoral structure in the MSA in 2004 (sectors are 2 digit NAICS).

Thus, we reproduce the MSA-level quarterly panel regressions of panels C in Tables 1.2 and 1.3 but replacing the direct local turnover variable by its predicted value $\widehat{CD}_{m,t}$. Standard errors are still clustered at the MSA level.[22] The results turn out to be quite similar when using the predicted measure of turnover instead of the actual quarterly turnover rate as the right-hand side variable. In particular, for Prediction 1, the coefficient for job turnover is larger when we control for unemployment than when we do not and the difference is significant at the 1% level. The coefficient remains positive and significant when we add MSA fixed-effects. Interestingly the MSA fixed-effects do not make the coefficient decrease

[22]If we assume that the sectoral composition in an MSA in 2004 has no direct effect on SWB in that same MSA in 2008-2011, we could use our predicted measure of creative destruction as an instrument to try and get at whether the effect of creative destruction on SWB is causal.

as much as when using the actual turnover rate as the main right-hand side variable. Table A.10 shows that the effect of job creation and job destruction, when captured by these predicted measures, are still significantly positive and negative, respectively.

1.5 Conclusion

In this paper we have analyzed the relationship between turnover-driven growth and SWB, using cross-sectional MSA level US data. We have first built a Schumpeterian model of growth and unemployment to make predictions on how job and firm turnover affect wellbeing under various circumstances. Our main empirical findings are consistent with the theory: namely: (i) the effect of creative destruction on wellbeing is unambiguously positive if we control for unemployment, less so if we do not; (ii) job creation has a positive and job destruction has a negative impact on wellbeing; (iii) job destruction has a less negative impact in MSA within states with more generous unemployment insurance policies; (iv) job creation has a more positive effect on individuals that are more forward-looking. We see these findings not just as a test of the Schumpeterian theory of growth, creative destruction, and unemployment, but also of the usefulness of current and future wellbeing measures.

This is the first step of a broader research project on innovation-led growth and wellbeing. A first avenue forward could be to use a similar combination of the theory and of cross-section analysis to investigate other potential determinants of wellbeing and compare them with the determinants of (per capita) GDP growth. A second extension would be to look at how the relationship between turnover and wellbeing is affected by individual characteristics and by characteristics of labor markets and labor market policy (e.g., training systems and availability of vocational education). A third extension would be to look for policy shocks (e.g., labor market reforms) that may affect the relationship between creative destruction and wellbeing. These and other extensions of the analysis in this paper are left for future research.

Chapter 2

Unemployment Insurance and Reservation Wages: Evidence from Administrative Data [1]

2.1 Introduction

In standard job search models, unemployed workers receive job offers that they accept if the value of the offered job is higher than the value of unemployment (McCall, 1970). Their search strategy can be summarized by one key concept, the *reservation wage*, which is the lowest wage of an acceptable job offer. Job search models predict that more generous unemployment insurance (UI) increases the value of unemployment, and thus also increases reservation wages. This selectivity channel pushes up accepted wages. However, unemployment insurance also lengthens unemployment and, as job prospects decrease with unemployment duration, this negative-duration-dependence channel drives down accepted wages. Which channel dominates is a priori ambiguous and may depend on the context. A recent strand of the empirical literature documents modest – either positive or negative – UI effects on accepted wages (Card *et al.*, 2007; Schmieder *et al.*, 2012b, 2016; Le Barbanchon,

[1] Co-authored with Thomas Le Barbanchon and Roland Rathelot

2016; Nekoei and Weber, 2017).[2] However, the existing literature has no precise estimate of the UI effect on reservation wages only, which corresponds to the selectivity channel. Our paper contributes to filling this gap.

We take advantage of unique administrative data on reservation wages and of a quasi-experimental research design. In France, when newly unemployed job seekers register at the public employment service, they have to declare their reservation wage and other information on the job they are looking for, such as commuting time/distance, desired number of hours and type of labor contract (temporary vs. long-term). Our main identification strategy relies on a reform that altered the Potential Benefit Duration (PBD) for some claimants groups while leaving it unchanged for some others, depending on their previous work tenure. Using this natural experiment, we compute difference-in-difference estimates of the elasticity of reservation wages with respect to PBD. Our results point to the lack of responsiveness of reservation wages and other dimensions of job selectivity to the potential duration of benefits. We obtain very similar results using an alternative identification strategy, based on the discontinuity of the PBD schedule with age.

While the previous literature on reservation wages is based on survey data (Feldstein and Poterba, 1984; Koenig *et al.*, 2014; Krueger and Mueller, 2016), we use administrative data on reservation wages. Our data have thus several strengths: large sample size, no missing values due to non-response and precise measures of UI-related policy variables and past labor outcomes, such as past tenure or past wages. On top of these strengths, the data also enable us to follow workers over multiple claims, so that we observe repeated measures of reservations wages for a given worker. As a sanity check, we verify that claimants stating higher reservation wages draw on UI benefits for a longer time period, holding constant the claimants' and the claims' characteristics, and controlling for unobserved time-invariant heterogeneity of claimants (fixed effects models). This first result confirms that the reservation wage stated by claimants to the UI agency is meaningful.

[2]See Schmieder and von Wachter (2016) for a recent literature review on the impact of unemployment insurance on non-employment duration.

Our main identification strategy relies on a UI reform, which occurred in 2009. The reform was not triggered by the Great Recession. Its main objective was to simplify the rules according to which the potential duration of benefits are computed. In France, PBD is mainly determined by the claimant's previous work duration. Before the 2009 reform, PBD was a step function of past tenure. The 2009 reform simplified the rule and made it linear, so that claimants are now entitled to as many days of benefits as days of work in the previous two years. Some tenure groups benefited from the reform while others lost. Some tenure groups were unaffected and can be used as control groups in a difference-in-difference setting.

Whatever the statistical specification we use, we cannot reject that the elasticity of reservation wages with respect to PBD is zero at the 5% level. Our results are precise and, in our favorite specification, rule out elasticities greater than 0.006. The elasticity of the *actual* duration of benefits with respect to PBD, estimated at 0.3, is in line with most results of the literature. We also find that more generous benefits slow down job finding even at the beginning of the spell when claimants declare their reservation wages.

Looking at other dimensions of job selectivity, we do not find any significant effect of PBD on the maximum commuting time/distance that job seekers are willing to accept. Nor do we find any effects of PBD on the number of hours or on the type of contract job seekers are looking for. The absence of responsiveness in *all* dimensions of job selectivity is a strong result. While the non-responsiveness of reservation wages could have been explained by strong wage rigidity and low mobility across jobs, the fact that the willingness to find open-ended contracts and to ensure job security does not change with PBD suggests that rigid labor markets are unlikely to be the only explanation to our results.

While the elasticity of reservation wages is zero on average, we find that it amounts to a significant 0.01 for job seekers with the lowest past tenure. These job seekers are entitled to short PBD and the date when their benefits could elapse is close to their registration date when they declare their reservation wages. Consistently, we also find that the elasticity of actual benefit duration is higher for these short tenure claimants. We do not find any significant heterogeneity of the PBD elasticity of reservation wages across gender or past wage groups.

We are able to check the robustness of our main results using another identification strategy, a Regression Discontinuity Design (RDD). When unemployed job seekers are over 50 years old at the separation date from their previous employer, they benefit from more generous PBDs, which are on average 30% longer. We find some manipulation of the separation date around the 50-year-old cutoff. Consequently, we adopt a "Donut" RDD strategy, which excludes observations in a window around the cutoff of the running variable. As with our main difference-in-difference strategy, we cannot reject that the PBD elasticity of reservation wages is equal to zero, while the elasticity of actual benefit duration is around 0.2. Claimants in the RDD strategy are different from those of the difference-in-difference, in particular they are more attached to the labor force and older; yet results are very similar.

Lastly, we discuss the theoretical relation between the elasticities of unemployment duration and the reservation wage with respect to PBD. In partial equilibrium, we can decompose the elasticity of unemployment duration into two components: one due to the elasticity of the reservation wage (scaled by the slope of the wage offer distribution taken at the level of the reservation wage) and the other one due to the elasticity of the job offer arrival rate (or search effort). Taking the upper bound of the 95% confidence interval of the estimate of the reservation wage elasticity, we find that the reservation wage margin accounts, at most, for 6% of the elasticity of unemployment duration, the rest being attributed to the elasticity of search effort.

Our paper is, to the best of our knowledge, the first one to obtain precise quasi-experimental estimates of the effect of more generous UI on self-reported reservation wages and other dimensions of job selectivity. Most previous contributions could not rely on credible exogenous variations in UI generosity and found mixed results. Feldstein and Poterba (1984) finds a large elasticity of reservation wages to benefit levels, while Krueger and Mueller (2016) cannot reject that this elasticity is equal to zero. [3]

Our findings on reservation wage responsiveness shed lights on the current debate on the effect of UI on accepted wages. Our results show that changes in PBD have no significant effect on job selectivity at the beginning of the job-search spell for the average job seeker. The absence of selectivity effect is in line with the conclusion of Schmieder *et al.* (2016) that reservation wages are not binding in Germany. When we focus on job seekers with short potential benefit duration, who are more comparable to claimants in the Austrian sample of Nekoei and Weber (2017), we find an estimate of the elasticity of reservation wages around .01, which has a similar magnitude as the estimate found by Nekoei and Weber (2017) on the elasticity of accepted wages with respect to PBD (.016).

Section 2.2 describes the 2009 UI reform and the data. Section 2.3 explains our DiD strategy and provides evidence in favor of the identification assumptions. Section 2.4 presents the main estimates of the effects of PBD on job selectivity and the analysis of their heterogeneity. Section 2.5 deals with our alternative RDD strategy. Section 2.6 discusses the relationship between the elasticities of reservation wages and unemployment duration with respect to PBD. Section 2.7 concludes.

2.2 Research Design and Data

2.2.1 Research Design

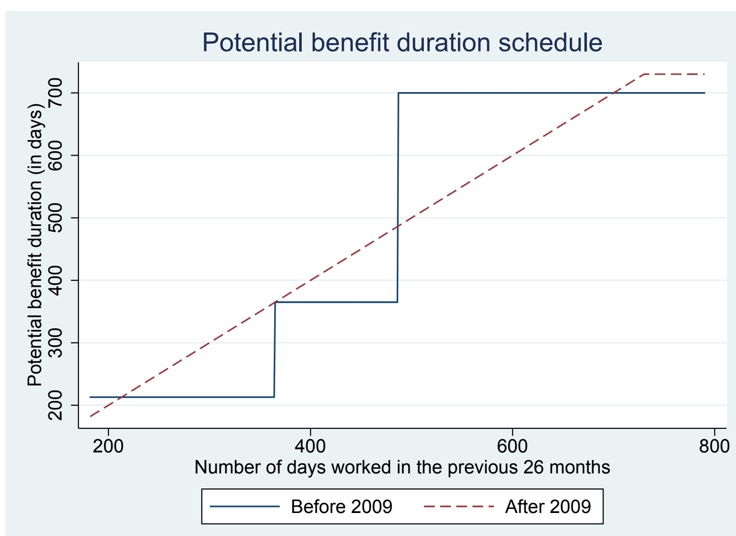
Our identification strategy relies on a change in UI rules that occurred at the beginning of 2009. The 2009 reform was not triggered by the Great Recession. In France, UI rules are renegotiated by unions and employers' organizations every three years. The renegotiation was thus expected but the nature of the change was not anticipated. The objective of the 2009 change was to simplify the computation of Potential Benefit Duration (PBD), i.e. the

[3] One recent exception is Lichter (2016) who uses a quasi-experimental setting to analyze the effect of PBD on job search effort, but also on reservation wages. His results on reservation wages are consistent with ours, although less precise because of the small sample size of his survey data.

number of days of benefits workers are entitled to when they become unemployed. Both before and after the change, PBD depends on the number of days worked in the previous years but in a different way. For workers aged less than 50, before the reform, PBD could take three values: i) 213 days if the unemployed had worked between 182 and 365 days (excl.) in the previous 26 months, ii) 365 days if she had worked between 365 and 487 days (excl.); and iii) 700 days if she had worked 487 days or more. After the reform, the unemployed are entitled to as many days of benefits as they worked in the reference period, with a cap at 730 days. In the current section, we restrict ourselves to workers less than 50 years old. The UI rules are more generous for workers of more than 50 years old, a group on which we focus in our alternative identification strategy in Section 2.5.

Figure 2.1 shows this shift from a step function to a linear function. Some tenure groups benefited from the change, whereas others lost from it, and within winners and losers, some tenures were associated with larger changes than others. The new UI rule applies to anyone whose contract terminated after March 31st 2009.[4]

Figure 2.1: *Schedules of the potential benefit duration, before and after the 2009 reform*



[4]People whose contract terminated before and who are still unemployed on March 31st 2009 stay entitled to the old rule.

2.2.2 Sample Selection

From the administrative records of the Public Employment Service (PES), we select the inflow of job seekers registering for a new UI claim from 2006 to 2012.[5] We focus on claimants of the regular UI rules, excluding senior workers over 50 years old, workers from temporary help agencies –*interimaires*– and workers in the culture and arts industries –*intermittents du spectacle*. We restrict the sample to claimants that were previously employed full-time. This resolves any ambiguity about the reservation wage question (see below). Finally, we exclude observations with extreme past wages or reservation wages.[6]

We further select unemployed people whose pre-unemployment contract terminated between April 1st 2006 and March 31st 2012, 3 years before and after the 2009 reform. Although we show the main results on the full sample as well, the analysis (until Section 2.5) focuses on claimants with multiple claims, more precisely with exactly two claims (there are very few individuals with 3 claims or more so we drop them[7]). Focusing on claimants with multiple spells enables us to control for time-invariant unobserved heterogeneity, for instance, in productivity, which is important to make sense of the link between reservation wages and unemployment duration as we show in section 2.2.3. Controlling for time-invariant heterogeneity is also a plus when implementing our difference-in-difference strategy (see section 2.3). We also believe that job seekers with multiple spells have a better knowledge of the UI system and are thus more likely to react to changes in the PBD schedule. Our final sample has around 180,000 claims, while the full sample of both one-spell and multiple-spells claimants has around 2,000,000 claims. Summary statistics can be found in Appendix Table B.1.

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[5]New claims correspond both to first claims ever and to situations when a worker registers again and has no benefits left from her previous claim.

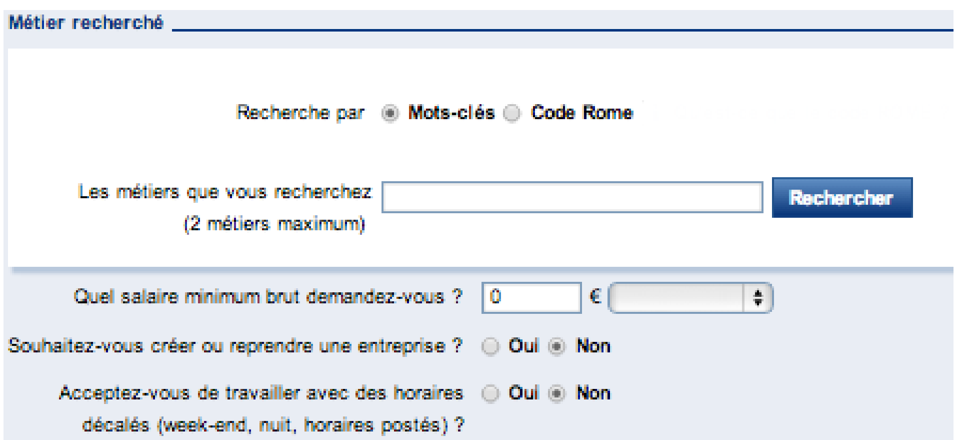
[6]Observations with reservation wages or past wages below the minimum wages are excluded. We also exclude observations whose past wage is over 3,400 euros, or whose reservation wage is over 3,200 euros (95% percentile). We trim observations with a ratio of the reservation wage over the previous wage that is below 0.4 or above 3 (the first and last percentile).

[7]They account for less than 1% of all claims

2.2.3 Reservation Wage Data

When registering as unemployed in France, people are asked the type of job they are looking for and their reservation wage for this job. Figure 2.2 is a screenshot of the online registration form. The question is stated in these terms: "What minimum gross wage do you ask for?". People indicate an amount and choose the unit in which they are reporting their reservation wage: hourly, monthly or annual. Before answering the reservation wage question, they are asked what occupations they are looking for. These occupations may be different from their previous job and they probably provide some kind of anchor for the reservation wage question. Job seekers cannot move on to the next page without having reported their reservation wage.

Figure 2.2: *Screenshot of the section dedicated to reservation wage on the Public Employment Service website at registration*



After the reservations wage, questions about desired hours, type of labor contracts (long-term vs temporary contrats), commuting times/distances are asked in order to help the PES caseworkers choose the type of vacancies they will send to each job seeker.[8] If browsing through vacancies is costly for job seekers, basic theory suggests that the best response of job seekers is to reveal their true reservation wage to the PES.[9] We are also

[8]The fact that these variables are used for this purpose is confirmed here: `http://www.pole-emploi.fr/candidat/le-projet-personnalise-d-acces-a-l-emploi-@/article.jspz?id=60640`.

[9]In case of incomplete information, this conclusion may not hold. Assume for instance that job seekers do

confident that the monitoring/sanctioning role of the PES does not lead job seekers to lie about their reservation wages. Indeed, when controlling the search effort of job seekers, the legal rule requires caseworkers to compare the posted wages of vacancies to which job seekers apply to their *past wage* – and not to their reservation wages.[10] Anecdotal evidence also suggest that sanctions for failing to comply to job-search requirements are never implemented.

We focus on job seekers previously working full-time because the reservation wage question is not explicit about whether it relates to full-time or part-time jobs. Almost all job seekers in our sample look for a full-time job (see Section 2.2.4) and the reservation wage data can be interpreted as being in relation to a full-time job.

Figure 2.3 shows the distribution of the monthly reservation wage.[11] The upper panel displays the reservation wage (in nominal terms). In the intermediate panel, the reservation wage is divided by the minimum wage at the time of registration. The lower panel displays the reservation wage divided by the pre-unemployment wage. Figure 2.3 shows that 35% of workers report the minimum wage as their reservation wage. The minimum wage is high in France[12] and applies to a large share of the workforce: 40% of the unemployment spells of our sample are associated with a previous wage that is inferior or equal to 1.2 times the minimum wage. The upper panel of Figure 2.3 also shows that individuals tend to round their stated reservations wages to hundreds of euros. We discuss the consequences of rounding on our identification in Appendix B.1. We show that the non-classical measurement error in our main outcome variable induced by rounding is unlikely

not know for certain the distribution of wage offers when they answer the question. In this case, they may be tempted to declare a lower reservation wage than their true one, to learn about the distribution. Interestingly though, shorter PBD would then lead job seekers to declare even lower reservation wages, as they need to learn more quickly. This would amplify the impact of PBD on the observed reservation wage, which is at odds with our results.

[10]See `https://www.legifrance.gouv.fr/eli/loi/2008/8/1/ECEX0812043L/jo/texte` for more details.

[11]We convert in monthly terms wages declared in hours or in annual terms, using the legal number of working hours for full-time employees.

[12]For instance, in 2012, the gross monthly minimum wage was 1400 euros.

to bias our main estimates, as the exogenous variation in PBD used in our context is independent of the measurement error.

Dividing the reservation wage by the past wage in the lower panel is a first attempt to control for individual heterogeneity. It captures whether people are being more or less picky, for instance whether they are willing to accept a wage cut. 70% of job seekers would accept a job that pays less than their previous job. The median of the reservation wage ratio is 0.93. The distribution of the reservation wage ratio in our data, is close to the one obtained by Feldstein and Poterba (1984) or Krueger and Mueller (2016).

One potential concern to address is the extent to which, conditional on the previous wage, the reservation wage conveys some additional information. It could be for instance that the reservation wage is anchored on the previous wage with some noise. We show that the reservation wage carries information. First, the reservation wage is meaningfully correlated with workers' characteristics. Second, we show that the reservation wage helps to predict actual benefit duration, even conditional on past wages. In the remainder of the text, we also use the term "unemployment duration" for actual benefit duration.

First, Table 2.1 shows how workers' characteristics are correlated with the reservation wage, controlling non-parametrically for the previous wage.[13] Conditional on the previous wage, women tend to have a lower reservation wage. On the contrary, age, experience and education all lead to a higher reservation wage.

Second, we show that higher reservation wages predict longer unemployment duration. One concern is that the empirical relationship between the reservation wage and unemployment duration, controlling for observables, is likely to reflect both the causal effect of the reservation wage on unemployment duration and the influence on the reservation wages of unobservables that are also correlated with unemployment duration. The workers who are more productive are likely to have better prospects and thus higher reservation wages. They are also more likely to stay unemployed for shorter periods. The unobserved heterogeneity

[13] We split the sample in 20 equal-sized bins in terms of previous wage and use dummies for these bins

Figure 2.3: *Distribution of reservation wages*

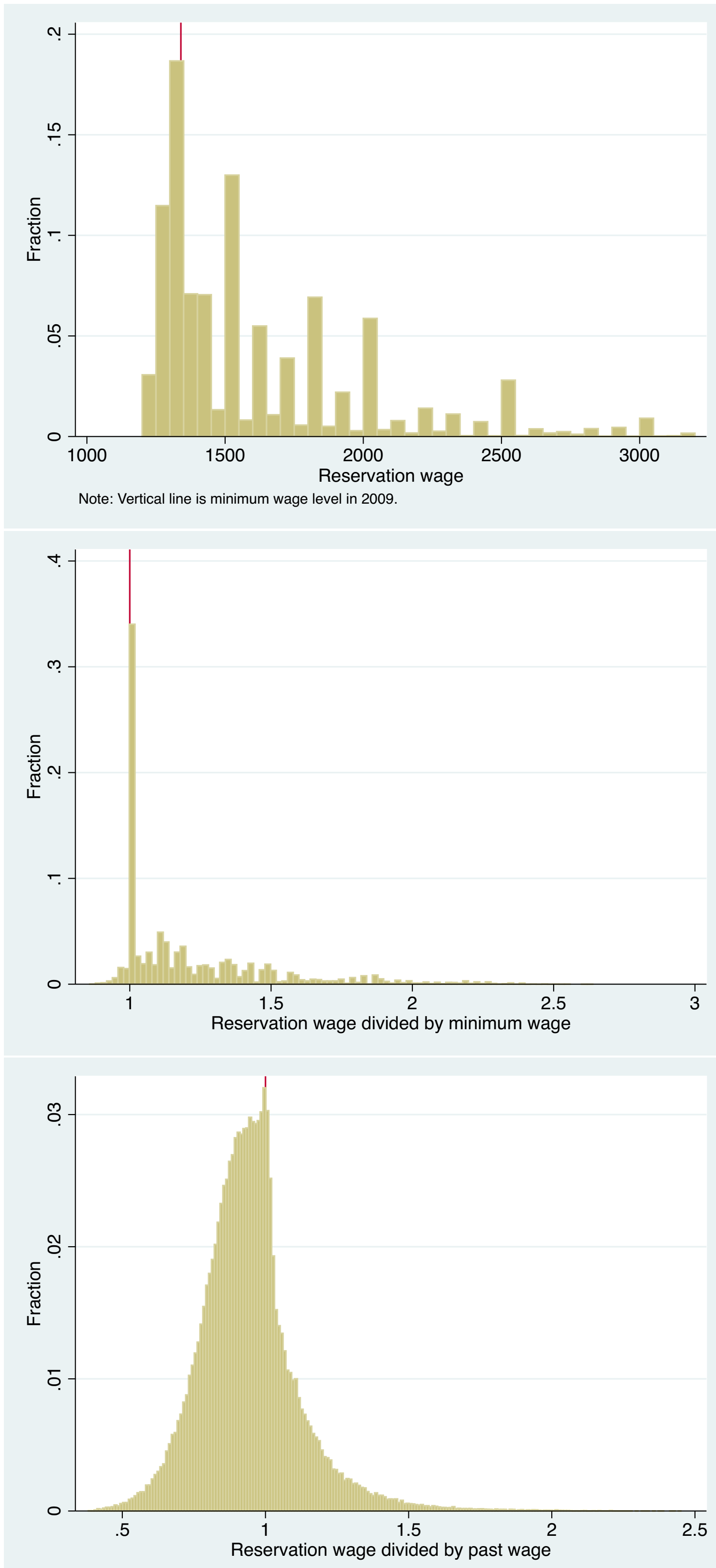


Table 2.1: *Socio-demographic determinants of reservation wages*

	Reservation wage	Reservation wage over past wage
Dummies for 20 equal sized bins of past wage	x	x
Female	-44.52***	-0.0289***
	(1.650)	(0.000904)
Married * Female	-20.86***	-0.0129***
	(2.000)	(0.00107)
Married * Male	39.15***	0.0220***
	(2.043)	(0.00111)
Age	2.510***	0.00148***
	(0.101)	(5.53e-05)
Experience	8.131***	0.00456***
	(0.171)	(9.16e-05)
Education	25.99***	0.0141***
	(0.248)	(0.000138)
Obs.	180,637	180,637
R-squared	0.455	0.237

Source: FNA-FH (Pole emploi).
Note: Estimates from an OLS regression. Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

in productivity is likely to create a negative relationship between reservation wage and unemployment duration, holding observables constant. We test whether this unobserved heterogeneity matters by estimating the following equation with and without individual fixed effects:

$$\log Dur_{i,n} = Indiv.F.E._{i} + \delta \log resW_{i,n} + \beta X_{i,n} + \epsilon_{i,n} \tag{2.1}$$

where $Dur_{i,n}$ is the duration of the n-th claim of individual i; $resW_{i,n}$ is the reservation wage declared by individual i at the beginning of her n-th claim; $X_{i,n}$ are a set of covariates including the log of PBD, time fixed effects (quarterly date of registration), 50 past wage bins and age (see footnote of Table 2.2 for the exhaustive list). First, the OLS coefficient without individual fixed effects (Column (1) in Table 2.2) is negative and equal to -0.16. This suggests that the unobservable productivity component of reservation wages dominates the correlation. Then we estimate the fixed effects regression and find a positive significant coefficient of 0.28 (Column (2) in Table 2.2). Once we control for unobserved (time invariant) heterogeneity using the individual fixed effects, the relationship between unemployment duration and reservation wages only reflects the differences between claims in the work vs. non-work valuation. The comparison between Column (1) and Column (2) of Table 2.2 shows the importance of controlling for unobserved (time-invariant) heterogeneity. This justifies our choice of selecting a sample of job seekers with multiple claims, which enables fixed effects estimation. While this selection is admittedly important – job seekers with multiple spells are probably less productive than those with one spell only –, the OLS coefficient of Column (1) in Table 2.2 does not change much when it is estimated on the whole sample in Column (3) (-0.21 vs. -0.16).

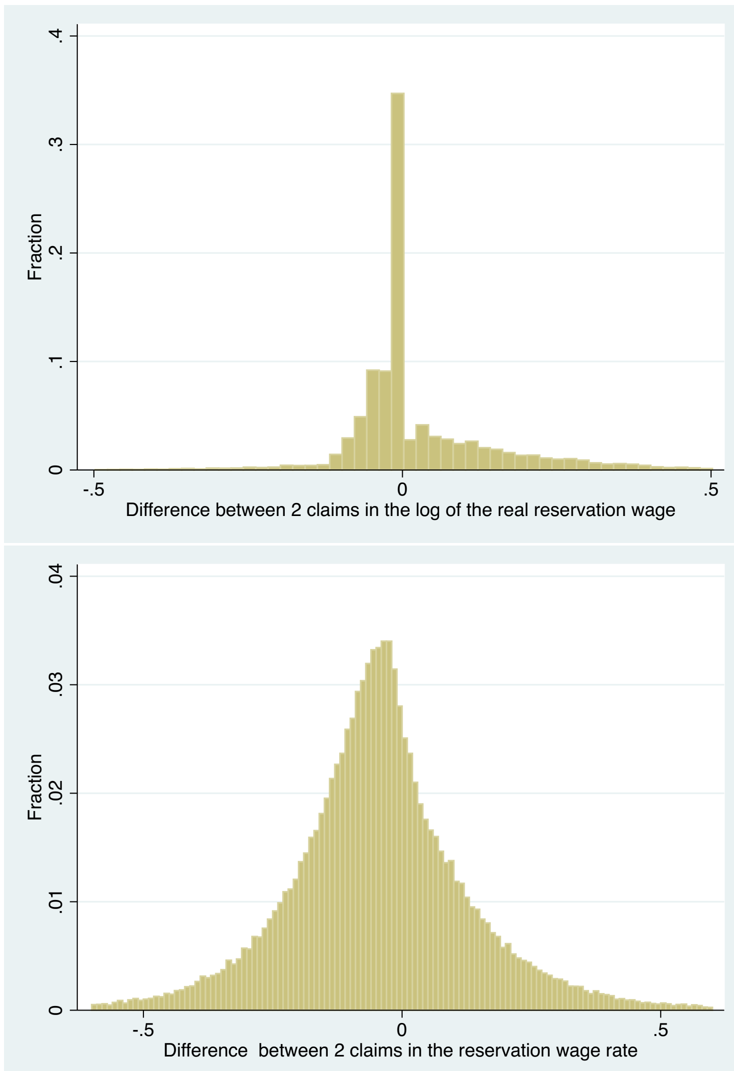
Table 2.2: *Unemployment duration and reservation wage*

	Log unemployment duration		
	OLS	FE-OLS	OLS
	(1)	(2)	(3)
Log reservation wage	-0.155***	0.277***	-0.210***
	(0.0149)	(0.0337)	(.0048)
Sample	multiple-spell	multiple-spell	full sample
Obs.	180,637	180,637	1,957,794
R-squared	0.063	0.091	0.066

Source: FNA-FH (Pole emploi).
Note: Estimates from an OLS regression. Are included in the specification time fixed-effects as well as individual controls: log of PBD, 50 past wage bins, gender, gender interacted with family status dummies (married, divorced, widow, having kids), foreign born, age and age square, experience and experience square, education, reason of separation from previous job, quarterly inflows of new job seekers and of vacancies in the commuting zone (both in logs). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 2.4 shows the distribution of the difference between the reservation wages declared by the same individual at the beginning of her first and second spell over our period of analysis. As the figure shows, although 35% of job seekers declare the same real reservation wage for their two unemployment spells, there is still some variability that we relate to exogenous variations in PBD in Sections 2.3 and 2.4.

Figure 2.4: *Within individual variation in the reservation wage*



Source: FNA-FH (Pole emploi). Note: Upper panel: we divide the reservation wage by the minimum wage prevailing at the time of unemployment registration. Lower panel: the reservation wage ratio is the ratio of the reservation wage over the previous wage.

2.2.4 Data on Other Dimensions of Job Selectivity

During the registration process and after stating their reservation wage, job seekers are also asked about other characteristics of the job they are looking for. They are asked the type of contract and the type of hours they would like. In France, there is a strong duality in the labor market between jobs with open-ended contracts and jobs with temporary contracts. Open-ended contracts ensure that workers benefit from a high level of employment protection: we call them long-term contracts. Table 2.3 shows that almost 90% of job seekers declare that they are looking for such contracts. This is in sharp contrast with the share of job seekers separating from a long-term contract, which amounts to 35%. In contrast to the wage dimension along which the unemployed are willing to accept a cut, job seekers are looking for jobs with more job security than their previous jobs. Table 2.3 also shows that 97% of job seekers are looking for a full-time job.

Table 2.3 also reveals the extent to which job seekers are mobile. On average, job seekers declare that they would accept a job that is in a 32 km radius around their home.[14] For those who give a commuting time, on average they would accept a maximum of 44 minutes of commute. 2% of job seekers declare that they would accept a job anywhere in France. When estimating the effect of UI on mobility, we will abstract from this last category.

As we do not observe the occupation of job seekers in their previous jobs, we cannot study how mobile job seekers are in the occupational dimension.

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[14]Unfortunately, our data do not comprise the commuting distance/time to the previous job. A recent survey conducted on a representative sample of the employed population reveals that the average 2-way commuting time is 50 minutes. `http://dares.travail-emploi.gouv.fr/IMG/pdf/2015-081.pdf`

Table 2.3: *Reservation strategy - all dimensions*

Variable	Mean	Std. Dev.	N
Past Wage (gross monthly, in euros)	1721.6	388.4	180,670
Unemployment Benefits (gross monthly, in euros)	1006.9	226.5	180,670
Reservation Wage (gross monthly, in euros)	1599.9	382.1	180,670
Past Contract is long-term (a)	0.353	0.478	166,486
Looking for a long-term contract	0.895	0.307	180,670
Past job is full-time (b)	1	.	180,670
Looking for a full-time job	0.971	0.167	180,670
Maximum commute time accepted (in minutes) (c)	44.441	19.974	53,880
Maximum commute distance accepted (in kilometers) (c)	31.538	24.4	109,620
No geographical constraint	0.02	0.138	180,670

Source: FNA-FH (Pole emploi).
Note: (a) The data is missing for 7.9% of the ample; (b) We selected our sample among workers with full-time jobs before unemployment. ; (c) The question on commuting time/distance is not mandatory, so that the corresponding variable is missing for 7% of our sample.

2.3 Empirical Strategy: Difference-in-Differences

Our preferred identification strategy relies on exogenous variations in the Potential Benefit Duration (PBD) triggered by the 2009 reform of the Unemployment Insurance (UI) rules. Whereas Figure 2.1 displayed the theoretical change in PBD, Figure 2.5 exhibits the variation in PBD as we observe it in the data. More precisely we plot the α_j coefficients from the following regression

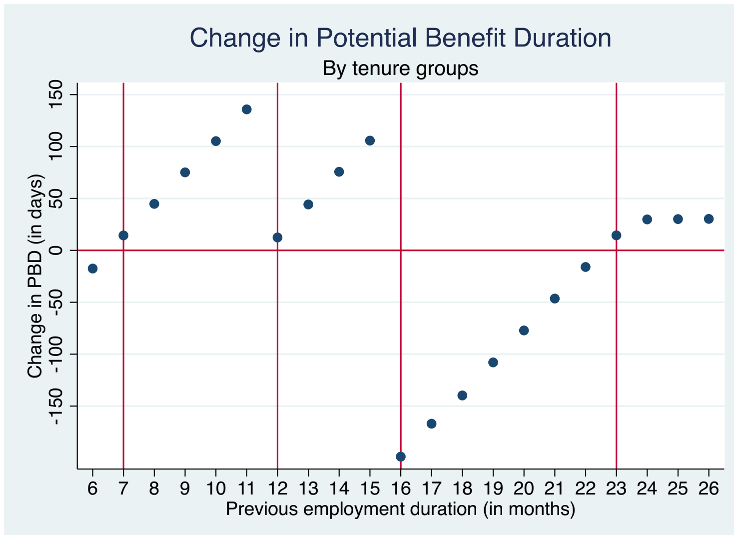
$$PBD_{i,n} = \sum_{j=6}^{26} \alpha_j D(Tenure_{i,n} = j) \times After_{i,n} + \sum_{j=6}^{26} \delta_j D(Tenure_{i,n} = j) + Indiv.F.E. + \nu_{i,n} \quad (2.2)$$

where i, n denotes the n-th unemployment spell of individual i. $Tenure_{i,n}$ is the number of months worked in the 26 months before the beginning of this spell.[15] $After_{i,n}$ is a dummy variable equal to 1 if the last job before the claim terminated after March 31st 2009.

Figure 2.5 highlights that our identification strategy can be thought of as a difference-in-difference analysis. For job seekers with past tenure equal to 7 months, 12 months and 23 months, PBD according to the 2009 rules is just 15 days over what it would have been

[15]The past employment duration which is available in days in the data, is rounded down in number of months.

Figure 2.5: *Treatment: change in the Potential Benefit Duration, in levels (days)*



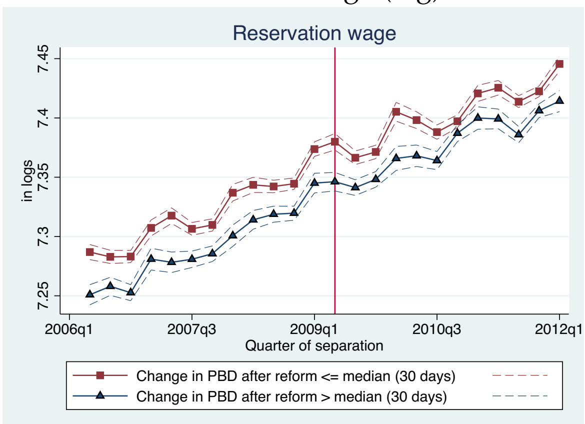
under the 2006 rules. These tenure groups constitute our control groups. All other tenure groups experience larger changes in PBD. Several assumptions are needed to ensure that our design actually identifies the effect of PBD.

The main assumption is the common trends assumption. Figure 2.6 presents a graphical check of the validity of this assumption. Because we have a continuous treatment, for the purpose of this test only, we constitute two equal-sized groups based on the size of the change triggered by the reform. The median change is a 30-day increase in PBD. We split the sample between tenure groups that are above and below the median. For these two groups, we plot in Figure 2.6 the evolution over time of the main outcomes of interest: the reservation wage and the *actual* duration of benefits. The red line with squares shows the evolution of these outcomes for workers whose tenure would make them lose from the reform or gain less than 30 days. The blue line with triangles shows the evolution for workers whose tenure would make them gain more than 30 days of PBD. Figure 2.6 shows roughly parallel trends before the reform, for both outcomes. These graphs also preview the results presented in the next section: the actual duration of benefits seems to be affected by the reform whereas this is not obvious for the reservation wage.

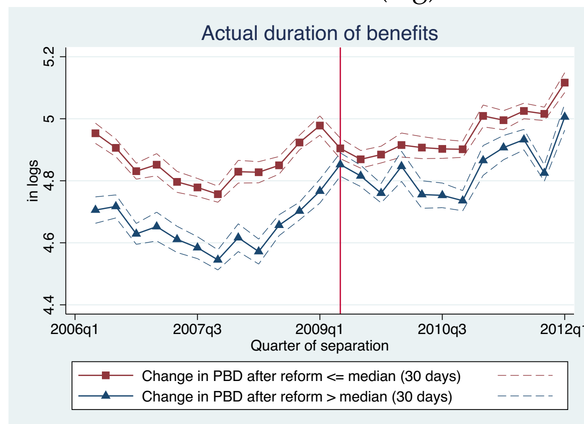
One may be concerned that the difference-in-difference groups are defined according

Figure 2.6: *Common trends*

Reservation wage (log)



Benefit duration (log)



to an endogenous variable: past tenure. We verify in Appendix Figure B.3 that the tenure distribution is similar over time and does not seem to be affected by the 2009 reform.[16] In Section 2.4, we also show results of placebo tests that further confirm our identification strategy.

Another concern is related to the differential selection of claimants after the Great Recession. For example, the 2008-2009 crisis may have triggered early separations of productive individuals, thus affecting differently the average productivity of tenure groups. Our focus on multiple spell individuals allows us to control for time-invariant unobserved heterogeneity, e.g. in productivity, and thus partly alleviates this concern.

Finally, the Stable Unit Treatment Value Assumption (SUTVA) states that each tenure group is not affected by the fact that workers in other tenure groups are treated differently. Lalive *et al.* (2015) show that UI affects equilibrium conditions on the labor market. Compared to other reforms that have been studied, the 2009 reform is less likely to violate the SUTVA as there are both winners and losers, so that the overall generosity of the system does not change much.

[16] Appendix Figure B.4 shows a further test. We compute from the tenure distributions the average PBD according to both the 2006 and 2009 rules. We show that the evolution of the average PBD according to both rules is parallel over time.

2.4 Main Results

In this section, we present our main estimates of the effect of the Potential Benefit Duration (PBD) on the reservation wage, the actual benefit duration, and the other dimensions of job selectivity. Then, we present an analysis of the heterogeneity of the effects according to workers' characteristics.

2.4.1 Reduced Form Effect of the Reform

We first estimate the reduced form effect of the reform, tenure group by tenure group.

$$\log Y_{i,n} = \sum_{j=6,excl.7,12,23}^{26} \beta_j D(Tenure_{i,n} = j) \times After_{i,n} + \sum_{j=6,excl.7,12,23}^{26} \delta_j D(Tenure_{i,n} = j) \\ + X_{i,n}\gamma + Year \times QuarterF.E. + Indiv.F.E._{\cdot i} + \nu_{i,n} \quad (2.3)$$

$Y_{i,n}$ is either the reservation wage or the number of days of benefit receipt for the n-th claim of individual i. $X_{i,n}$ are job seeker's observable characteristics. These include gender, age and experience, age square and experience square, number of years of schooling, marital status and number of children, a dummy for being foreign born, and dummies for 20 bins of the previous wage.[17] We also include dummy variables for the year × quarter at which the previous job ended. We show the results with the individual controls and the year × quarter fixed effects but the patterns are similar without these individual controls (and replacing the time fixed effects by the time dummy for the reform $After_{i,n}$).

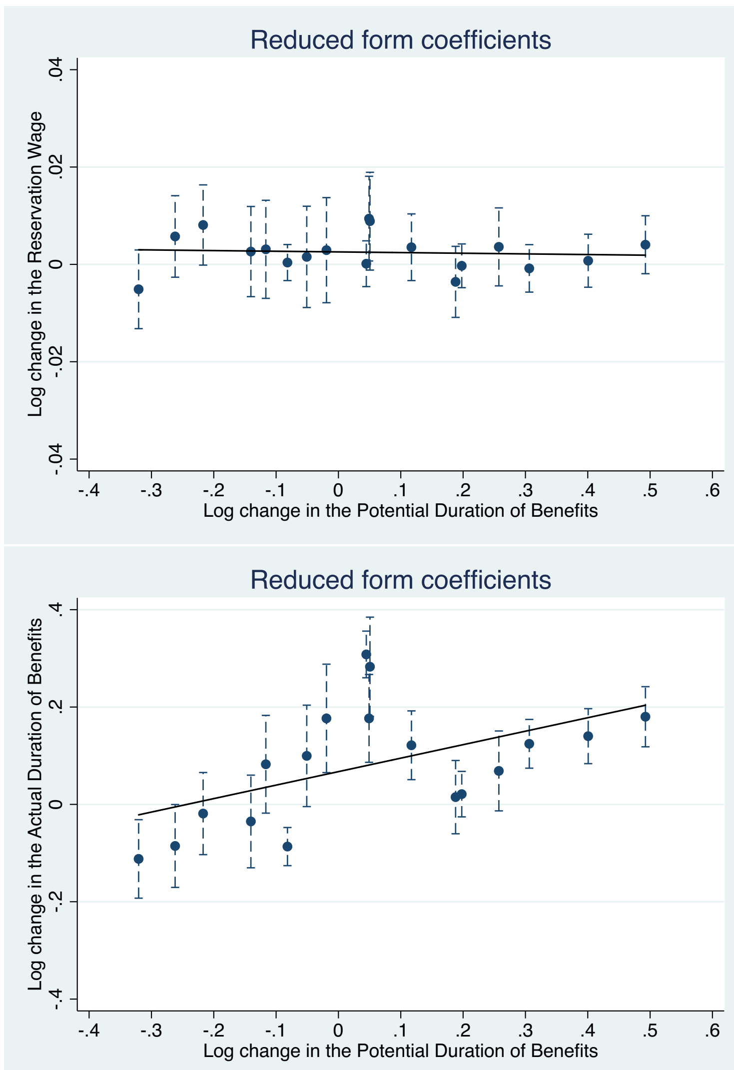
The main coefficients of interest in this regression are the β_js that capture the differential evolution in the outcome of tenure group j after 2009, compared to that of almost unaffected groups, i.e. groups with 7, 12 or 23 months of tenure. [18] Figure 2.7 plots the β_js of each tenure group against its corresponding change in PBD induced by the 2009 reform. In the upper panel, the reduced-form effects on the reservation wages are small in magnitude

[17]Obviously, time-invariant characteristics only matter when we run specifications without individual fixed effects.

[18]Figure B.5 in the Appendix plots these coefficients.

(below 0.01) and they do not seem to be related to the changes in PBD. This contrasts with the positive slope in the lower panel. The reduced-form effects on the actual benefit duration are positively related with the underlying change in PBD, and larger in magnitude. To sum up, Figure 2.7 suggests that UI generosity has no effect on reservation wages, while it affects unemployment duration.

Figure 2.7: *Reduced form effects of the reform on the reservation wage and on the actual duration of benefits*



Source: FNA-FH (Pole emploi). Note: Each panel plots the reduced-form effects of the 2009 reform for various tenure group against its change in PBD. Each dot represent a tenure group. The reduced-form effects correspond to the β_j's estimated in Equation (2.3). The upper panel focuses on the reduced form effects on the reservation wage, the lower panel on the actual benefit duration. The vertical line around each dot corresponds to the 95% confidence interval.

2.4.2 Effect of the Potential Benefit Duration on the Reservation Wage and on the Actual Benefit Duration

Using the notations previously introduced, we estimate the following model:

$$\log Y_{i,n} = Indiv.F.E._i + \alpha \log PBD_{i,n} + \sum_{j=6,excl.7,12,23}^{26} \delta_j D(Tenure_{i,n} = j) + \gamma X_{i,n} + Year \times QuarterF.E. + \epsilon_{i,n} \quad (2.4)$$

Table 2.4 shows the elasticity estimates (α) of the reservation wage in the upper panel, and of the actual benefit duration in the lower panel. In Column (1), we report the elasticity estimated by OLS without individual fixed effects. In Column (2), we use the 2009 reform as an instrument for PBD. More precisely, we instrument $\log PBD_{i,n}$ by the set of tenure group dummies interacted with the dummy indicating that the reform has taken place: $\forall j \in [6, 26], D(Tenure_{i,n} = j) \times After_{i,n}$. In Columns (3) and (4), we include individual fixed effects, respectively in the OLS and IV estimations. In Columns (5) and (6), we report the OLS and IV estimates on the full sample, without restricting it to multiple-spell job seekers, and consequently without fixed effects. Standard errors are robust and clustered by monthly tenure group in Columns (1) and (2).

The estimates of the elasticity of the reservation wage are not statistically different from zero on the sample of multi-spell job seekers, whatever the specification. The estimation is very precise, especially when individual fixed effects are introduced. The standard errors in Columns (3) and (4) enable us to rule out that the elasticity is greater than 0.006, i.e the upper bound of the 95% confidence interval. These findings are qualitatively identical on the full sample.

We perform placebo tests, detailed in Appendix Table B.2. We consider four placebo reforms dates: end of March 2007, 2008, 2010 and 2011. None of the placebo reforms yields significant effects on the reservation wage, which supports the common trend assumption.

The absence of responsiveness of the reservation wage contrasts with that of the the

Table 2.4: *Elasticity of the reservation wage and of benefit duration with respect to the potential benefit duration*

	OLS (1)	IV (2)	FE (3)	FE, IV (4)	OLS (5)	IV (6)
			Log of reservation wage			
log PBD	0.000954	0.00473	-0.000132	-0.000535	-0.00595*	-0.00270
	(0.00854)	(0.00691)	(0.00310)	(0.00318)	(0.00289)	(0.00207)
Obs.	180,637	180,637	180,637	180,637	1,957,794	1,957,794
R-squared	0.474		0.340		0.517	
			Log of actual benefit duration			
log PBD	0.227***	0.232***	0.314***	0.306***	0.218***	0.220***
	(0.0274)	(0.0257)	(0.0317)	(0.0325)	(0.0357)	(0.0349)
Obs.	180,637	180,637	180,637	180,637	1,957,794	1,957,794
R-squared	0.062		0.095		0.062	
Sample	multiple-spell		multiple-spell		full	
Indiv. FE	no	no	yes	yes	no	no

Source: FNA-FH (Pole emploi)
Note: Robust standard errors in parentheses. Standard errors clustered by monthly tenure group in Columns (1) to (4). *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Individual controls, time fixed-effects and tenure fixed-effects are included in all specifications.

actual benefit duration. The estimates of the actual benefit duration elasticity range from 0.22 in Columns (1) and (2) to 0.33 when individual fixed effects are introduced in Columns (3) and (4). All point estimates are statistically significant. The IV strategy hardly affects these estimates. Introducing individual fixed effects increases the elasticity estimates by 50%. This points to positive selection into PBD. More productive individuals, who leave unemployment faster, tend to have longer PBD. Overall, our elasticities are in line with the literature: they are lower than the 0.5-0.6 elasticity reported in Schmieder *et al.* (2012a), but higher than the usual 0.1-0.2 estimates of the elasticity of non-employment duration.

Job seekers with higher PBD find jobs at a slower rate even in the first weeks of unemployment, period during which they declare their reservation wage. Table 2.5 shows the effect of PBD on the job finding probability during the first five weeks of unemployment. We report the estimate of α in equation (2.4), where we instrument PBD by the 2009 reform on the sample of recurrent job seekers – our preferred specification. In the n-th column of Table 2.5, the outcome variable is equal to one if the individual left the unemployment registers and declared having found a job within n weeks after entering unemployment. After the very first week, PBD decreases significantly (at the 5% level) the cumulative job finding rates: from 1 percentage point in week 2 to more than 2 percentage points in week 5. These results suggest that job seekers react to PBD from the very beginning of the unemployment spell and therefore that PBD is salient to them from the start. The absence of responsiveness of reservation wages cannot be attributed to an overall lack of reaction by the job seeker at the beginning of the spell.

Table 2.5: *Effect of the potential benefit duration on cumulative job finding rates at the beginning of the unemployment spell*

	Cumulative job finding rate				
# of weeks since U entry	1	2	3	4	5
log PBD	-0.00778	-0.0130**	-0.0165**	-0.0195**	-0.0234***
	(0.00459)	(0.00594)	(0.007703)	(0.00789)	(0.00862)
Average outcome	0.018	0.030	0.044	0.057	0.070
Indiv. FE	yes	yes	yes	yes	yes
Obs.	180,637	180,637	180,637	180,637	180,637

Source: FNA-FH (Pole emploi)
Note: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The Table reports the coefficient α of Eq. 2.4, where we instrument PBD by the 2009 reform. Individual controls, time fixed-effects and tenure fixed-effects are included. The specification corresponds to that of column (4) in Table 4.

2.4.3 Effect of the Potential Benefit Duration on Other Dimensions of Job Selectivity

Table 2.6 shows the effect of PBD on other job characteristics. We report the estimates of α in Equation (2.4) where the outcome is: a dummy indicating whether the job seeker looks for a long-term contract (Column 1), a dummy indicating whether she looks for a full-time job (Column 2), a composite measure of the willingness of the job seeker to commute (Column 3). We use our preferred specification, with the 2009 reform as an instrument and controlling for individual fixed effects (and covariates).

At registration, job seekers can answer the question about mobility in terms of commuting time or commuting distance. In Column (3), we use as an outcome the log of the commuting time or commuting distance, controlling the declaration unit (kilometers or minutes).

Table 2.6 does not display any statistically significant coefficient relating to PBD. We can rule out small effects of PBD on these extra dimensions of job selectivity. For example, a 10% increase in PBD cannot trigger effects larger than 0.08 percentage points on the probability to look for a long-term contract. These results are robust when we estimate the model by OLS, with or without fixed effects (results available upon request).

Table 2.6: *Effect of the potential benefit duration on other dimensions of job selectivity*

	Looking for a		Max. commuting
	long-term contract	full-time job	time/distance (log)
	(1)	(2)	(3)
log PBD	-0.00462	0.000111	-0.000931
	(0.00825)	(0.00496)	(0.0132)
Indiv. FE	yes	yes	yes
Obs.	180,637	180,637	163,192

Source: FNA-FH (Pole emploi)
Note: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The Table reports the coefficient α of Eq. 2.4, where we instrument PBD by the 2009 reform. Individual controls, time fixed-effects and tenure fixed-effects are included. The specification corresponds to that of column (4) in Table 4. There are missing values in Column (3) because the mobility question is not mandatory.

2.4.4 Heterogeneity Analysis

In this section, we analyze the heterogeneity of the effect and we show that job seekers who have a tenure lower than the median and who thus face a shorter horizon before benefit exhaustion react more to changes in PBD.

When the heterogeneity of the effect relates to time-invariant characteristics, we directly use the fixed-effect specification in stratified samples. For characteristics such as tenure or past wage which can vary across spells within individuals, we compute the average within individuals across spells and split the sample at the median of this average value.

Table 2.7 reports the estimates of the PBD elasticity of the reservation wage (upper panel) and of the actual benefit duration (lower panel) in various sub-samples. The elasticity estimates are obtained according to our preferred estimation strategy, instrumenting PBD with the 2009 reform and controlling for individual fixed effects. Column (1) reports the elasticity estimates for job seekers with an average past tenure below the median, which is 13 months. Their elasticity of the reservation wage is around 0.01, and statistically significant. They react more to changes in PBD presumably because their horizon before benefit exhaustion is shorter : they face on average a PBD of 9 months whereas the average PBD of job seekers with a past tenure above the median is 18 months. Consistently, the elasticity of the actual benefit duration is higher for job seekers with lower tenure (Column 1) than for job seekers with higher tenure (Column 2).

Note that the positive correlation between PBD elasticities of reservation wages and of unemployment duration does not contradict the negative correlation between elasticities of accepted wages and of unemployment duration highlighted in Nekoei and Weber (2017). Indeed, negative duration dependence in unemployment does not drive down the elasticity of reservation wage measured at the beginning of the spell.

When restricting our sample to claimants with low tenure, we analyze a sub-population that experience a PBD shock closer to Nekoei and Weber (2017). They focus on 40-year-old claimants who experience a PBD increase from 30 to 39 weeks. Our *reservation* wage elasticity on the low-tenure sub-population (.01) is then in line with their elasticity of *accepted* wages (.016).[19] That being said, we still expect elasticities on reservation wages to be higher than elasticity on accepted wages, as unemployment duration, which is increased by UI, affects negatively job offer prospects.

We do not find any difference in the elasticity of the reservation wage across gender (Column (3) vs. Column (4) in Table 2.7). In the lower panel, the elasticity of the actual benefit duration is higher for females than for males –a standard result in the literature

[19]Nekoei and Weber (2017) report that reemployment wages increase by .5% (Table 2 of their paper) when PBD increases from 30 to 39 weeks (30%).

on labor supply–, although the difference in elasticity is not statistically significant. Lastly we do not find any difference in the elasticity of the reservation wage and of the actual benefit duration, between low-wage and high-wage job seekers (Column (5) vs. Column (6)). This is surprising as we would expect higher wage workers to be less constrained by the minimum wage and thus to respond more to changes in PBD. However, high-wage job seekers probably have higher job finding rates and higher past tenure, which also makes them less responsive to changes in PBD.

Table 2.7: *Heterogeneity analysis*

	Tenure		Gender		Past wage level	
	Low tenure	High tenure	Female	Male	Low wage	High wage
	(1)	(2)	(3)	(4)	(5)	(6)
	Log of Reservation wage					
log PBD	0.00964**	-0.00272	0.00156	-0.00245	0.00323	-0.00285
	(0.00379)	(0.00557)	(0.00454)	(0.00435)	(0.00340)	(0.00543)
	Log of Actual Benefit duration					
log PBD	0.514***	0.202***	0.332***	0.292***	0.321***	0.291***
	(0.0399)	(0.0558)	(0.0508)	(0.0423)	(0.0448)	(0.0473)
Obs.	90,364	90,273	72,472	108,165	90,203	90,434
Indiv. F.E.	yes	yes	yes	yes	yes	yes

Source: FNA-FH (Pole emploi)
Note: Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Individual controls, time fixed-effects and tenure fixed-effects are included in the specification. The Table reports the coefficient α of Eq. (2.4), where we instrument the potential benefit duration by the 2009 Reform.

2.5 Alternative Empirical Strategy: Regression Discontinuity Design

In this section, we consider an alternative identification strategy based on an age discontinuity in Potential Benefit Duration (PBD) at age 50.

When workers are above 50 years old at the time of job separation, they benefit from a more generous PBD schedule. Before 2009, when senior claimants had worked more than 27 months during the last 36 months, they were entitled to 1,095 days of benefits, i.e. 36 months. If they did not fulfill this criteria, they faced the step function schedule described in Section 2.2. Starting in 2009, senior workers are entitled to as many days of benefits as days worked within the last 36 months before job separation: the maximum PBD is thus 36 months. Consequently, senior workers with *continuous work history* over the last 3 years before unemployment are entitled to benefits for a period that is 50% longer than that of younger workers. Besides this difference in PBD schedule, UI rules are essentially the same for claimants above and below 50 years old. In particular, the formula used to compute the amount of benefits as a function of past wages is identical for senior and younger workers.

We select a sample of new claims filed between 2006 and 2012. We apply the same sample restrictions as above, but do not restrict the sample to job seekers with multiple claims over the period. We only keep claims filed by job seekers aged between 45 and 55, which corresponds to a 10-year window around the RDD cutoff. In our sample, we estimate that PBD is on average around 30% higher for claimants above 50 than below 50.

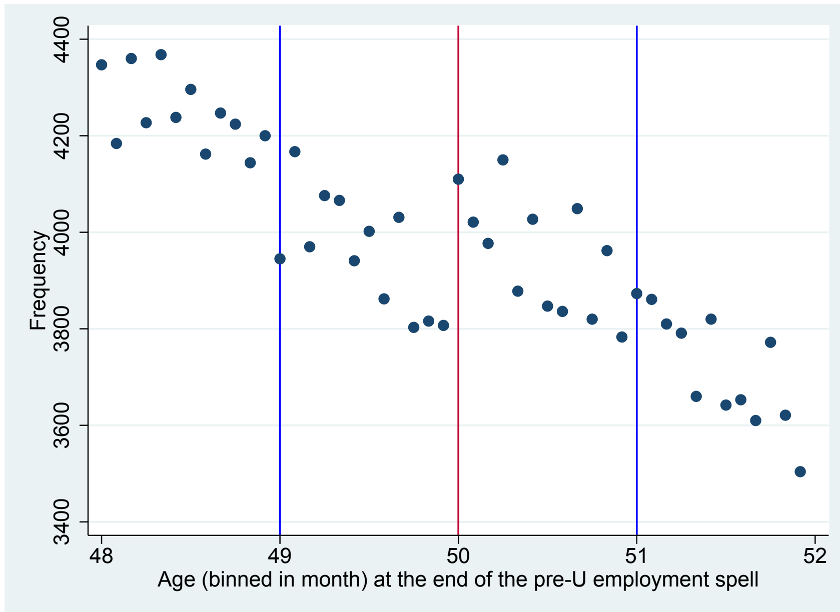
Figure 2.8 plots the distribution of the population in age bins that are one-month wide. This reveals some missing mass just below 50 and extra mass just above the cutoff. We perform the usual McCrary test and we estimate that the density is 8% higher just above the cutoff – statistically significant at the 1% level.[20]

We also find some discontinuities in the distribution of covariates around the threshold. Appendix Table B.3 shows that workers laid off just after 50 years old are more educated and are paid higher wages. This is consistent with some manipulation of the running variable, i.e. age at the separation date. Manipulation is a serious threat to the RDD identification strategy. This is the reason why we only consider the RDD as an alternative strategy and the next results as suggestive. Yet we find it reassuring that they are consistent with the main results obtained with the difference-in-difference strategy.

Lalive (2007) and Schmieder *et al.* (2012a) also report cases of manipulation in the context of UI rules based on age. To address this issue, we follow the same strategy as Schmieder

[20]The bandwidth used to perform the test is around 2 years.

Figure 2.8: *Distribution of the running variable (age in month)*



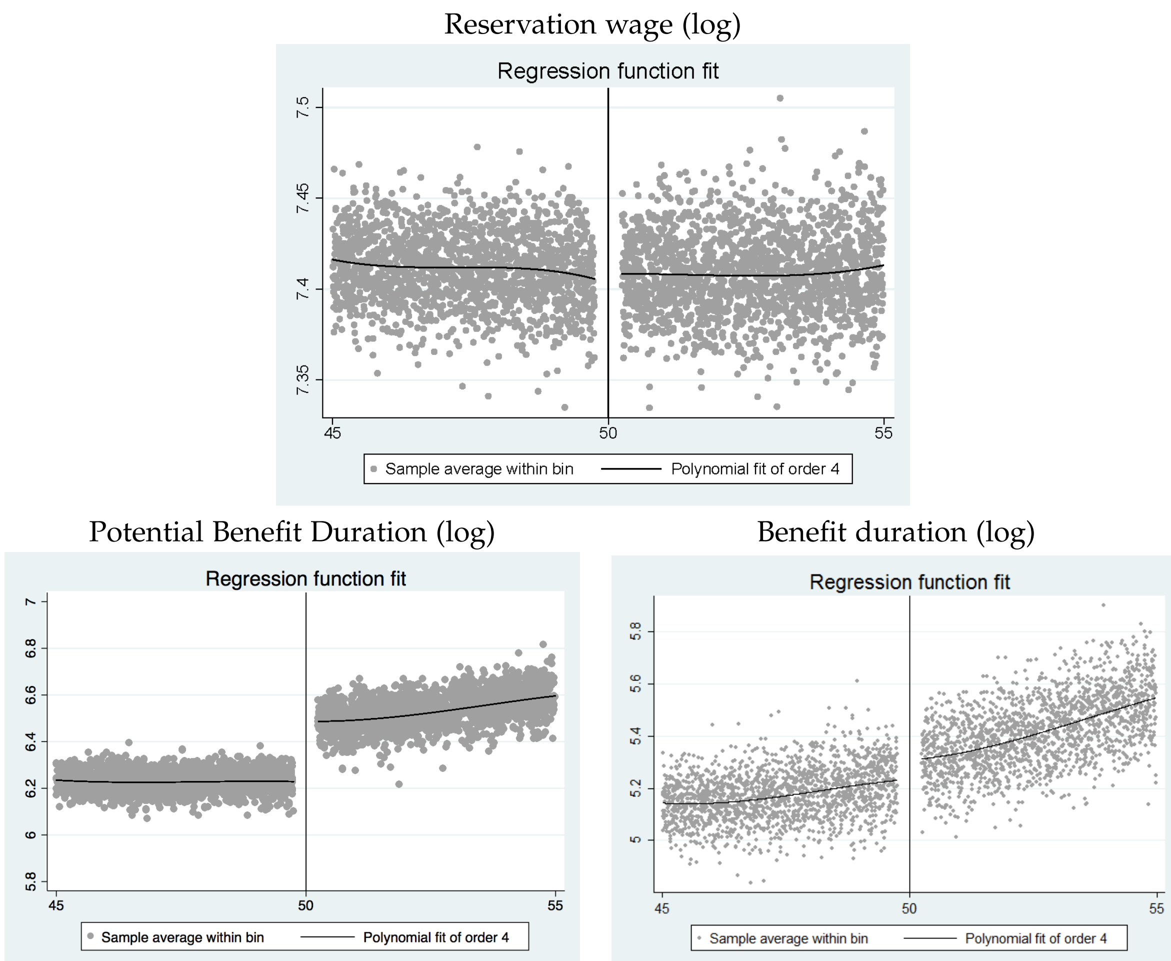
et al. (2012a) and we exclude the observations that are just around the age cutoff.[21] This is a valid correction as long as manipulation is a local matter. There are no theoretical guidelines to choose the size of the window around the cutoff where we exclude observations. Visual inspection of the density of the running variable plotted in Figure 2.8 suggests that the manipulation window would be between 3 months before and 3 months after the cutoff. We vary the size of the window to test the robustness of our estimates.

Figure 2.9 plots PBD and the outcomes of interest against age at job separation excluding individuals between 49.75 and 50.25 years old. The upper panel shows that the distribution of reservation wages seems continuous on either side of the cutoff whereas the lower panel shows that the size of the discontinuity in PBD is around 30% as already mentioned above and that there is a discontinuity in the actual duration of benefits at the cutoff.

For the estimation, we consider the fuzzy RD design where the treatment is the log of PBD. This framework enables us to interpret our estimates as elasticities with respect to PBD. We estimate the following model using the dummy $1(age \geq 50)$ indicating that the

[21] Lalive (2007) finds that only women manipulate their age to benefit from more generous entitlement.

Figure 2.9: *Regression discontinuity figures, excluding observations with age between 49.75 and 50.25.*



Source: FNA-FH (Pole emploi).

Note: plots are obtained with the stata command rdplot. The data is grouped in age bins, whose size is determined according to Calonico *et al.* (2014). For each age bin, we plot the averages of the reservation wage, of PBD and of the actual benefit duration. Then a polynomial of degree 4 is fitted on these averages - the solid line.

claimant is over 50 years old (at job separation) as an instrument.

$$\log Y_i = \alpha + \delta \log PBD + P_0(age_i - 50) \times 1(age_i < 50) + P_1(age_i - 50) \times 1(age_i \geq 50) + \epsilon_i \tag{2.5}$$

where $P_0(.)$ and $P_1(.)$ are polynomials whose coefficients are estimated (without constant). We follow the common practice and we use local polynomial regression. The bandwidth of the local estimation is selected according to Calonico *et al.* (2014). We also follow the methods they recommend for bias correction and robust standard error correction.

Table 2.8 reports the estimates of the elasticities δ on samples trimmed in different ways (see columns). The upper panel reports the estimates of the elasticity of the reservation wage, while the lower panel reports those of the elasticity of the actual benefit duration. The elasticity of the reservation wage is not statistically significant, whatever the size of the excluded region around the cutoff. This contrasts with the elasticity of benefit duration, which is around 0.2 and statistically different from zero. This confirms the main results of Section 2.4: inelastic reservation wage and responsive benefit duration. We also find that the effects of PBD on other dimensions of the reservation strategy, such as the desired type of labor contract or number of hours or the maximum commute accepted, are not statistically different from zero (results available on request).

Appendix Table B.3 tests for discontinuities in covariates, when observations around the threshold are excluded. Any discontinuities disappear as the bandwidth of the trimmed sample around the cutoff increases. In Appendix Table B.4, we further show that including covariates as controls in the RDD estimation hardly affects the estimated elasticities reported in Table 2.8. Finally, we conduct placebo tests at every age from 44 to 54. Appendix Table B.5 shows no significant discontinuities at every placebo age.

Table 2.8: *Regression discontinuity estimates of the elasticities of the reservation wage and of benefit duration*

Age excluded	(1) $[49.9, 50.1]$	(2) $[49.75, 50.25]$	(3) $[49.5, 50.5]$
	Log of Reservation wage		
log PBD	0.0116	0.0172	0.00457
	(0.0149)	(0.0162)	(0.0141)
	Log of Actual benefit duration		
log PBD	0.211***	0.242***	0.175**
	(0.0786)	(0.0669)	(0.0692)
Obs.	470,082	456,280	432,431

Source: FNA-FH (Pole emploi).
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: The reported coefficients are the estimated δ from equation (2.5). The estimation follows Calonico *et al.* (2014). The kernel used for local polynomial estimation is triangular.

2.6 Interpretation

In a simple job search model, the job finding rate (or hazard rate) h is the product of the probability to receive an offer e and the probability to accept this offer given one's reservation wage ψ.

$$h = e\bar{F}(\psi) \tag{2.6}$$

where $F(.)$ is the cumulative distribution function of wage offers.

When the potential benefit duration T changes, the probability to receive an offer may change, as job seekers adjust their search effort. The probability to accept the offer may change as well, as job seekers update their reservation wages. Given equation (2.6), there exists a mechanical relationship between the elasticities of h, e and ψ with respect to the potential benefit duration.

$$\epsilon_{h,T} = \epsilon_{e,T} - \frac{f(\psi)\psi}{\bar{F}(\psi)}\epsilon_{\psi,T} \tag{2.7}$$

where $\epsilon_{y,T}$ is the elasticity of variable y with respect to the potential benefit duration T, and $f(.)$ is the density of wage offers.

Using equation (2.7), we would like to assess the extent to which the elasticity of the reservation wage explains the elasticity of the hazard rate. For this empirical exercise, we will use the elasticities $\hat{\epsilon}_{h,T}$ and $\hat{\epsilon}_{\psi,T}$ we have estimated in the previous sections, as well as the distribution of wages obtained by job seekers newly employed.[22] Taking the average value of the reservation wage, we find that the empirical equivalent of $\psi f(\psi)/\bar{F}(\psi)$ is close to 3.[23] For the empirical value of the elasticity of the hazard rate to potential benefit duration, we can either consider the elasticity of unemployment duration (Table 4, column 4, second panel), or the elasticity of the job finding rate in the first weeks of the spell (Table 5). Both give a value of $\hat{\epsilon}_{h,T}$ around -.3.[24] Taking the upper bound value of the 95% confidence of the elasticity of $\hat{\epsilon}_{\psi,T}$, equal to .006, we find that the response of the reservation wage (-.018) represents at most 6% of the elasticity of the hazard rate ($\hat{\epsilon}_{h,T}$ =-.3), so that the search margin would represent at least 94%. Considering the median reservation wage instead of the mean leads to an even smaller role for the reservation wage: at most 3.5%.

2.7 Conclusion

Despite a significant and non negligible effect of the Potential Benefit Duration (PBD) on actual benefit duration, there is almost zero effect of PBD on the reservation wage and other dimensions of job selectivity at the start of the unemployment spell. The elasticity of reservation wages is insignificant and we can rule out that it is larger (in absolute value) than 0.006, which means that being entitled to 1 additional month of benefits for a claimant with an initial PBD of 10 months is associated with at most a 0.06% increase in the reservation wage. The lack of responsiveness of reservation wages to changes in UI echoes the results of Krueger and Mueller (2016) who find that reservation wages adjust at a very slow rate

[22]Because we work on claimants with multiple spells, we can observe in the administrative data the past wage relating to the second unemployment spell, which we consider a good proxy for the hiring wage after the first unemployment spell.

[23]We take $\psi = 1.2$, i.e. 20% higher than the legal minimum wage, and we find in the empirical distribution of wages that $f(1.2)$ = 1.92 and $\bar{F}(1.2) = .76$.

[24]Considering the elasticity of unemployment duration gives $\hat{\epsilon}_{h,T}$ = .306. Considering the elasticity of the job finding rate, for example in the three first weeks, gives $\hat{\epsilon}_{h,T}$ = -.0165/.044 = -.375.

along the unemployment spells.

The question of why reservation wages react so little to changes in UI remains open. We see at least two explanations that are worth further explorations. First, job seekers may have reference-dependent behaviors (Koenig *et al.*, 2014; DellaVigna *et al.*, 2016). They may anchor their reservation wages on their past wages, which would limit the responsiveness of reservation wages. Second, job seekers may have biased beliefs about their employment prospects. Over-optimistic job seekers, who consider that job offers arrive at a high arrival rate, might also react too little to changes in PBD, as they underestimate their likelihood to exhaust benefits (Spinnewijn, 2015). Exploring these possibilities are promising directions for future work.

A last natural question concerns the extent to which empirical measures of reservation wages inform us about the welfare cost of unemployment (Shimer and Werning, 2007). Answering this question is essential in order to assess how useful our elasticity of reservation wage is to the design of optimal UI.

Chapter 3

The Causal Effect of Job Loss on Health: The Danish Miracle?

3.1 Introduction

Job loss can affect health both through the income shock and through non-pecuniary channels like the loss of self-esteem or the loss of a structured schedule. The seminal work in sociology on the unemployed community of Marienthal, a small town in Austria where the main factory closed in 1930 leaving many people unemployed for a long time, shows how desperate unemployed and their family can become and the many dimensions in life that can be affected, from standards of living to the loss of a sense of purpose or of a social identity (Lazarsfeld *et al.*, 1933) [1]. Fortunately, since the Great Depression, developed countries have implemented some policies to alleviate the burden of unemployment, in particular unemployment insurance (UI). But job loss might also entail some non-pecuniary aspects against which policy cannot provide insurance.

This paper tries to investigate whether there remains a causal effect of job loss on health, and in particular mental health and substance abuse, in a setting where UI is

[1] Interestingly for our purpose, for the vast majority, the unemployed studied in that work lost their job as part of a plant closure. Thus we can view the distress reported as at least not entirely driven by selection biases.

generous, active labor market policies are available and health insurance is universal. The identification strategy relies on establishment closures, which lead to job losses that are arguably exogenous to employees' health. The context is that of Denmark after the implementation of flexicurity policies. The replacement rate of unemployment insurance is 90% [2] and the maximum potential duration of unemployment benefits, though it has been gradually reduced, remains long: 4 years during the relevant period for this study[3]. Active labor market policies are in place throughout the period [4]. Moreover health insurance is publicly provided with universal coverage.

Using a difference-in-difference design and Danish administrative data, I compare the health of roughly 25,000 workers who experience an establishment closure to that of a control group matched on observables. I find that on average in Denmark job losses due to establishment closures that occurred between 2001 and 2006 did not cause large significant health problems.

I focus on people strongly attached to their job: my sample consists of men and women of age between 25 and 60 who have at least 5 years of tenure at their establishment. 25% of my treatment group goes through a period of unemployment in the year of the closure, as opposed to 4% in the control group. They are also more likely to leave the labor force. However, despite a long lasting effect on their wage earnings, the drop they experience in post-tax post-transfer household income is only of 6%. In terms of health, the treatment group is not significantly more likely to purchase antidepressants or other anti-anxiety drugs, which I use as proxy for mental health. I can rule out effects of the order of 2%. I do not observe either any change in their regular health care consumption such as the number of visits to the General Practitioner nor any effect on severe physical health outcomes that

[2]with a cap, which corresponds to the third decile of the wage distribution (P32) ; in 2015 the cap was 4135 DKK per week which would be roughly equivalent to 628 USD (or 32,000 USD in annual terms)

[3]In 1994, it was set to 7 years, split between a passive period and an active one, where people had to participate in active labor market programs; from 2001 to 2010, which is the relevant period for this paper, it was 4 years; and now it is 2 years.

[4]Active labor market policies started to be implemented by the Social democrats in 1993-1994. For more details see section 3.2.

require inpatient care at the hospital, for which I can rule out respectively effects of the order of 1 and 4%. Mortality is also not significantly affected. [5] The two exceptions for which I find a marginally significant effect are visits to the hospital for alcohol issues [6] as well as purchases of diabetes related drugs, but these results are not very precisely estimated, the baseline mean is very low and the effect might well be false positive due to mutiple hypothesis testing.

This paper is related to several lines of research. The most closely related papers, which I discuss further after presenting my results, are those looking at the effect of mass layoffs or plant closures on some health outcomes. Table 3.1 provides a synthetic summary of the context, methods and results of these papers. Results from this literature are mixed: while some papers find strong effects, in particular on mortality for males (Sullivan and Von Wachter (2009), Eliason and Storrie (2009a), Browning and Heinesen (2012), Rege *et al.* (2009)), others find a relatively precise zero (Browning *et al.* (2006), Kuhn *et al.* (2009)). Part of the variety of the results comes from differences in the precise definition of the treatment and of the control groups as well as sample restrictions and outcomes of interest (for instance many papers focus on mortality for males and I do find a positive point estimate for mortality for males, though not significant) or on some methodological differences (whether or not one includes the deaths that occur in the year of displacement can make a difference). But another part presumably has to do with the fact that the effect of job loss on health depends a lot on the institutional context and it is hard to compare results across countries.

[5]As I show, there is a strong significant difference in the death hazard of the treatment v. the control group in the year of displacement but some of these deaths could be the cause of the establishment closure rather than caused by the closure. This reverse causality concern seems all the more relevant that the difference is entirely driven by the smallest establishments. Thus, though I show results both with and without the deaths of year 0, my preferred estimates are, as in Sullivan and Von Wachter (2009), the ones that focus on deaths that occurred from year 1 onwards.

[6]This includes both visits to the emergency room for alcohol abuse and inpatient or outpatient care for alcohol dependence

Table 3.1: *Literature review*

Paper	*Context*	*Treatment*	*Control group*	*Health outcome(s)*	*Results*
Browning & al. 2006	Denmark 1986-1996	Mass-layoff and plant closure	All other establishments	Diseases of circulatory or digestive system	No effect
Browning & al. 2012	Denmark 1986-2002	Plant closure	Positive growth or less than 10% downsizing	Mortality	Significant effect for males
Eliason & Storrie 2007	Sweden 1987-1988	Plant closure	All other establishments	Mortality	Short run effect for males
Eliason & Storrie 2009	Sweden 1987-1988	Plant closure	All other establishments	Cardiovascular diagnoses Alcohol-related diagnoses	No effect Positive effect
Kuhn & al. 2009	Austria 1999-2001	Plant closure	Positive growth or less than 30% downsizing	Hospitalizations Doctor visits Mental health drugs	No effect No effect Small effect for males
Martikainen & al. 2007	Finland 1989 and 1994	Plant closure or 50% downsizing	All other establishments	Mortality	No causal effect
Rege & al. 2009	Norway 1995-2000	Downsizing by at least 60%	Positive growth establishments	Mortality	9%increase significant at 10%
Sullivan & von Wachter 2009	United States 1980-1986	Downsizing by at least 30%		Mortality	50-100% effect in year 1 10-15% afterwards

Compared to this literature, I contribute by looking at a very wide set of health outcomes with a long period of observation, which allows me to give a comprehensive picture. Moreover I am able to provide direct visual evidence that the treatment and control group were on parallel trends in terms of health in the five years before the job loss shock. I interpret my results as showing that it is possible, presumably through an adequate set of policies, to make the causal effect of job loss on health very small, if not negligible.

The paper also relates to work on unemployment and subjective wellbeing (Winkelmann and Winkelmann, 1998). This literature has shown that unemployment is associated with lower subjective well-being. My paper adds to this literature by focusing on job losses that are arguably exogenous and by aiming at capturing more objective but also more severe health conditions. It is possible that unemployment leads to lower satisfaction but if it is not to the point that people start taking anti-depressants or to be in worse health, it may not be of first order importance for policy to deal with it.

This paper also relates to work on income-health gradients, and in particular recent work by Cesarini *et al.* (2016) on the causal effect of wealth on health and child development in Sweden. They find that an exogenous increase in wealth (due to winning a lottery) has overall no effect on health, neither on mortality [7] nor on health care utilization. This provides evidence totally in line with what I find that, at least nowadays in Scandinavian countries, the cross-sectional association between health and economic variables is mostly driven by selection.

Finally the paper can relate to rising concerns in the US about addiction to painkillers and the increase in mortality from poisoning, suicide and alcohol related deaths highlighted by Case and Deaton (2015). Data from the International Narcotics Control Board show that Denmark also experienced rising trends in painkillers consumption. [8] Our data allows us to test whether job loss makes people more likely to develop addiction to such substances, to

[7] In particular they are able to rule out effects on mortality one sixth as large as the cross-sectional gradient.

[8] In 2013, Denmark was fifth out of 76 countries in terms of oxycodone consumption but with amounts in mg/capita four times smaller than that of the US who were first; however Denmark was first in terms of hydromorphone consumption.

engage in excessive alcohol drinking or to commit suicide. Despite a strong association in the cross-section between unemployment and purchases of opiod painkillers, I do not find any effect on such purchases following an exogenous layoff. However I do find a marginally significant effect of establishment closures on alcohol issues.

The rest of the paper is organized as follows. Section 3.2 provides some background information about the Danish context and the data used, as well as the cross-sectional association between unemployment and health in Denmark, controlling for observables. Section 3.3 describes the empirical strategy to identify the causal effect of job loss on health, while Section 3.4 presents the results. Section 3.5 discusses the relationship with the literature while Section 3.6 concludes.

3.2 The Danish Data and Context

Denmark is an ideal setting to study the question at hand for several reasons. Because of its generous unemployment insurance, its active labor market policies and its universal public health insurance, it allows us to test whether there remains a causal effect of job loss on health when the unemployment risk is well insured by policies. The results have to be understood as a lower bound of the causal effect of job loss on health in general: the question of interest is to what extent this lower bound can be made zero or negligible. Moreover because of its single-payer health care system with universal coverage, administrative data on health provide a comprehensive and reliable picture of health care consumption, which would be very different in countries like the United States.

3.2.1 The Data

I use Danish administrative data from several registers covering the period 1996-2013 for the universe of Danes. The data on economic outcomes come from tax data and from matched employer-employee registers. Tax data give in particular annual wage earnings, annual amount of unemployment benefits, post-tax post-transfer income [9]. Employer-employee registers allow to compute, at the establishment level, closures and employment changes. I also use data from unemployment insurance funds from 1996 to 2007, which give UI claims at the weekly level.

The health data come from reimbursements of medical care or related purchases: they include doctor visits; hospital visits/admissions with precise diagnosis; and prescription drugs purchases. In addition I also use death records with the precise cause of death. Table C.1 provides a summary of the main registers and variables used. One of the advantage of the data is the many health outcomes it allows to look at. To proxy for poor mental health or addiction, I look at purchases of antidepressants and related drugs as well as purchases of painkillers and at specific causes of deaths like suicide and deaths associated with excessive alcohol consumption (e.g. chronic liver disease). To proxy for severe health conditions, I look at diagnosis of disease of the circulatory system and cancer diagnosis, as well as any inpatient visit to the hospital (treating separately pregnancy related visits). I also look at regular health care consumption such as visits to various types of doctors. Of course health care consumption may fail to capture accurately the health status if people do not seek treatment but i) because health care is basically free there is no liquidity constraints for people to seek treatment and ii) by looking at mortality, I am sure to capture severe health issues that could have gone undiagnosed or untreated.

3.2.2 The Context

Over the period studied, the unemployment rate was on a declining trend from more than 10% in the early 1990s to 2% right before the crisis. Unemployment had started to rise in the 1970s and the first response to this increase in unemployment was to push old workers out of the labor force through early retirement schemes, while maintaining the standard of living of the unemployed through generous and long-lasting social transfers. In 1993, the

[9] All amounts in annual DKK are converted in 2015 DKK using the CPI table from Statistics Denmark http://www.dst.dk/en/Statistik/emner/forbrugerpriser/forbrugerprisindeks

Social Democrats came to power and started to implement what we now call a "flexicurity" system. [10] They reduced the potential duration of unemployment benefits, which used to be unlimited, to a maximum of seven years: four years of passive support followed by three years of activation. In parallel, contrary to other countries, spendings for active labor market policies were increased, with a significant share devoted to training.

Contrary to many countries, unemployment insurance is voluntary in Denmark. There are 36-state approved unemployment funds to which people must contribute if they want to benefit from unemployment insurance. Most people do insure themselves. 77% of the labor force is a member of such a UI fund and 95% if we consider people with at least 5 years of tenure at their establishment, which is the group I focus on in this paper. However, even if you are not insured, you are entitled to some welfare benefits from the municipalities if you become unemployed. The income measure I use is post-tax post-transfer income such that I abstract from where the redistribution is coming from[11].

3.2.3 Health and Unemployment in the Cross-Section

Before focusing on the causal estimates of layoff on health, I document the association in the cross-section between unemployment and a few health outcomes. I look at all new UI recipients in a given year 0, who are full-time unemployed and who are between 25 to 60 years old. By new recipients, I mean UI claimants who did not receive UI in the prior year -1. I perform event studies around the year of UI receipt, comparing them to a control group of people who did not receive UI in year 0 but who were fully insured (i.e. who would have received full-time UI had they been fired), and who were also non UI recipient in year

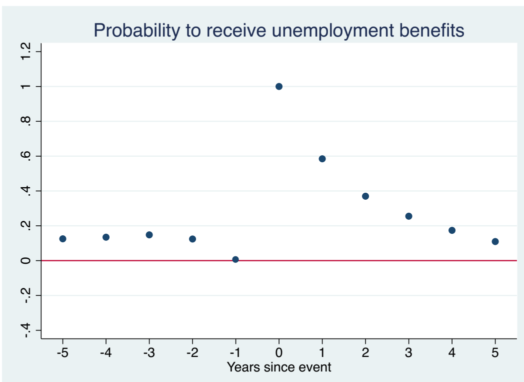
[10] A quick note on the history of the flexicurity system: In 1982 a center-right government came to power under the leadership of Poul Schluter and implemented some liberal reforms (massive liberalisation of capital markets, decentralisation and revamping of the public sector etc) but did not have enough political support to implement a big labor market reform in a time of high unemployment. However cuts in funding for local governments led municipal officials to experiment with active social policies as a way to simultaneously cope with fiscal constraints and enhance the productivity of marginal workers. Schluter ordered a report in 1991 to a tripartite commission, the Zeuthen Commission, which advocated deep structural changes to the Danish unemployment insurance system. In 1993, the Social Democrats returned to power and started to implement the recommendations of the Zeuthen Commission in which they had been involved.

[11] In addition, although UI funds are privately organized, 90% of their resources come from the state

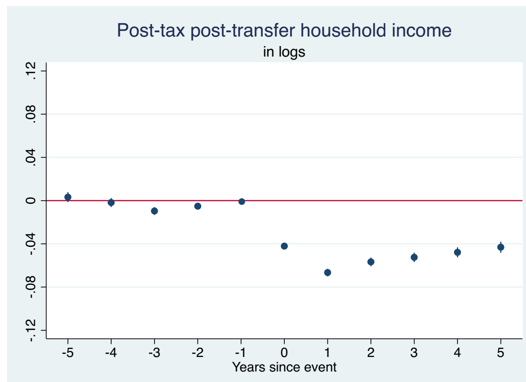
-1. I match, without replacement, each UI recipient to a non UI recipient based on gender, exact age and some lagged characteristics of year -1: occupation (6 categories: manager, skilled employee, unskilled employee, blue-collar worker etc) marital status (dummy for being married), region of residence (5 categories), previous unemployment history (dummy for being never unemployed since 1980).

Figure 3.1: *Unemployment and health in the cross-section*

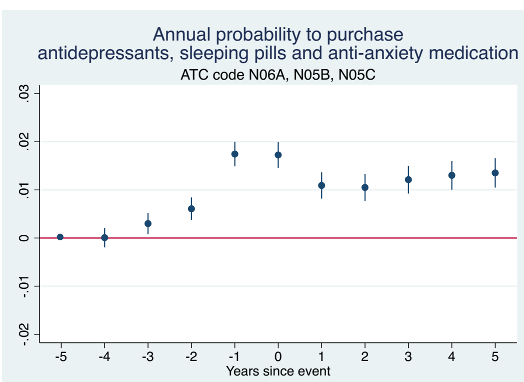
(a) *Unemployment benefits*



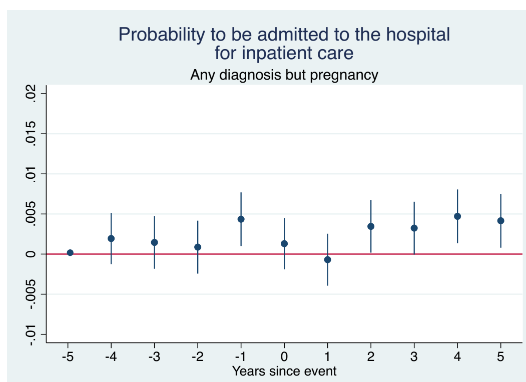
(b) *Household disposable income*



(c) *Mental health*



(d) *Severe physical health issues*



In Figure 3.1 I just plot the difference in means between the two groups over time, taking out individual fixed effects [12]. Patterns are similar if I don't include the individual

[12] I normalize to zero for both groups the value in year -5, except for UI receipt where I use year - 1 as benchmark since UI receipt is by construction 0 for both groups in year - 1

fixed-effects [13]. UI recipients experience a 110,000 (2015) DKK drop in annual wage earnings in year 1, which is equivalent to a 16,500 USD or a 30% drop; earnings then recover but are still 10 % lower (40,000 (2015) DKK) five years out than they were prior to unemployment. In terms of post-tax post-transfer income, the drop, though long-lasting as well, is smaller at impact: around 10% for individual income and 7% for household income as can be seen on Figure 3.1 b. This Figure also shows that there is no strong sign of declining income prior to unemployment in year 0 and this is also true for wage earnings. This stands in contrast with what happens for many health outcomes. In particular, UI recipients are significantly more likely to have purchased antidepressants, sleeping pills or anti-anxiety medication already 3 years before their UI spell as can be seen on Figure 3.1 c. The same kind of pattern holds for opioid painkillers. Regarding severe physical health issues, there is a significant increase in the probability to have received inpatient care at the hospital in the year just before UI receipt (see Figure 3.1 d). This is driven by diseases of the circulatory system. The results shown in Figure 3.1 are for UI recipients of 2002 but results are very similar if I look at any other year in our sample frame. As seen on Figure 3.1 a, UI recipients in a given year are more likely to have received UI in some prior years so this could explain why some health issues show up prior to the event year. But even if I restrict the pool of new UI recipients and the control group to people who have never been unemployed since 1980 [14], the patterns just described stay the same (results available upon request).

Table C.2 shows that UI recipients are also much more likely to die: on average their 5 years and 10 years mortality rate are roughly 30% higher than that of the control group (columns 1 and 2). Again this is also true if I restrict attention to people with no prior history of unemployment: the effect is actually even bigger (column 3). And the effect is stronger for males (column 4 versus 5). Results are similar whether using a linear probability model

[13]Results are also similar whether or not I add additional controls such as an immigrant dummy, very detailed education (over 1,000 categories), time varying detailed marital status, number of years of employment in year -1 (as measured by number of years of social security contribution) and number of years of UI fund membership in year -1

[14]This date is chosen because that's when the data on labor market status start to be collected

or logit regressions. Table C.3 shows that there is no significant increase in the probability to die of cancer: the overall average effect is thus mostly driven by deaths from external causes and from diseases of the circulatory system. There is a 100% increase in the 5 years suicide rate and a 150% increase in the 5 years probability to die from an alcohol-related cause. Of course this increased mortality can be the result i) of the drop in income experienced by UI recipients, ii) of a causal effect of unemployment on mortality and/or iii) of the selection of less healthy people into unemployment. More generally the health issues associated with unemployment that I documented in this subsection calls for a careful research design in order to correct for selection into unemployment.

3.3 Identifying the Causal Effect of Job Loss on Health

I use establishment closures as a way to isolate job losses that are exogenous to the employee's pre-existing health. Recent very interesting work by Hilger (2016) argues that people who work at plants that close tend to be selected compared to people who work at plants that do not close. However, as Hilger (2016) says himself, this is worrying when studying the effect of job loss on an outcome that cannot be observed in the pre-period such as mortality or children's adult outcomes because there is then no way to check whether treatment and control groups are on parallel trends in the pre-period. On the contrary my health panel data on a wide range of outcomes allows me to provide direct graphical evidence in support of the common trend assumption and to be thus confident that there is no selection in terms of health into our treatment group.

3.3.1 Identifying Establishment Closures

Establishment closures. I use a workplace level employment register to identify establishment closures. The data contain a variable which directly codes the status of the workplace in the following year, and in particular whether it closes. This variable also allows me to drop, from both treatment and control groups, establishments that either split or merged. Moreover, for each workplace, the data give for each year of operation, the current workplace identifier but also the workplace identifier in the preceding and in the following year, which enables me to make sure that I do not code as closure a change in workplace identifier. I keep for the treatment group all the establishments which closed between 2001 and 2006 [15], and who had at least 5 employees five years before closure (this excludes 65% of closures). I chose this cutoff to strike a balance between the importance of maximizing power and the need to discard too small establishments for which the closure could be driven by the health of one individual. Results are robust, though less precisely estimated, to using a higher threshold such as for instance 10 employees.

In total, I have 8,233 treated establishment, approximately 1,300 per year, which represents 2.5% of the pool of control group establishments.For this control group, I keep workplaces which operate any time between 2001 and 2006 and who had between 5 and 500 employees five years prior (the 500 cutoff comes from the fact that the biggest establishment which closes in my data had 447 employees 5 years before closure).

Closure year. Following prior work (Eliason and Storrie (2009a) and Browning and Heinesen (2012)) I consider the closure event year to be either the last year of operation of the establishment or one of the 2 preceding ones depending on which of these 3 years saw the biggest reduction in the workforce in absolute value, and making sure that this reduction represents at least 30% of the workforce and at least 3 people. For 70% of establishments the closure year is the last year of operation (for 22% it's the year before and for 8% 2 years before[16]). Given this definition, I also have in my treatment group some people who were not laid off (but who will ultimately lose their job at most 2 years after the event year). They represent 10% of the treatment group. My estimates are thus to be interpreted as Intention-To-Treat estimates. Results are similar if I always consider as closure year the last year of operation but the sample size is almost halved.

To be precise, the employment register are updated every year at the end of November.

[15]These years are chosen in order to have at least 5 years of observation in the pre- and post-periods

[16]the numbers are respectively 58%, 31% and 11% if we do in terms of share of people rather than share of establishments.

Thus when I say event year 2002, I mean that the closure/downsizing/job loss occurred some time between December 1st 2001 and November 30th 2002 and I call 2002 the year 0.

3.3.2 Matched Sampling Procedure to Identify the Comparison Group

Treatment group. The treatment group is composed of everyone of age 25 to 60 whose primary job is at a treated establishment during the closure year and who has at least 5 years of tenure at this establishment. The tenure restriction allows me to focus on people who are losing a stable job, which is presumably a big shock. I also looked at what happens when I relax this assumption and results are qualitatively similar for instance when including everyone who had at least 2 years of tenure. If an individual experiences several establishment closures over the period 2001-2006, I keep the first occurrence.

Pool for control group. The control group consists of all workers at control establishments who satisfy the same age and tenure restriction as the treatment group.

Matching procedure to select control group. I use exact matching instead of propensity score techniques following Azoulay *et al.* (2010), Jaravel *et al.* (2016) and Jaeger (2016). I perform the matching year by year. Potential controls can appear in multiple years. Once they have been used as control for a given year, they are dropped from the pool of controls in the subsequent years. For each year, all treated individuals and potential controls are assigned to a strata based on lagged characteristics, five years before. The characteristics I use for these strata are gender, exact age, tenure quintiles, establishment size quintiles, and occupation (6 categories: managers, skilled employees, unskilled employees, blue collar workers). Then for each treated individual, I select randomly a control within the relevant strata.

3.3.3 Summary Statistics

Table 3.2 presents the summary statistics of the sample in year -1. The difference-in-difference strategy allows for differences in average levels of outcomes variables between treatment and control group and only requires a common trend assumption. However, the summary statistics enable us to assess to what extent the matching procedure has created a balanced comparison group and provides some information to get a sense of the population on which we are estimating the effect of job loss on health. Note that the similarity between the treatment and the control group is not a mechanical effect of the matching since the matching was done on variables in $t-5$.

Table 3.2: *Summary statistics in year -1*

	Treatment	Control
Male	0.59	0.59
	(0.49)	(0.49)
Age	45.16	45.16
	(9.13)	(9.13)
Married	0.66	0.69
	(0.47)	(0.46)
Tenure at estab. (in years)	10.71	10.72
	(5.43)	(5.44)
# of years of employment	20.02	20.02
	(4.73)	(4.77)
# of years of UI fund membership	17.77	17.32
	(6.71)	(7.05)
Gross wage (in 2015 DKK)	368,329	365,984
	(185,468)	(158,251)
	$\approx$ 55,249 USD	54,897 USD
Post-tax post transfer household	412,057	421,580
income (in 2015 DKK)	(275,618)	(337,789)
	$\approx$ 61,809 USD	63,237 USD
Estab. size	45,62	54,21
	(67,42)	(79,66)
# visits to G.P.	2.55	2.50
	(3.21)	(3.11)
Takes antidepressants, anti-	0.09	0.09
anxiety medication or sleeping pills	(0.29)	(0.29)
Takes painkillers	0.19	0.18
	(2.00)	(1.83)
Received inpatient care	0.07	0.07
	(0.25)	(0.25)
# of observations	24,234	24,234

3.3.4 Estimating Equations and Identification

Figures 3.2 through 3.4 show the results of event studies, comparing the within individual variation over time for the treatment group and the control group. More precisely, they plot the α_j coefficients from the following specification:

$$Y_{i,t} = \sum_{j=-5}^{6} \alpha_j (DistYear_t = j) \times Treatment_i + \sum_{j=-5}^{6} \beta_j (DistYear_t = j) + Indiv_i + \epsilon_{i,t} \qquad (3.1)$$

where $Y_{i,t}$ is the economic or health outcomes of indiv. i in year t; $Treatment_i$ is a dummy for being in an establishment that closes; $DistYear_t = j$ are dummies equal to 1 if Year t is j years apart from the event year; and $Indiv_i$ are individual fixed-effects. Standard errors are clustered at the individual level. We normalize the α_{-5} coefficient to 0. The common trend identification assumption, which can be very easily checked visually, is that for each $j < 0$ and for each health outcome of interest, the α_js are not statistically significantly different from zero. The effect of job loss on health is then given by the α_js for all $j > 0$.

For the health outcomes for which the event studies look totally flat both before and after the shock, I pool together all the years in the pre- and post-period respectively in order to assess the magnitude of the zero effect with more precision. That is I report the α coefficient from the following difference-in-difference equation:

$$Y_{i,t} = \alpha After_t \times Treatment_i + \beta After_t + \gamma T_t + Indiv_i + \epsilon_{i,t} \qquad (3.2)$$

Results are similar whether I replace the individual fixed effects by a $Treatment_i$ dummy.

Mortality specifications. For mortality, we estimate both linear probability models and logit specifications. For linear probability models, I report the α coefficient from equations of the form

$$p_{i,t+1,t+5} = \alpha Treatment_{i,t} + \delta X_i + T_t + \epsilon_{i,t} \qquad (3.3)$$

where $p_{i,t}$ is the probability that individual i dies between year $t+1$ and $t+5$ (or sometimes t+10, either of any cause or by cause of death), where t is the event year or placebo event year of individual i. X_i are individual controls and T_t are Year fixed effects. I also show graphically the death probability of both treatment and control groups, by year, from the year of the event to 6 years out.

For logit specifications, I report α coefficient of equation of the form

$$Ln(\frac{p_{i,t+1,t+5}}{1-p_{i,t+1,t+5}}) = \alpha Treatment_i + \delta X_{i,t} + T_t + \epsilon_{i,t} \tag{3.4}$$

Where notations are the same as for equation (3.3). α gives the increase in the log-odds of death associated with experiencing an establishment closure, holding constant the other variables in the model. Because mortality rates are typically quite small, α can be interpreted as the percentage increase in the 5 years mortality rate.

3.4 The Effect of Establishment Closures on Economic and Health Outcomes in Denmark in the 2000s

3.4.1 Effect on Economic Outcomes

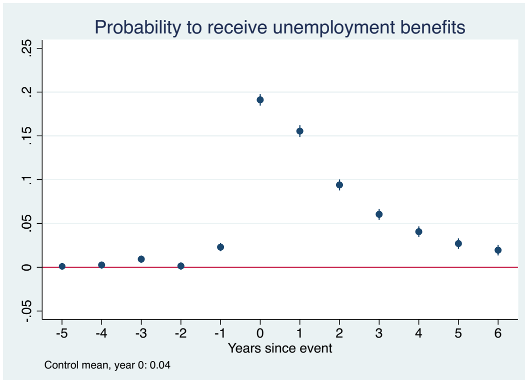
Figure 3.2 a shows that workers in the treatment group are much more likely to go through an unemployment spell than those in the control group. 4% of the control group receives unemployment benefits at some point during year 0 as opposed to 24% in the treatment group. Conditional on receiving UI in year 0, their unemployment duration is slightly longer: they receive benefits during 18 weeks in year 0, relative to 15 weeks for the control group. [17] The treatment group is also more likely to have zero annual earnings, which can be seen as a proxy for dropping out of the labor force. This effect is more long-lasting than the unemployment effect, as can be seen on Figure 3.2 b: the treatment group is on average 7 percentage points more likely to have zero annual earnings in the year following the event, relative to a control mean of 2%, and still 5 percentage points more likely 5 years out.

Figure 3.2 c and d then show the effect of plant closure on earnings and income. Workers who experience an establishment closure suffer from a long-lasting drop in annual earnings:

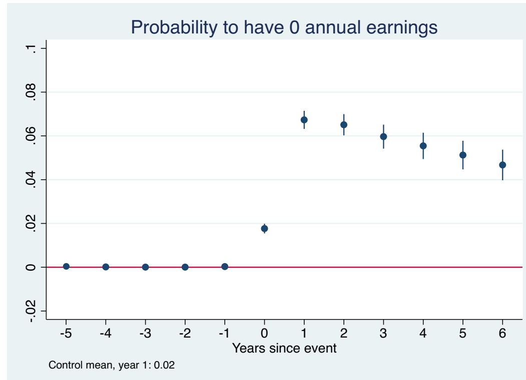
[17] And in total, still conditional on receiving UI in year 0, the treatment group receives 51 weeks of benefits over a period of 92 weeks, whereas the control group receives 45 weeks of benefits over a 97 weeks period.

Figure 3.2: *Effect of establishment closures on economic outcomes*

(a) *Unemployment*

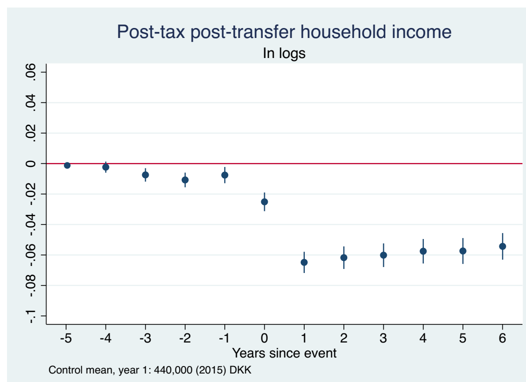


(b) *Zero annual wage earnings*



(c) *Annual wage earnings*

(d) *Post-tax post-transfer income*



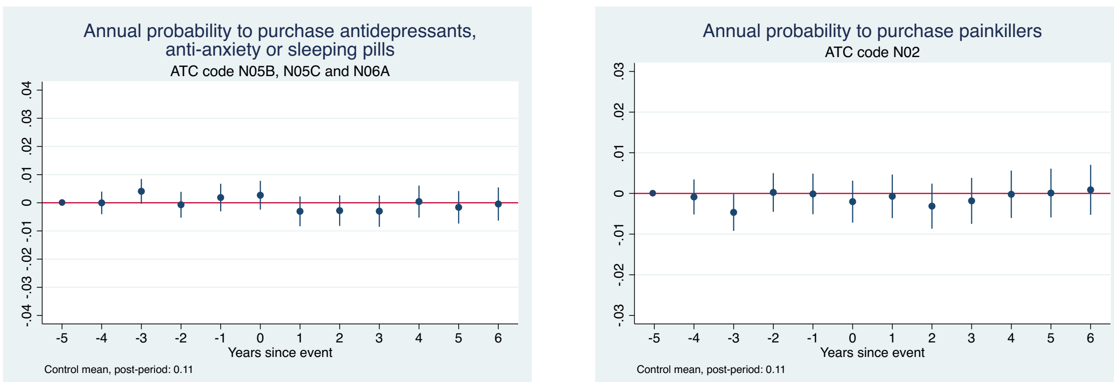
on average, 60,000 (2015) DKK in the year following displacement; and their earnings are still 40,000 (2015) DKK lower than that of the control group five years out. That's roughly equivalent to a 9,000 USD or 17 % drop in year 1 and a 6,000 USD or 12% drop in year 5. In terms of household post-tax post-transfer income, which is arguably the relevant economic outcome when thinking about health, the effect is smaller. Workers in the plant closing group experienced a 6% income drop (with the average disposable household income for the control group in the post-period being 450,000 (2015) DKK or 67,500 USD). Most of the insurance against the earnings drop comes from the transfer and tax system, not the spousal adjustment. Indeed if we look at individual, instead of household, disposable income, we observe a drop of 8%. [18]

3.4.2 Effect on Health Outcomes

Figures 3.3 through 3.4 and Table 3.3 through 3.4 show the effect of establishment closures on some health outcomes.

Figure 3.3: *Effect of establishment closures on prescription drugs purchases*

(a) *Antidepressants and related drugs* **(b)** *Painkillers*



The event studies provide graphical evidence that there is no selection in terms of health in our treatment since there is no significant differences in the pre-period between the treatment and the control group. The regressions help assess the magnitude of the effects by pooling together all the years in the pre- and post-period respectively

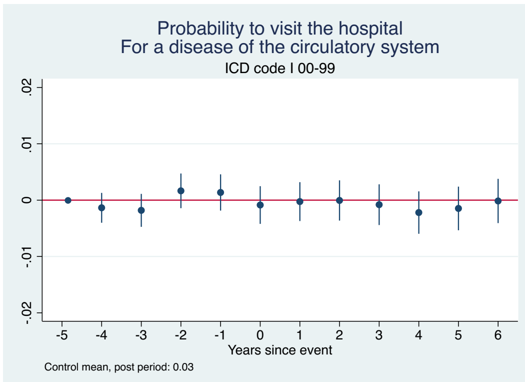
Effect on Mental Health

The main outcome of mental health that I consider is the purchase of antidepressants, anti-anxiety medication and sleeping pills. On the extensive margin, that is the probability to purchase any of these drugs at some point during the year, I can rule out increases of 0.1 percentage point relative to a control mean of 6% (see Figure 3.3 a and Table 3.3). Results are similar if one looks at the intensive margin, that is the number of days per year under

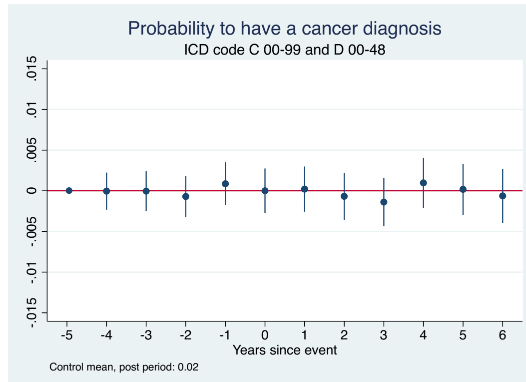
[18] I use a specification in levels for earnings and in logs for disposable income because for earnings, as opposed to income, there are many zeros in the post-event period.

Figure 3.4: *Effect of establishment closures on hospital visits and diagnoses*

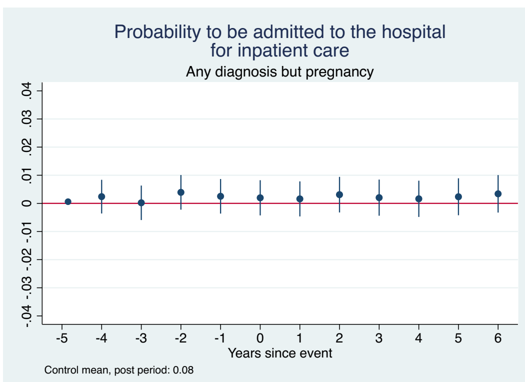
(a) *Heart attacks*



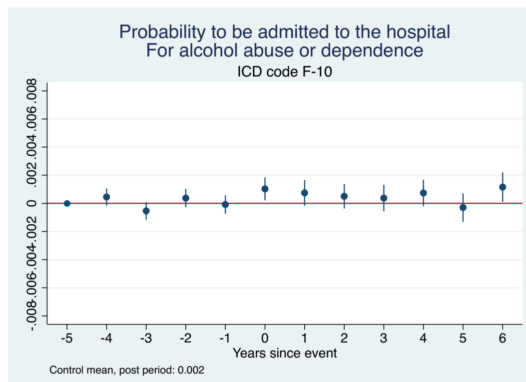
(b) *Cancer diagnosis*



(c) *Inpatient care*



(d) *Alcohol-related issues*



treatment.

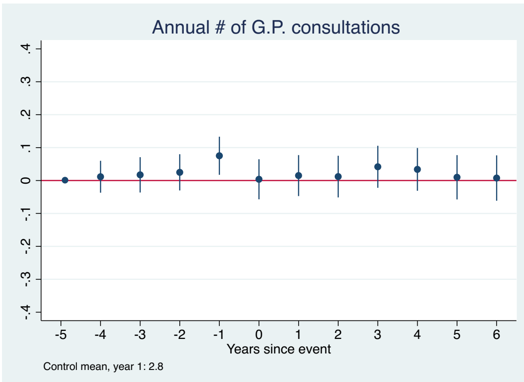
Antidepressants and related drugs are prescribed either by psychiatrists or by General Practitioners. I do not see any effect on visits to psychiatrists. I do not see any effect either on visits to General Practitioners (see Figure 3.5 a). Table 3.5 shows that I can rule out increase of 0.03 in the number of visits per year, relative to a control mean of 2.8.

There does not seem either to be any significant effect on purchases of opioid painkillers.

As can be seen on Figure 3.4 d, there is one outcome related to mental health on which there seems to be an effect: it is the probability to visit the hospital for an alcohol-related issue. This pools together both visits to the emergency room for alcohol abuse and inpatient or outpatient care related to alcohol addiction. This effect is entirely driven by males. However starting from such a low baseline mean, it is not sure that this result is very

Figure 3.5: *Effect of establishment closures on doctors' visits*

(a) *General Practitioners*



(b) *Physiotherapists (for which there is a co-payment)*

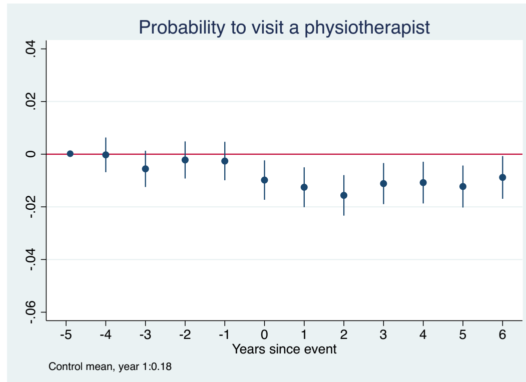


Table 3.3: *Prescription drugs*

VARIABLES	(1) Anti-depressants	(2) Opioid painkillers	(3) Anti-addiction	(4) Diabetes related
Displaced x After	-0.000457	-0.000963	0.000403	0.00221***
	(0.000820)	(0.000966)	(0.000396)	(0.000432)
Control Mean After	0.06	0.05	0.01	0.03
Obs.	578,496	578,496	578,496	578,496
R-squared	0.012	0.006	0.003	0.016
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (3.2). The dependent variable are dummies for whether one purchased any antidepressants (ATC code N06A, column 1), painkillers (ATC code N02A, column 2), anti-addiction (ATC code N07B, column 3) or diabetes-related drugs (ATC code A01, column 4) during the year.

meaningful, it could just be a false positive.

Effect on Doctors' Visits

The Danish health insurance system reimburses fully all doctors' visits, except for dentists, physiotherapists and psychologists for which there are co-payments. This can explain the patterns I find in doctors visits. Indeed, as already mentioned, I do not find any significant

Table 3.4: *Hospital diagnosis*

VARIABLES	(1) Any inpatient care	(2) Cardio-vascular issues	(3) Cancer	(4) Alcohol related issues
Displaced x After	0.000487	-0.000797	-0.000206	0.000567***
	(0.00133)	(0.000802)	(0.000634)	(0.000206)
Control Mean After	0.077	0.034	0.021	0.002
Obs.	578,496	578,496	578,496	578,496
R-squared	0.000	0.002	0.003	0.000
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (3.2). The dependent variables are dummy variables for whether one received during the year i) any inpatient care (column 1); ii) any diagnosis of a disease of the circulatory system (col. 2); iii) any cancer diagnosis (col. 3); iv) any alcohol-related diagnosis (col. 4).

change in the number of visits to the General Practitioner, but there is a significant, though small, decrease in the annual probability to go to a physiotherapist (a one percentage point decrease, relative to a 17%control mean, see Figure 3.5 b and Table 3.5) and in the annual probability to see a dentist (half a percentage point decrease, relative to a 70%control mean). However there is no significant effect on the probability to visit a psychologist.

Effect on Severe Physical Health Outcomes

Figure 3.4 a shows that the treatment group is not significantly more likely to be hospitalized for a disease of the circulatory system, such as a heart attack, than the control group. It is not more likely either to be diagnosed with cancer, or to be hospitalized for any kind of disease that requires inpatient care (Figure 3.4 b and c) . As Table 3.4, I can rule out effects of the order of 3 to 4% for all these outcomes.

Table 3.5: *Doctors visits*

VARIABLES	(1) G.P.	(2) Psycho-logist	(3) Physio-therapist	(4) Dentist
	# visits/year	Any visit in the year		
Displaced x After	-0.00829	2.75e-05	-0.00947***	-0.00737***
	(0.0128)	(0.000390)	(0.00158)	(0.00154)
Control Mean After	2.78	0.01	0.17	0.78
Obs.	578,496	578,496	578,496	578,496
R-squared	0.007	0.000	0.002	0.002
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (3.2).

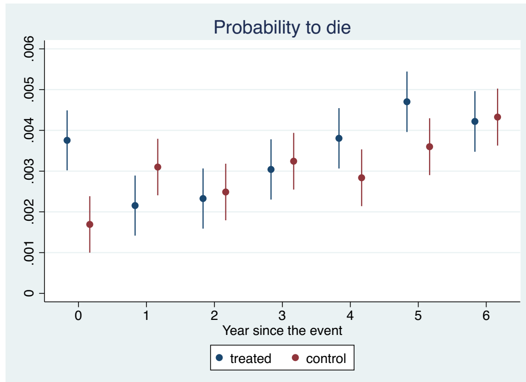
3.4.3 Effect on Mortality

As documented in previous work, I also find that the death hazard is higher for the treatment group than for the control group in the year of displacement (see Figure 3.6 a). However because the employment data is at the annual level, I cannot be sure that the death occurred after the closure and it can very well be on the contrary that the establishment closure was at least in part driven by this death. This concern seems all the more relevant that this difference in death hazard is driven almost entirely by smaller establishments (see Figure 3.6 b and c). This why my preferred estimates of the causal effect of job loss on mortality are the ones that do not include year 0 in the analysis. Yet I report results both ways, in particular for comparison with prior work.

Table 3.6 reports the coefficient of a dummy variable for being in the treatment group on the 5 years mortality rate, using either a linear probability model or a logistic regression framework (see equations 3.3 and 3.4), controlling for many observables listed at the end of the table. Column 1 gives the effect on the overall mortality rate, whereas the next columns looks at the effect by cause of death. Panel A reports results when looking at the 5 years mortality rate from year 1 to year 5, and panel B from year 0 to year 4. In both cases I do not

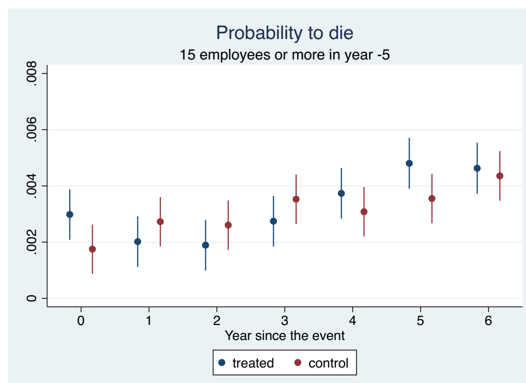
Figure 3.6: *Effect of establishment closures on death hazards*

(a) *Overall*



(b) *Small establishments*

(c) *Big establishments*



find any significant effect on mortality, yet the point estimate is bigger when I include year 0. When I don't, I can rule out increase of the order of 15% of the overall 5 years mortality rate (0.25 percentage point relative to a 1.5% mean). Given the data the longer horizon I can look at is the 10 years mortality rate and the results are very similar. The fact that the point estimate is positive comes from deaths from external causes, for which there is a strong but only marginally significant effect. When including year 0, the point estimate of the effect on the 5 years mortality rate is higher and this is entirely driven by deaths from circulatory diseases.

Table 3.7 shows that when looking by subgroups, I do not find any significant effect but the point estimate for males is higher, especially so for younger males.

Table 3.6: *5 years mortality rate (Deaths/100) - By cause of death*

	(1) All causes	(2) Circulatory disease	(3) Cancer	(4) External cause	(5) Alcohol related
			Panel A: Not including Year 0		
Linear proba.	0.0367	0.00608	-0.0690	0.0560*	0.0227
model	(0.112)	(0.0515)	(0.0791)	(0.0301)	(0.0293)
Control Mean	1.51	0.31	0.79	0.08	0.09
Logit	0.0211	0.0127	-0.0957	0.533*	0.231
model	(0.0745)	(0.162)	(0.106)	(0.289)	(0.288)
			Panel B: Including Year 0		
Linear proba.	0.142	0.0340	-0.00248	0.0567*	0.0264
model	(0.107)	(0.0488)	(0.0770)	(0.0307)	(0.0272)
Control Mean	1.33	0.27	0.72	0.08	0.07
Logit	0.0979	0.110	-0.00860	0.537*	0.309
model	(0.0781)	(0.172)	(0.108)	(0.283)	(0.313)
Observations	48,467	48,467	48,467	48,467	48,467

Standard errors in parentheses - *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Controls include log of mean earnings in the 5 years prior, log of average household disposable income in the 5 years prior, age dummies, gender, a dummy for being foreign born, and some controls defined in year -1: occupation, tenure dummies, number of weeks of unemployment since 1980, number of years of employment, number of years of unemployment insurance membership , a dummy for being married or having a partner, region of residence, a dummy for living in Kopenhagen.

Table 3.7: *5 years mortality rate (Deaths/100) - By subsample*

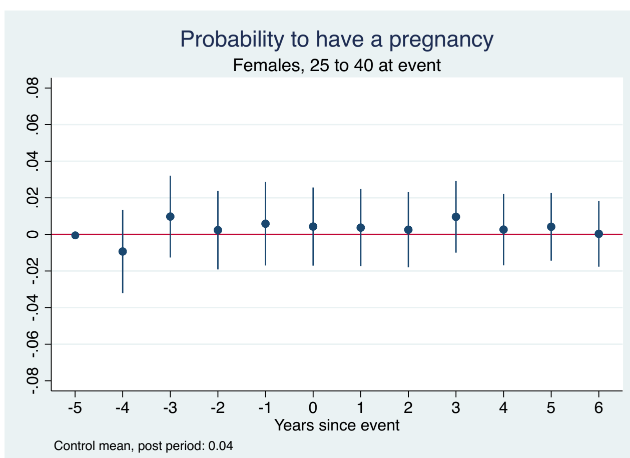
	(1) Males	(2) Females	(3) Males Age $\leq$ 47	(4) Males Age $>$ 47	(5) Males Tenure$<$9	(6) Males Tenure$\geq$9
Linear proba.	0.146	-0.121	0.177	0.0808	0.233	0.0767
model	(0.157)	(0.153)	(0.134)	(0.300)	(0.214)	(0.227)
Control Mean	1.71	1.23	0.58	2.99	1.36	1.99
Observations	28,633	19,834	15,200	13,433	12,905	15,728

Standard errors in parentheses - *** p<0.01, ** p<0.05, * p<0.1
Same controls as for Table 3.6

3.4.4 Effect on Fertility Decisions

Figure 3.7 shows that, when restricting attention to females in child-bearing age, the treatment group is neither more nor less likely to become pregnant in any of the five years following the establishment closure than the control group.

Figure 3.7: *Effect of establishment closures on fertility decisions*



3.5 Discussion

As already mentioned in the introduction, it is hard to compare results across countries. For instance Sullivan and Von Wachter (2009) show that in the US mass layoffs have a significant and strong adverse effect on mortality. They find that Pennsylvanian workers who were laid off as part of a mass-layoff during the early 1980s recession had a 50-100% higher likelihood of dying in the year following displacement. 20 years out, mass-layoffs still induce a 10-15% increase in the death hazard. But the back of the envelope calculation they provide shows that the magnitude of their effect is consistent with a pure income channel explanation. In their setting, displaced workers experience a 50% drop in annual earnings in the year following displacement and their earnings are still 15-20% lower than the control group 5 years out. In contrast, in my setting, the earnings drop is much smaller at impact (15%), although long-lasting as well, and more importantly the drop in post-tax post-transfer household income is even smaller: 6%. With such a small drop, if we compute an upper bound of a potential causal effect on health using health-income gradients, we find very small figures that lie within my confidence interval.

However, there are several papers that look at one outcome in particular in Scandinavian or European countries. Consistent with what I find, some papers find zero or negligible effects. In particular Browning *et al.* (2006), who also study Denmark, find no effect of displacement on the 4 years probability of being hospitalized for potentially stress-related diseases of the circulatory system and digestive system: e.g. high blood pressure, heart disease, gastric catarrh and gastric ulcers. Kuhn *et al.* (2009), who look at the short-run effect of plant closures on health costs in Austria, find that overall expenditures on medical treatments are not significantly affected. The only exception is expenses for mental health drugs which increase for males but the magnitude is very small. Other papers report significant adverse effects of job loss on health. For instance Eliason and Storrie (2009b) report a positive effect of establishment closures in Sweden in the 1980s on alcohol-related hospitalizations. This is actually not inconsistent with what I find, although I tend to downplay the meaningfulness of this outcome given its extremely low baseline mean.

However my results can seem to stand somewhat in contrast with Browning and Heinesen (2012) and Eliason and Storrie (2009a) who find a positive effect of plant closures on males mortality, respectively in Denmark (in 1986-2002) and Sweden [19]. First, I also find a bigger point estimate for males but I can rule out the magnitudes that they report as average effects. Second, Browning and Heinesen (2012) include the year of displacement in their analysis. For them, the treatment group is 79% more likely to die in the year of displacement than the control group, which is also what I find. But I tend to disregard deaths that occur in the year of displacement as they might suffer from reverse causality. Indeed this higher likelihood of dying holds across causes of death, including cancer, which does not make much sense as an immediate consequence of job loss. However Browning and Heinesen (2012) also find a significant 35% higher death probability when looking at a 4 years horizon (including the year of displacement), whereas, even if I include the year of displacement and restrict to males, I can rule out effects of 35% (but our confidence intervals, with these same restrictions, overlap, mine is from -0.2% to 35%, whereas theirs is from 20% to 50%). Yet Browning and Heinesen (2012) are looking at closures that occurred between 1986 and 2002. One possible reconciliation between my magnitudes and theirs is that the late 1980s and 1990s was a different period than the 2000s I'm looking at and indeed at that time active labor market policies and the so-called "flexi-curity" system were not yet implemented.

3.6 Conclusion

This paper documents the causal effect of job loss on a wide variety of health outcomes for Danish people strongly attached to their job. I find that overall job losses driven by establishment closures in Denmark between 2001 and 2006 for people with at least 5 years

[19] Rege *et al.* (2009) also report a positive effect of job loss on mortality. They are are primarily interested in the effect of mass layoffs on disability pension utilization in Norway, for which they find a large effect. However they also report an estimate of the effect on mortality for both genders pooled together. They find that workers in plants that downsized by 60% between 1995 and 2000, compared to workers in plants which experienced positive growth, have a 9% higher probability to have died by 2001, significant at the 10% level, which is not inconsistent with what I find though I tend to think that these estimates could be partly biased by reverse causality issues given the timing / horizon definition.

of tenure at their establishment did not cause significant large effect on health. Not only do I observe no change in health care consumption, except for doctors visits for which there is a co payment, but I also find no significant increase in the death probability, as was the case for the Pennsylvanian workers during the 1982 recession studied by Sullivan and Von Wachter (2009). I interpret my results as showing that, presumably with an adequate set of policies, it is possible to make the causal effect of job loss very small.

I see several important directions for future research. The main one would be to perform a similar comprehensive analysis on other countries than the Scandinavian ones, in particular the United States in more recent years. Of course this all relies on how to get access to relevant data.

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Appendix A

Appendix to Chapter 1

A.1 Theory Appendix

A.1.1 Proof of Lemma 1

The output in this economy is

$$\ln Y_t = \int_{j\in\mathcal{J}} \ln A_{jt} dj \equiv (1-u)\ln \bar{A}_t$$

Then after a small time interval Δt :

$$\begin{aligned}
\ln Y_{t+\Delta t} &= \int_{\mathcal{J}} \left[x\Delta t \times 0 + (1-x\Delta t)\ln A_{jt}\right] dj + \int_{\mathcal{J}'} \left[\frac{m}{v}\Delta t \ln(1+\lambda)\bar{A}_t + \left(1-\frac{m}{v}\Delta t\right)\times 0\right] dj \\
&= (1-x\Delta t)(1-u)\ln \bar{A}_t + u\frac{m}{v}\Delta t \ln(1+\lambda)\bar{A}_t \\
&= [1-u]\ln \bar{A}_t + m\Delta t \ln(1+\lambda)
\end{aligned}$$

Hence we can find the growth rate as

$$g = \lim_{\Delta t\to 0} \frac{\ln Y_{t+\Delta t} - \ln Y_t}{\Delta t} = m\ln(1+\lambda)$$

A.1.2 Derivation of Equation (1.10).

Note that using the fact that in steady state $\dot{E} = gE$ and $\dot{U} = gU$, and after subtracting the second equation from the first:

$$(r - g)(E - U) = BY + (1 + x)(U - E),$$

where $B \equiv \beta\pi - b$.

This yields:

$$E - U = \frac{BY}{r - g + 1 + x}.$$

Then, substituting for $(E - U)$ in the above asset equations (1.8) and (1.9), yields:

$$U = \left[bY + \frac{BY}{r - g + 1 + x} \right] \frac{1}{r - g}; \text{ and } E = \left[\beta\pi Y - \frac{xBY}{r - g + 1 + x} \right] \frac{1}{r - g}.$$

so that, after substituting for E and U in the expression for W, and using the fact that in equilibrium $u = x/(1 + x)$, we get:

$$W = \frac{Y}{r - g} \left[\beta\pi - \frac{xB}{1 + x} \right].$$

A.1.3 Derivation of Equation (1.12).

Recall that:

$$W = uU + (1 - u)E,$$

where E and U are expressed in (1.8) and (1.9). Now, using the fact that $m(u, v)/u = (1 - u)x/u$ and that in steady state $\dot{E} = gE$ and $\dot{U} = gU$, we obtain:

$$E - U = \frac{BY}{r - g + x/u}.$$

Substituting for $(E - U)$ in the asset equations (1.8) and (1.9), yields:

$$U = \left[bY + \frac{[(1 - u)x/u]BY}{r - g + x/u} \right] \frac{1}{r - g},$$

and

$$E = \left[\beta\pi Y - \frac{xBY}{r-g+x/u}\right]\frac{1}{r-g}.$$

so that:

$$W = \frac{Y}{r-g}[ub + (1-u)\beta\pi].$$

A.1.4 Transitional Dynamics

In this Appendix we consider a sudden change in the entry rate to analyze its impact on the economy's transition from one steady state to another.

Assume that the economy starts at its steady state with entry rate x_{old} and the entry rate suddenly increases from x_{old} to x_{new} such that $x_{new} > x_{old}$. We start by focusing on the unemployment rate first. After the change in the entry rate, the flow equation of the unemployment rate becomes

$$\dot{u}_t = (1-u_t)\,x_{new} - m_t.$$

Since $u_t = v_t$ in every period, we get $m_t = u_t = v_t$; therefore

$$\dot{u}_t = x_{new} - (1+x_{new})\,u_t. \tag{A.1}$$

The solution to this differential equation is simply

$$u_t = \left[\frac{x_{old}}{1+x_{old}} - \frac{x_{new}}{1+x_{new}}\right]e^{-(1+x_{new})t} + \frac{x_{new}}{1+x_{new}}.$$

Recall that the growth rate is simply $g = m\ln\lambda$. Therefore the aggregate growth rate of this economy during transition is

$$\begin{aligned} g_t &= \left\{\left[\frac{x_{old}}{1+x_{old}} - \frac{x_{new}}{1+x_{new}}\right]e^{-(1+x_{new})t} + \frac{x_{new}}{1+x_{new}}\right\}\ln\lambda, \\ &= g_{ss}^{new} - e^{-(1+x_{new})t}\left[g_{ss}^{new} - g_{ss}^{old}\right]. \end{aligned}$$

Now we turn to the value functions

$$rE_t - \dot{E}_t = \beta\pi Y_t + x_{new}(U_t - E_t), \text{ and } rU_t - \dot{U}_t = bY_t + (m_t(u_t,v_t)/u_t)(E_t - U_t).$$

Note that out of the steady state, it is not possible to solve these value functions further analytically. However, we can explore them numerically. For that, we need to determine 6 parameters: λ, x_{new}, x_{old}, ρ, β, and b. Since our model is stylized, our goal here is to show you the numerical properties of the model, rather than trying to provide a detailed calibration exercise. We pick the discount rate, which also corresponds to the interest rate in the benchmark model, to be $\rho = 5\%$. We will set $x_{old} = 6.4\%$ and $x_{new} = 8.7\%$ such that the steady-state unemployment rates are 6% and 8%, respectively. We set $\lambda = 1.18$ in order to obtain an initial steady-state growth rate of 1%. The worker share of output is chosen to be $\beta = 0.9$ such that the profit share of the firm is 10%. Finally, we set the unemployment benefit to be $b = 0.3\%$.

The following figures illustrate this experiment. Until time 0, the economy is at its initial steady state and at $t = 0$, the rate of creative destruction increases from x_{old} to x_{new}. Figure A.1 shows the evolution of the unemployment rate and the Figure A.2 figure shows the effect on equilibrium welfare. For expositional purposes, we plot the welfare after normalizing it by the aggregate output every period.

Figure A.1: *Unemployment rate*

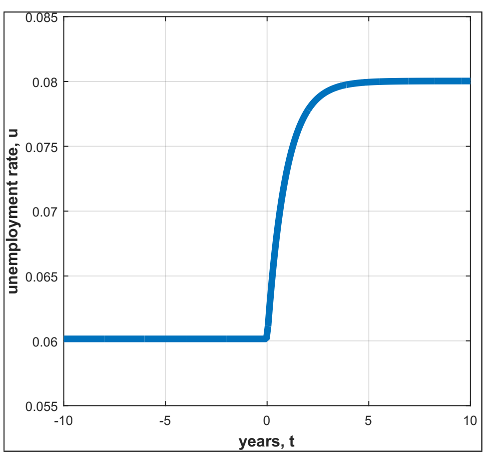
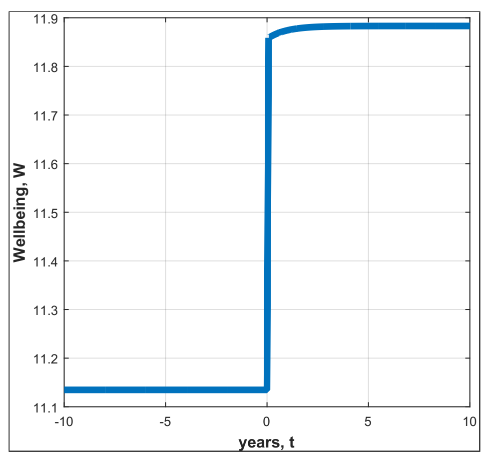


Figure A.2: *Wellbeing*



After the change, the unemployment rate starts to evolve towards its new level according to the law of motion in A.1. What we see is that the convergence is quick and the economy assumes its new steady state value almost after 6 years. The impact on welfare is slightly different. After the sudden change, the welfare function features a sudden jump at time 0 and then starts to evolve towards the new steady state. The big change in welfare occurs at the time of the change in creative destruction and the remaining portion of the transition has much lower impact on the new level of welfare.

Figures A.3 and A.4 illustrate the change in welfare, i.e., $\Delta W_t = W_{t>0} - W_{t=0}$ for different values of the discount rate ρ and unemployment benefit b.

Figure A.3: *Change in wellbeing with different r Values*

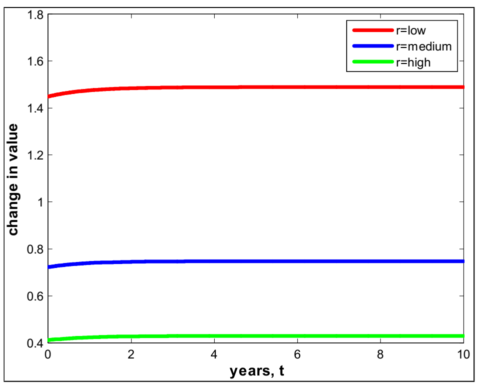


Figure A.4: *Change in wellbeing with different b Values*

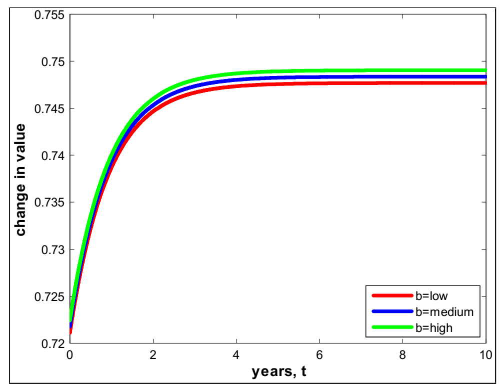


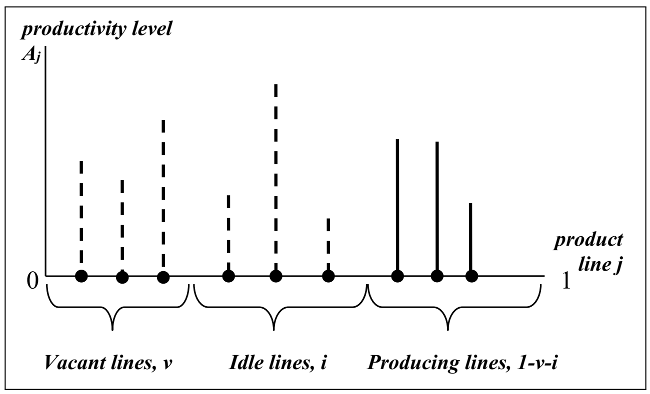
Figure A.3 shows that the increase in welfare is higher, the lower is the discount rate. Similarly, Figure A.4 shows that the increase in welfare after the increase in entry is higher, the higher is the unemployment benefit. Hence, the steady-state results of the benchmark model are confirmed in this simple numerical exercise even when the transitions are taken into account.

A.1.5 Exogenous Job Destruction

So far, the only source of job destruction, as well as job creation, was new entry. However, in reality, new entry is not the only source of job destruction. Following Pissarides (1990) we now allow for an additional -exogenous- source of job destruction rate. To capture this, we assume that each job is destroyed at the rate ϕ. Upon this destruction shock, the worker joins the unemployment pool and the product line becomes *idle*. When a new entrant comes into this product line at the rate x, it first posts a vacancy in which case then the same product line moves from being *idle* to being *vacant*. Finally, when a vacant product line finds a suitable worker, the product line becomes *producing*. Similarly, if a new entrant enters into a currently producing line, then the sector becomes directly *vacant*, the incumbent worker joins the unemployment pool, and the new entrant searches for a new suitable worker.

In steady state, there will a constant fraction of product lines that are *vacant* (of measure v), a constant fraction of lines that are *idle* (of measure i), and the remaining fraction will be *producing*. We illustrate this economy in Figure A.5:

Figure A.5: *Model economy*



Next, one can compute the steady-state fraction of idle, vacant, and producing lines using the following flow equations:

$$
\begin{aligned}
(1-v)\,x &= m; \\
(1-v-i)\,\phi &= ix.
\end{aligned}
$$

The left-hand side of the first equation is the flow of sectors *into* the *vacant* stage: it is equal to the flow of productive sectors which become (directly) vacant, namely $(1-v-i)x$, plus the flow of idle sectors which become vacant, namely ix. The sum of these two terms is equal to $(1-v)x$. The right-hand side of the first equation is the flow of sectors *out of* the vacant stage: it is simply equal to the job matching rate m.

Similarly, the left-hand side of the second equation is the flow *into* the *idle* stage: it is equal to the flow of producing sectors which become idle, namely $(1-i-v)$ times the flow probability ϕ of an exogenous job destruction shock in such a sector. The right-hand side is equal to the flow *out of* the idle stage. It is equal to the number of idle sectors times the flow probability of a new entry in such a sector, which will make it become vacant: namely, ix.

By definition unemployment is equal to all the product lines where there is no production, therefore:

$$u = i + v$$

Hence the above flow equations can be reexpressed as

$$(1-v)\,x = m, \text{ and } (1-u)\,\phi = (u-v)\,x. \tag{A.2}$$

Moreover, the matching technology is such that

$$m = u^{\alpha} v^{1-\alpha} \tag{A.3}$$

Substituting (A3) into (A2) we get

$$(1-v)\,x = u^{\alpha} v^{1-\alpha}, \text{ and } (1-u)\,\phi = (u-v)\,x. \tag{A.4}$$

These last two equations give us a system of 2-equations and 2-unknowns. For analytical tractability, assume $\alpha = 0.5$. Then the equilibrium unemployment rate solves a simple quadratic equation, yielding the solution:

Figure A.6: *Unemployment rate vs x*

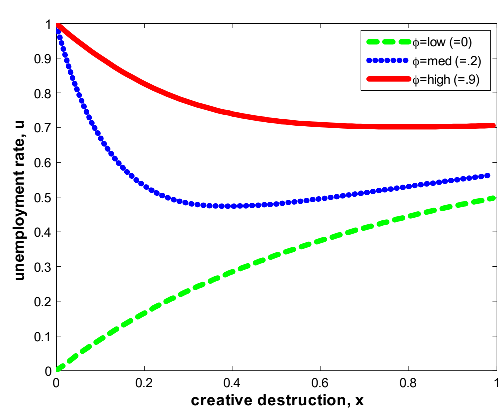
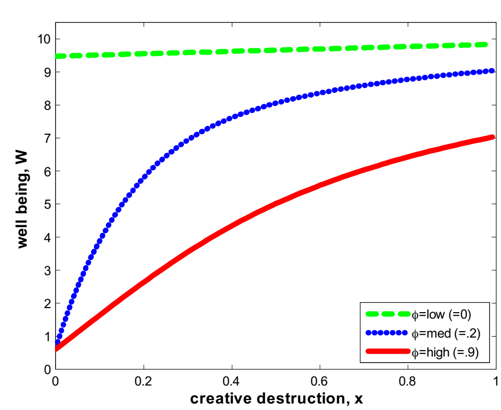


Figure A.7: *Wellbeing vs x*



Unlike in the model without exogenous job destruction, the relationship between entry and unemployment, and therefore between growth and unemployment, is no longer automatically monotonic. Here, jobs are being destroyed both by creative destruction at the rate x and also by the exogenous shock ϕ. The only source of job creation is job postings that happens though new entrants. Hence, one would expect that when ϕ is large, then the main role of entry will be job creation whereas when ϕ is very low, then we are back to the previous model and entry will mainly create unemployment. This is evident in Figure A6 that plots the unemployment rate against the entry rate for various values of the exogenous destruction rate $\phi \in \{0, 0.2, 0.9\}$. As expected, as $\phi \to 0$, entry (turnover) and unemployment becomes positively correlated: in this case the *job destruction effect* dominates the job creation effect. On the other hand, when ϕ is very high, then the relationship is negative: in that case the *job creation effect* of innovation-led growth on unemployment dominates the job destruction effect.

Now, moving to the relationship between the innovation-led turnover rate x and well-being W, Figure A.7 shows that the higher the exogenous job destruction rate ϕ, the more positive the effect of x on W, especially for small initial values of x : this is not surprising, as the lower x is relative to ϕ, the more the job creation effect of increasing x dominates the job destruction effect.

A.1.6 Risk Aversion

We now consider the case where individuals are risk averse with instantaneous preferences $U = \ln C$, and compute the steady-state value functions under this assumption. Recall that the individuals discount the future at the rate ρ. Then the value functions for currently employed and unemployed individuals satisfy the asset equations:

$$\begin{aligned} \rho E - \dot{E} &= \ln(\beta \pi Y) + x(U - E) \\ \rho U - \dot{U} &= \ln(bY) + (m(u, v)/u)(E - U) \end{aligned}$$

Now the value functions take the following form

$$
\begin{aligned}
E &= \frac{1}{\rho}\left[\ln(\beta\pi) - \frac{x\ln(\beta\pi/b)}{1+x+\rho} + \frac{g}{\rho} + \ln Y\right] \text{ and} \\
U &= \frac{1}{\rho}\left[\ln(b) + \frac{\ln(\beta\pi/b)}{1+x+\rho} + \frac{g}{\rho} + \ln Y\right].
\end{aligned}
$$

Using the above expressions for E and U, wellbeing can be shown to be equal to:

$$
W^{u(c)=\ln c} = \frac{1}{\rho}\left[\frac{x}{1+x}\ln(b) + \frac{1}{1+x}\ln(\beta\pi)\right] + \frac{1}{\rho}\left[\frac{g}{\rho} + \ln Y\right]
$$

This expression shows that for given growth rate wellbeing is affected more negatively by creative destruction than in the risk neutrality case: since here the agent is risk averse, more asymmetry between the returns when employed $(\beta\pi)$ and when unemployed (b) lowers her wellbeing by more.

The net effect of creative destruction on wellbeing will ultimately depend upon the size of the asymmetry and upon the magnitude of the growth effect: in particular, if the unemployment benefit is too low relative to the wage rate, or if the growth effect is too small, then the overall effect of creative destruction on wellbeing is negative. More precisely:

Proposition 3. *When agents are risk averse with $U = \ln C$ and the unemployment benefit is sufficiently low, namely $b < \frac{\beta\pi}{\lambda^{1/\rho}}$, then a higher turnover rate x decreases life satisfaction W:*

$$
\frac{\partial W^{u(c)=\ln c}}{\partial x} < 0.
$$

This proposition states that, when agents are risk averse, job loss is perceived more detrimentally than when they are risk neutral. Consequently, there is a range of unemployment benefits for which higher turnover reduces life satisfaction for risk-averse individuals with log preferences whereas it would increase life satisfaction for risk-neutral individuals:

$$
\beta\pi\left[1 - \frac{\ln\lambda}{\rho}\right] < b < \frac{\beta\pi}{\lambda^{1/\rho}}
$$

Finally, moving continuously from the baseline case where individuals are risk-neutral towards the risk-averse case where individuals have log preferences, makes the effect of creative destruction on life satisfaction become increasingly less positive (or increasingly more negative).[1]

A.1.7 Endogeneizing the Turnover Rate

In this section of the Appendix, we endogenize the turnover rate x. To this end, we first solve for the value function of posting a vacancy (V) and a filled vacancy (P) that is currently producing. If the cost of posting a vacancy is cY, which we think as the registration fee that has to be paid to the government, then we can write the value of a vacancy as

$$rV - \dot{V} = -cY + \frac{m}{v}\left[P - V\right].$$

Note that a vacancy is filled at the rate $\frac{m}{v}$. The value of a filled vacancy is

$$rP - \dot{P} = \pi Y + x\left[0 - P\right]$$

In steady state we get the following values

$$P = \frac{\pi Y}{r - g + x} \tag{A.5}$$

and

$$V = \frac{Y}{r - g + 1}\left[-c + \frac{\pi}{r - g + x}\right]. \tag{A.6}$$

Now we are ready to introduce free entry. There is a mass of outsiders enter at the flow of innovation x. Then the free entry condition is simply equates the value of vacancy to 0:

$$V = 0. \tag{A.7}$$

[1]More formally, if

$$W(x, \varepsilon) = (1 - \varepsilon)W^{u(c)=c}(x) + \varepsilon W^{u(c)=\ln c}(x),$$

where

$$W^{u(c)=c} = \frac{Y}{r - g}\left[\beta\pi - \frac{xB}{1 + x}\right]$$

is the equilibrium life satisfaction when individuals are risk neutral with $u(c) = c$ (see above), the variable ε reflects the degree of risk aversion, and we have

$$\frac{\partial^2 W}{\partial x \partial \varepsilon} < 0.$$

Then using $(A.6)$ and $(A.7)$ we find the entry rate as

$$x = \frac{\pi}{c} - r + g.$$

This equation is intuitive. The entry rate increases in flow profits and decreases in the cost of vacancy. Moreover, it increases in the equilibrium growth rate due to *capitalization* effect (it indicates that any formed business today will have higher future growth opportunities).

Recall that $r = \rho$ from the household maximization and $g = \frac{x}{1+x} \ln \lambda$. Hence equation $(A.7)$ is reexpressed as

$$x = \frac{\pi}{c} - \rho + \frac{x}{1+x} \ln \lambda.$$

To ensure the existence of a unique equilibrium, it is sufficient to have the following assumption.

Assumption: The discounted sum of future profits is greater than cost of posting vacancy $\frac{\pi}{\rho} > c$.

Then the entry rate is implicitly determined as

$$x = \Pi + \frac{x}{1+x} \ln \lambda$$

where $\Pi \equiv \frac{\pi}{c} - \rho$. Hence

$$x = \frac{-(1 - \Pi - \ln \lambda) + \sqrt{(1 - \Pi - \ln \lambda)^2 + 4\Pi}}{2}. \tag{A.8}$$

Proposition 4. *There exists a unique entry rate x. Moreover, the equilibrium entry rate is increasing in profits π and innovation size λ and decreasing in the cost of posting vacancy c and discount rate ρ*

$$\frac{\partial x}{\partial \pi}, \frac{\partial x}{\partial \lambda} > 0 \quad \textit{and} \quad \frac{\partial x}{\partial \rho}, \frac{\partial x}{\partial c} < 0.$$

This in turn has implications for the relationship between wellbeing and the determinants of creative destruction. In particular a lower entry cost will have the same effects on wellbeing as the effects of an increase in x identified above. An increase in λ will enhance both the growth effect for given x and the creative destruction effect (it will foster x).

A.1.8 Long-term Cost of Unemployment

In the baseline model, innovation has a long-lasting impact on income whereas the cost of innovation is transitory. Recent literature on routine vs non-routine jobs has shown that the IT revolution has replaced the workers executing routine jobs by computers and pushed them into unemployment [see David *et al.* (2003)]. Since these workers do not have human capital that can be carried to new jobs, they suffer from both longer unemployment spells and lower salaries. Clearly innovation, creative destruction, and the resulting unemployment can have long-lasting costs. For instance, job-specific human capital might be lost upon the job destruction and finding a new job might be even more difficult. Relatedly, Hamermesh (1987), Jacobson *et al.* (1993), Polsky (1999), Couch and Placzek (2010), Davis and von Wachter (2011), and von Wachter *et al.* (2011) show that older and more experienced workers have longer unemployment periods and lower wage replacement rates.

There are various ways of capturing longer-term costs of innovation. One tractable way of introducing this into our baseline model is to consider a lower efficiency in matching technology. To capture the increased difficulty in finding a new match, we can consider a matching technology

$$m(u_t, v_t) = \xi u_t^{\alpha} v_t^{1-\alpha} \tag{A.9}$$

where, everything else equal, a lower value of ξ would imply a longer term of unemployment. At the extreme, as $\xi \to 0$, the spell of unemployment goes to infinity. With this matching function, equilibrium unemployment rate becomes

$$u_t = \frac{x}{\xi + x}.$$

As expected, higher efficiency ξ lowers the equilibrium unemployment rate and the unemployment rate $\to 1$ when $\xi \to 0$. Accordingly, the workers' value functions become

$$rE - \dot{E} = \beta\pi Y + x(U - E) \tag{A.10}$$

$$rU - \dot{U} = bY + \xi(E - U). \tag{A.11}$$

And the wellbeing becomes

$$W = \frac{Y}{(\rho - g)} \left\{ \beta\pi - \frac{x}{\xi + x} (\beta\pi - b) \right\}$$

we can capture the longer term cost of unemployment by a lower efficiency in the matching technology.

$$\frac{\partial^2 W}{\partial x \partial \xi} = \frac{Y(\beta\pi - b)}{(\rho - g)} \frac{\xi - x}{(\xi + x)^3} > 0 \text{ when } 0 < x < \xi.$$

This implies that, controlling for growth rate, the effect of creative destruction on wellbeing is always negative. However it is less so when the cost of unemployment is short term. As the cost of unemployment has longer impact (as ξ declines), the negative impact of innovation on wellbeing through unemployment is amplified.

A.1.9 Taxing Labor to Finance Unemployment Benefits

In this section, we consider a framework where unemployment benefits are financed by labor and profit taxes. This implies that while unemployed workers gain from unemployment benefits, the employed workers will be hurt by taxes. Let us denote the labor tax by τ_w and profit tax by τ_π. In this case government period-by-period balanced budget implies

$$(1 - u_t)(\tau_w w_t + \tau_\pi \pi_t) = b_t u_t$$

where the left-hand side denotes the tax revenue from labor income tax and corporate profit tax and the right-hand side denotes the total amount of distributed unemployment benefit. Imposing $w_t = \beta\pi Y_t$, $\pi_t = \pi Y_t$ and $b_t = bY_t$ we get

$$(1 - u_t)(\tau_w \beta\pi + \tau_\pi \pi) = bu_t.$$

Let us denote the fraction of unemployment benefit financed by $\kappa \in [0, 1]$. Then we can express the portion of labor tax payment as

$$\kappa b u_t = (1 - u_t) \tau_w \beta\pi$$

and the portion of profit tax as

$$(1-\kappa)\, bu_t = (1-u_t)\, \tau_\pi \pi.$$

Since the equilibrium unemployment rate is

$$u_t = \frac{x}{1+x}$$

we can express the required labor tax rate as

$$\tau_w = \frac{\kappa b x}{\beta \pi}.$$

Solving the value functions for the employed and unemployed worker we find

$$rE - \dot{E} = (1-\tau_w)\, \beta \pi Y + x(U-E), \text{ and} \tag{A.12}$$

$$rU - \dot{U} = bY + (m(u,v)/u)(E-U). \tag{A.13}$$

In this case, the wellbeing is simply

$$W_t = \frac{Y_t}{(\rho - g)} \left\{ \beta\pi - \frac{x}{1+x} \left[\beta\pi - (1-\kappa)\, b \right] \right\}.$$

$$\frac{\partial W_t}{\partial x} = -\frac{\left[\beta\pi - (1-\kappa)\, b\right] Y_t}{(1+x)^2 (\rho - g)} < 0, \text{ and } \frac{\partial^2 W_t}{\partial x \partial b} = \frac{(1-\kappa)\, Y_t}{(1+x)^2 (\rho - g)} > 0.$$

Note that, when controlling for the growth rate, wellbeing is negatively affected by turnover, but less so when the unemployment benefit is higher. Note that, as long as the tax burden of the unemployment benefit is shared by both workers and firm owners, i.e., $\kappa < 1$, the negative impact of the creative destruction is mitigated by the unemployment benefit.

A.1.10 Sufficient Condition for Exit in Section 1.2.1. .

Denote the value of an incumbent before entry by V_1 and after entry V_2. Then we can express these value functions as

$$rV_1 - \dot{V}_1 = \pi Y + x\,(V_2 - V_1)\,, \text{ and } rV_2 - \dot{V}_2 = \pi Y + \frac{m}{v}\,(0 - V_2)\,.$$

Since in equilibrium $m = v$, we get

$$V_2 = \frac{\pi Y}{1 + r - g}. \tag{A.14}$$

Then we can express V_1 as

$$V_1 = \frac{(1 - \beta)\pi Y + x V_2}{x + r - g} \tag{A.15}$$

Note that $(A.14)$ implies $\pi Y = (1 + r - g)\, V_2$. Substitute this into $(A.15)$:

$$V_1 = V_2 + \frac{V_2}{x + r - g} > V_2.$$

Hence any outside option O such that $V_1 > O > V_2$:

$$\frac{\pi Y}{1 + r - g}\left(1 + \frac{1}{x + r + g}\right) > O > \frac{\pi Y}{1 + r - g}$$

implies the incumbent firm will exit as soon as there is a new entrant. This is what we assume throughout our analysis.

A.2 Empirical Appendix

A.2.1 Testing Prediction 4

In this section, we test Prediction 4: *A higher turnover rate increases future wellbeing more for more forward-looking individuals.*

The existing literature argued that old individuals, educated individuals, and rich individuals tend to be more patient [Gilman (1976), Black (1983), Lawrance (1991), Warner and Pleeter (2001)]. Therefore, we use age, education, and income to proxy for individuals with different patience levels. The interesting finding is that all these different proxies deliver similar results. Note that these variables, as Table A.1 shows, are not that highly correlated.

The highest correlation is between education and log income: 0.4. This indicates that each of them is potentially carrying a different information about the discount rate.[2] Follow-

[2]One might think of smoking as a good discount rate proxy. But the literature has shown that smoking is

Table A.1: *Correlation matrix*

	Log (income)	Age	Education
Log (income)	1.00		
Age	0.140	1.00	
Education	0.414	0.109	1.00

ing the literature, our test for Prediction 4 will thus be to interact our creative destruction variables with these three proxies: age, education, income. Since these are individual-level characteristics, we perform the regressions at the individual level. The main coefficient of interest is that of the interaction term between the job creation rate and the discount rate proxies. All regressions include individual controls as well as the job destruction rates and the interaction term of job destruction with the discount rate proxy but we only report the job creation rate main effect and interaction to save space.

Following the theoretical predictions, we use the future ladder as dependent variable. Recall that when creative destruction increases from x_1 to x_2 (where $x_2 > x_1$), the economy starts transitioning from a low steady state to a higher steady state in terms of its wellbeing. Given that the economy will be closer to the new steady state in any future year than the current period, the positive impact of creative destruction will reflect itself more in future ladder than the current ladder. Hence, empirically we test this Prediction 4 using the future ladder in columns 1 and 2. We also take the difference between the reported values of future ladder and current ladder in columns 3 and 4 in order to get closer to the differential effect of creative destruction on future wellbeing.

Individual-level Results The regression results are reported on Table A.11.

The discount rate proxy is age in Panel A, number of years of schooling in Panel B, and

strongly associated with risk-taking behavior [Anderson and Mellor (2008), Pfeifer (2012)]. Since discounting and risk-aversion are different forces in our model and have different implications, we have been hesitant to use smoking as a proxy for differential discounting.

log of income in Panel C. These variables are demeaned such that the coefficient for the job creation rate is the effect of job creation for individuals either of average age in Panel A (40 years old), or of average education in Panel B (14 years of schooling), or of average log of income in Panel C. Now looking at the interaction terms in all columns, we see that the effect of the job creation rate is significantly more positive for older individuals, more educated individuals, and richer individuals. Columns 2 and 4 are similar to columns 1 and 3 except that additional MSA-level controls are added. This provides evidence in favor of Prediction 4. In terms of magnitude, if we look at column 2, a one standard deviation increase in age increases the effect of the job creation rate on the future ladder by 35% ($= 11.91 \times 0.028/0.949$), a one standard deviation increase in education increases it by almost 50% ($= 2.346 \times 0.181/0.919$), and a one standard deviation increase in log of income increases it by 66% ($= 0.984 \times 0.655/0.967$). Thus these interaction effects are not only statistically significant but also economically so.

A.2.2 Additional Tables

Table A.2: *Robustness to an alternative measure of life satisfaction - Prediction 1*

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Life satisfaction (BRFSS)					
	MSA-level			Individual level		
Job turnover	0.258	0.377	0.443	0.126	0.171	0.241
	(0.0785)	(0.0787)	(0.0859)	(0.0520)	(0.0488)	(0.0474)
Unemployment rate		-1.389	-1.365		-0.898	-0.993
		(0.252)	(0.255)		(0.165)	(0.155)
MSA log of income	0.137	0.0710	0.0839	-0.0401	-0.0717	-0.0857
	(0.0145)	(0.0146)	(0.0152)	(0.0172)	(0.0202)	(0.0120)
Additional MSA controls			x			x
Individual controls				x	x	x
Year and Month F.E.				x	x	x
Observations	364	364	343	780,169	780,169	738,770
R-squared	0.186	0.347	0.385	0.103	0.103	0.104
p-value Turnover [1]= Turnover [2]		0.004				
p-value Turnover [4]= Turnover [5]					0.005	

Note: The first three columns are similar to that of Table 1.2 Panel A except that the life satisfaction measure comes from BRFSS, for which sample years are 2005-2010. The last three columns are similar to the first three columns of Table 1.2 Panel B except that the life satisfaction measure comes from BRFSS.

Table A.3: *Robustness to an alternative measure of life satisfaction - Prediction 2*

	(1)	(2)	(3)	(4)
VARIABLES	Life satisfaction (BRFSS)			
	MSA-level		Individual level	
Job creation	1.546	1.657	0.342	0.410
	(0.221)	(0.228)	(0.0837)	(0.0925)
Job destruction	-1.236	-1.217	-0.115	-0.0489
	(0.237)	(0.237)	(0.0792)	(0.0804)
MSA log of income	0.124	0.133	-0.0421	-0.0533
	(0.0133)	(0.0142)	(0.0166)	(0.0125)
Additional MSA controls		x		x
Individual controls (incl. income)			x	x
Year and Month F.E.			x	x
Observations	364	343	780,169	738,770
R-squared	0.275	0.323	0.103	0.104

Note: The first two columns are similar to that of Table 1.3 Panel A except that the life satisfaction measure comes from BRFSS, for which sample years are 2005-2010. The last two columns are similar to the first two columns of Table 1.3 Panel B except that the life satisfaction measure comes from BRFSS.

Table A.4: *Robustness to different subperiod restrictions - Prediction1*

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Life satisfaction (BRFSS, MSA-level)					
	2005-2007			2008-2010		
Job turnover	0.231	0.257	0.349	0.158	0.332	0.390
	(0.0747)	(0.0681)	(0.0767)	(0.112)	(0.116)	(0.129)
Unemployment rate		-1.340	-1.359		-1.043	-1.022
		(0.282)	(0.297)		(0.213)	(0.218)
MSA log of income	0.148	0.103	0.116	0.143	0.0808	0.0870
	(0.0182)	(0.0192)	(0.0204)	(0.0169)	(0.0180)	(0.0199)
Additional MSA controls			x			x
Observations	364	364	326	364	364	336
R-squared	0.146	0.217	0.254	0.160	0.262	0.279

Note: Everything in the first three columns is similar to the first three columns of Table A.2, except that the variables are averaged across 2005-2007 instead of 2005-2010. The last three columns are similar to the first three except that the variables are averaged across 2008-2010.

Table A.5: *Robustness to different subperiod restrictions - Prediction 2*

	(1)	(2)	(3)	(4)
VARIABLES	Life satisfaction (BRFSS, MSA-level)			
	2005-2007		2008-2010	
Job creation	1.014	1.127	0.630	0.659
	(0.154)	(0.162)	(0.259)	(0.284)
Job destruction	-0.905	-0.842	-0.225	-0.135
	(0.206)	(0.222)	(0.227)	(0.243)
Log income	0.143	0.156	0.136	0.140
	(0.0170)	(0.0181)	(0.0170)	(0.0196)
Additional MSA controls		x		x
Observations	364	326	364	336
R-squared	0.210	0.247	0.173	0.190

Note: Everything in the first two columns is similar to the first two columns of Table A.3, except that the variables are averaged across 2005-2007 instead of 2005-2010. The last two columns are similar to the first two except that the variables are averaged across 2008-2010.

Table A.6: *Robustness to an alternative measure of creative destruction - Prediction 1*

VARIABLES	(1)	(2) Current ladder	(3)	(4) Future ladder
Job turnover rate	0.820	0.981	1.028	0.551
	(0.150)	(0.167)	(0.172)	(0.169)
Unemployment rate		-2.438	-2.448	-0.358
		(0.549)	(0.598)	(0.455)
Log of income	0.426	0.315	0.305	0.363
	(0.0936)	(0.0947)	(0.114)	(0.0884)
Additional MSA controls			x	x
Observations	358	358	344	344
R-squared	0.170	0.259	0.292	0.410
p-value Turnover [1]= Turnover [2]		0.02		

Note: Everything is similar to Table 1.2 panel A except that the job turnover rates come from the Longitudinal Employer Household Dynamics.

Table A.7: *Robustness to an alternative measure of creative destruction - Prediction 2*

VARIABLES	(1) Current ladder	(2)	(3) Future ladder	(4)
Job creation rate	3.906	3.647	3.934	2.508
	(0.689)	(0.722)	(0.739)	(0.609)
Job destruction rate	-1.741	-1.425	-2.299	-1.115
	(0.598)	(0.640)	(0.632)	(0.546)
Log of income	0.465	0.461	0.353	0.405
	(0.0888)	(0.106)	(0.0679)	(0.0788)
Additional MSA controls		x		x
Observations	358	344	358	344
R-squared	0.213	0.238	0.129	0.425

Note: Everything is similar to Table 1.3 panel A except that the job creation and destruction rates come from the Longitudinal Employer Household Dynamics.

Table A.8: *Allowing for a non-linear effect of unemployment*

	(1)	(2)	(3)	(4)
VARIABLES		Current ladder		Future ladder
Job turnover rate	0.599	1.251	1.245	1.685
	(0.361)	(0.382)	(0.385)	(0.294)
Log of income	0.342	0.158	0.181	0.274
	(0.0839)	(0.0862)	(0.106)	(0.0800)
Cubic polynomial of unemployment rate		x	x	x
Additional MSA controls			x	x
Observations	363	363	344	344
R-squared	0.100	0.303	0.348	0.495
p-value Turnover [1]= Turnover [2]		0.00		

Note: Everything is similar to Table 1.2 panel A, except that the unemployment rate is introduced in the regressions along with its square and its cube.

Table A.9: *Bartik (predicted) measure of creative destruction - Prediction 1*

VARIABLES	(1)	(2) Current ladder	(3)	(4) Future ladder
Job turnover	0.550	1.185	1.064	0.776
	(0.193)	(0.205)	(0.529)	(0.514)
Unemployment rate		-3.129	-2.178	-1.515
		(0.364)	(1.102)	(1.080)
MSA log of income	0.402	0.332	0.414	0.195
	(0.0303)	(0.0310)	(0.0376)	(0.0393)
MSA F.E.			x	x
Additional MSA controls	x	x	x	x
Year and quarter F.E	x	x	x	x
Observations	4,828	4,828	4,828	4,828
R-squared	0.179	0.200	0.325	0.257
p-value Turnover [1]= Turnover [2]		0.00		

Note: Everything is similar to Table 1.2 panel C, except that the direct measure of the job turnover rate is replaced by a predicted (Bartik-line) one. For more details see the end of Section 1.4.2 of the main text.

Table A.10: *Bartik (predicted) measure of creative destruction - Prediction 2*

VARIABLES	(1) Current ladder	(2)	(3) Future ladder	(4)
Job creation rate	11.40	10.36	5.722	5.140
	(1.153)	(1.126)	(1.070)	(1.089)
Job destruction rate	-10.89	-10.40	-4.438	-4.595
	(1.220)	(1.315)	(1.150)	(1.302)
Log of income	0.390	0.403	0.257	0.191
	(0.0300)	(0.0375)	(0.0304)	(0.0392)
MSA F.E.		x		x
Additional MSA controls	x	x	x	x
Year and quarter F.E	x	x	x	x
Observations	4,828	4,828	4,828	4,828
R-squared	0.200	0.342	0.146	0.261

Note: Everything is similar to Table 1.3 panel C, except that the direct measures of the job creation and destruction rates are replaced by predicted (Bartik-line) ones. For more details see the end of Section 1.4.2 of the main text.

Table A.11: *Test of prediction 4*

VARIABLES	(1) Future ladder	(2)	(3) Ladder difference	(4)
	Panel A: Interaction with Age			
Job creation rate	1.071	0.949	-0.005	-0.343
	(0.207)	(0.221)	(0.347)	(0.369)
Job creation × Age	0.024	0.028	0.023	0.027
	(0.014)	(0.016)	(0.015)	(0.015)
Age	-0.0328	-0.0350	0.0621	0.0592
	(0.003)	(0.003)	(0.004)	(0.004)
Additional MSA controls		x		x
Individual controls	x	x	x	x
Year and Month F.E.	x	x	x	x
Observations	544,228	450,908	543,817	450,554
R-squared	0.094	0.095	0.071	0.071
	Panel B: Interaction with Education			
Job creation rate	1.033	0.919	-0.0343	-0.346
	(0.209)	(0.222)	(0.355)	(0.376)
Job creation × Education	0.137	0.181	0.242	0.290
	(0.072)	(0.080)	(0.104)	(0.110)
Education	0.0595	0.0535	-0.0201	-0.0263
	(0.013)	(0.015)	(0.019)	(0.021)
Additional MSA controls		x		x
Individual controls	x	x	x	x
Year and Month F.E.	x	x	x	x
Observations	544,228	450,908	543,817	450,554
R-squared	0.093	0.094	0.068	0.068
	Panel C: Interaction with Income			
Job creation rate	1.094	0.967	-0.0189	-0.357
	(0.208)	(0.222)	(0.351)	(0.372)
Job creation × Log income	0.557	0.655	0.836	1.031
	(0.177)	(0.192)	(0.264)	(0.263)
Log income	0.224	0.200	-0.212	-0.245
	(0.030)	(0.033)	(0.041)	(0.045)
Additional MSA controls		x		x
Individual controls	x	x	x	x
Year and Month F.E.	x	x	x	x
Observations	544,228	450,908	543,817	450,554
R-squared	0.093	0.094	0.070	0.070

Note: Everything is similar to Table 1.3 panel B except that the job creation and destruction rates are interacted with some proxies for the individual's discount rate: age in panel A, number of years of schooling in panel B, and log of income in panel C. All these proxies are demeaned. We don't report the interaction coefficient and the main effect for job destruction as the interaction of interest is the job creation one (see main text).

Appendix B

Appendix to Chapter 2

B.1 Consequences of rounding

In this Appendix, we discuss the consequences of rounding in the reservation wages on our identification. First, we explore potential biases taking an analytical perspective. Second, we quantify its importance using Monte-Carlo simulations.

We are not in the standard case of classical measurement error in a covariate. We consider measurement error on the reservation wage, which is the main outcome (left-hand side variable), and rounding is a non-classical measurement error, as the difference between the true variable and its proxy is not independent of the true running variable.

First, we clarify the potential bias from an analytical perspective. We denote Y^* the true unobserved outcome. We assume that we only observe Y, obtained after rounding to the nearest hundreds. We have $Y = v + Y^*$, where $v = Y - Y^*$ is the measurement error. We assume that the true data generating process is such that: $Y^* = \beta X + u$ where Y^* and X are centered and u is an error term uncorrelated to X. Because of the measurement error, we can only estimate the following model: $Y = \tilde{\beta} X + w$. After some algebra, the OLS estimate of β is $\tilde{\beta} = \beta + \frac{cov(v,X)}{var(X)}$. The measurement error will induce a bias if and only if the rounding error v is correlated with X (i.e. PBD in our case), which has a priori no reason to be true.

Second, we run some simulations to check what happens in practice with plausible data generating processes. We draw the PBD in $1 + 7U^{1/2}$, where U is a uniform between 0 and 1, which generates a Pareto between 8 and 24, and the wage is a log-normal with the first and last deciles equal to 800 and 3700. We set the elasticity of the wage to PBD to 0.2. Then, we build a version of the wage rounded to the hundreds. For the Monte-Carlo simulation, we generate 1,000 samples of size 10,000. For each sample, we estimate the elasticity of the wage wrt PBD and the elasticity of the rounded wage wrt PBD. Figure B.1 shows the distributions of the elasticity estimates without and with rounding: both distributions look extremely similar. In Figure B.2, we compute for each simulation the difference between elasticity estimates with and without rounding – the bias – and we plot the distribution of biases. The bias is very close to zero (magnitude 100 times lower than the original elasticity). The measurement error induced by rounding the outcome is likely not to be a first-order issue in our case.

Figure B.1: *Monte-Carlo simulations: distribution of elasticity estimates*

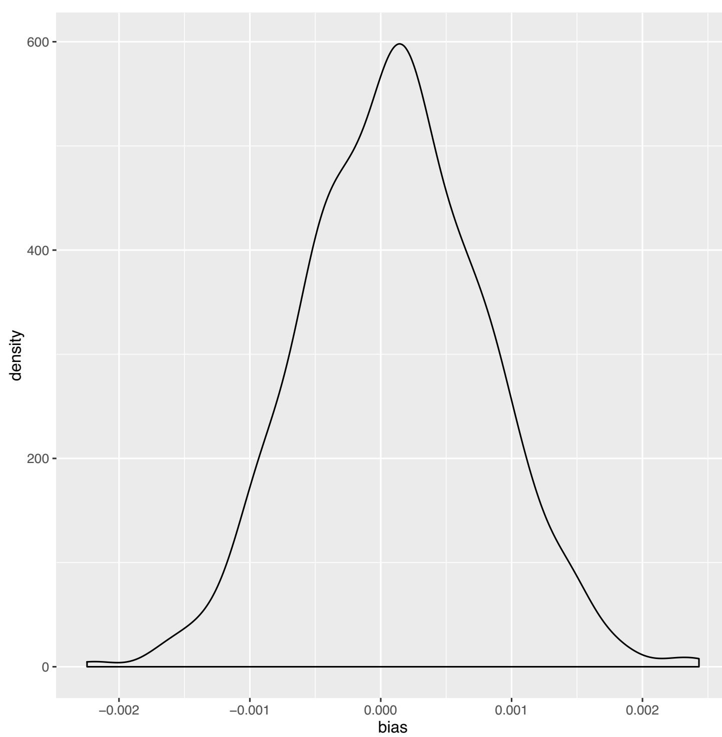
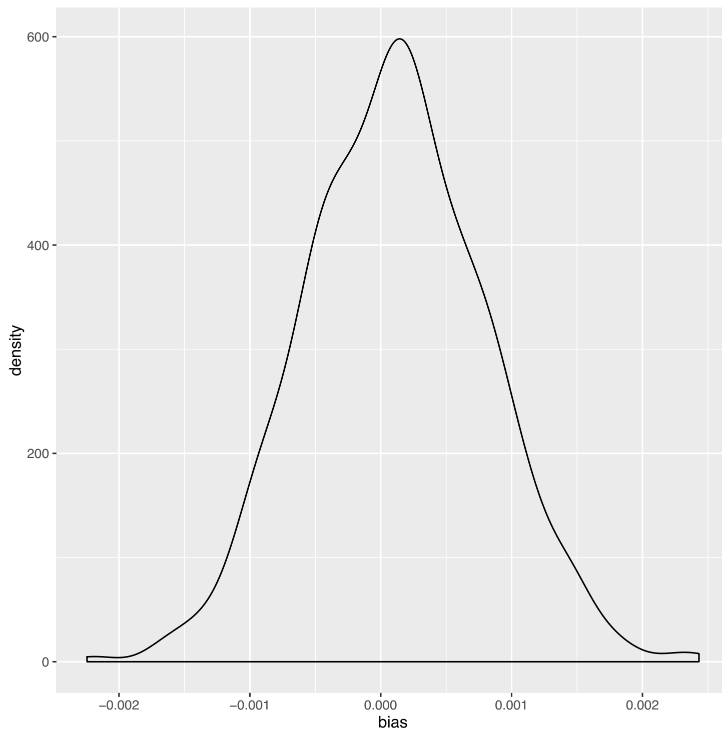
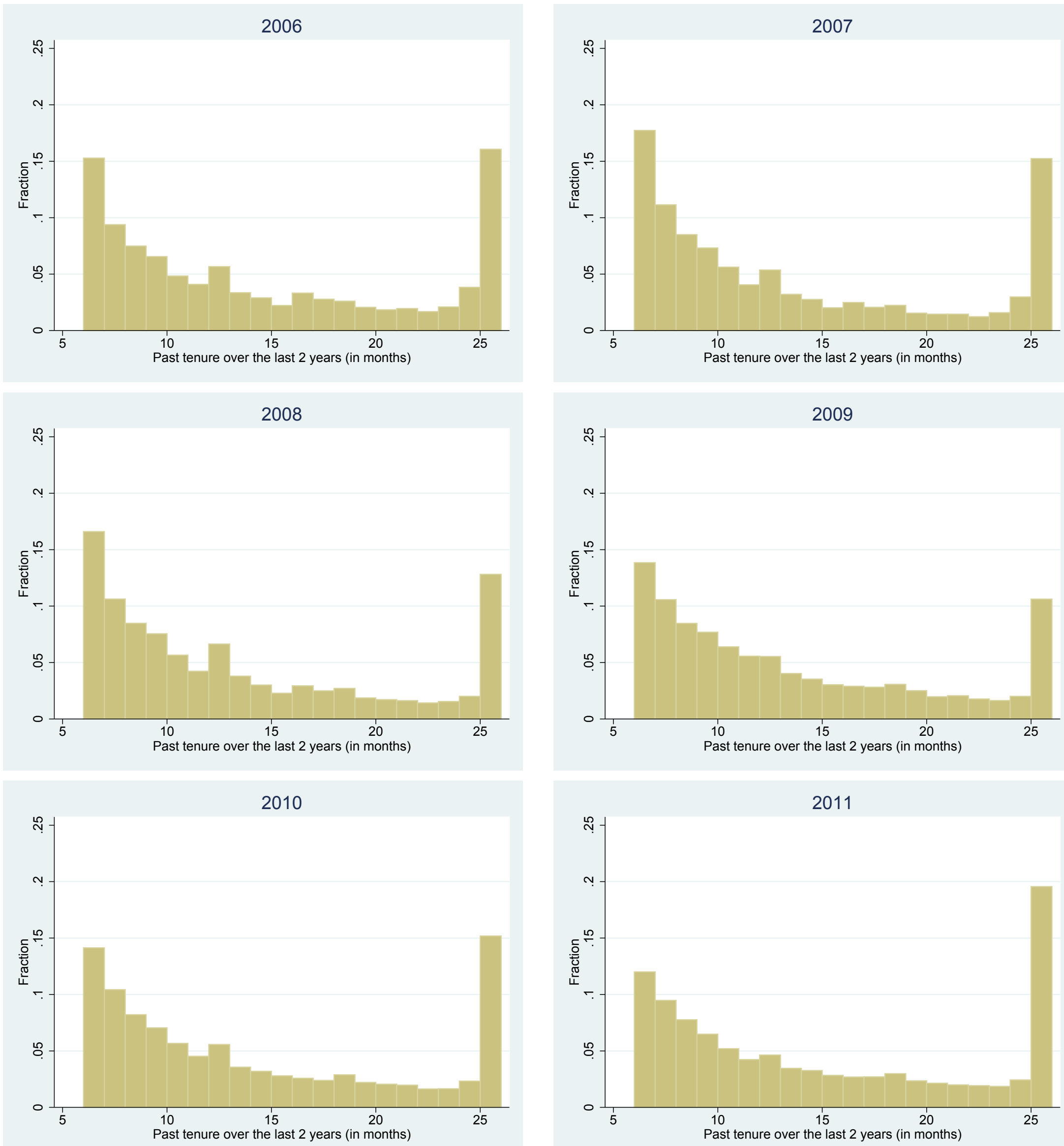


Figure B.2: *Monte-Carlo simulations: distribution of bias*



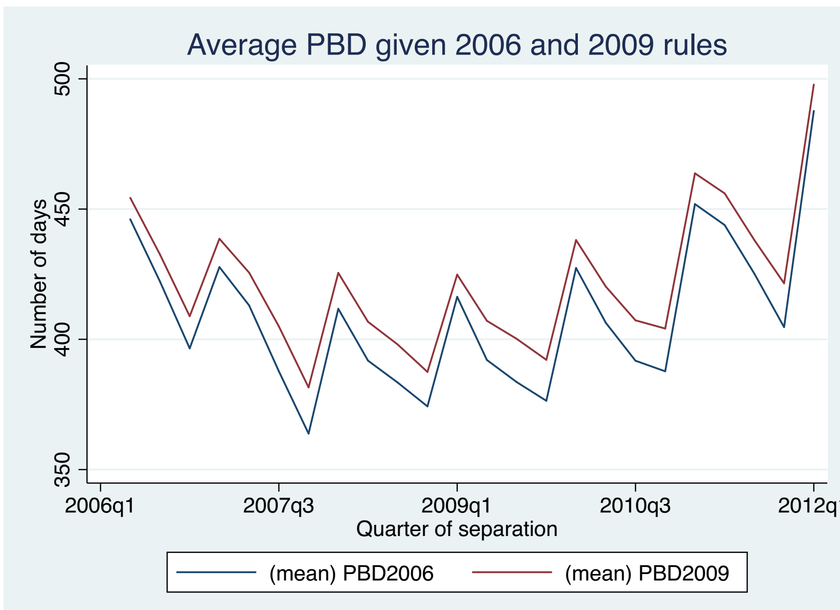
B.2 Additional Figures

Figure B.3: *Distribution of the tenure variables*



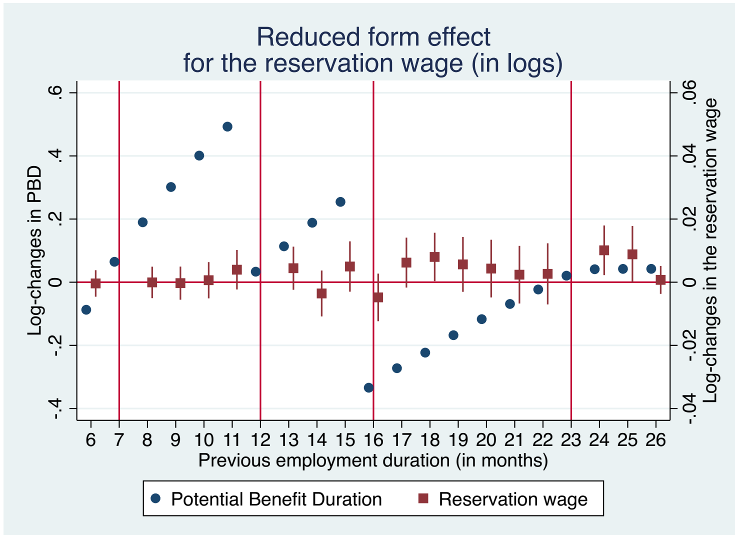
Source: FNA-FH (Pole emploi). Note: Each panel plots the distribution of the past tenure for the inflow of claimants who loose their job between the 1st of April of year N and the 31st of March of Year N+1. Thus the 3 first distributions are pre reform and the 3 last are post reform.

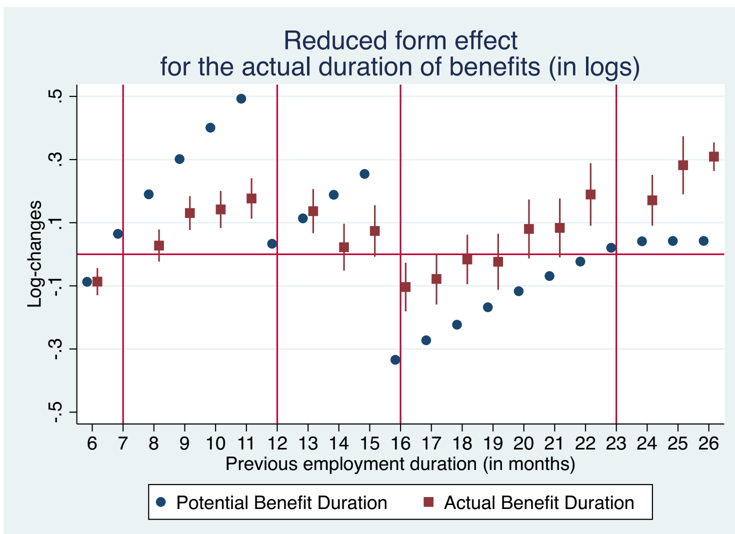
Figure B.4: *Average potential benefit duration given 2006 and 2009 rules*



Source: FNA-FH (Pole emploi). Note: for each claimant, we compute, from her tenure, her PBD according to the 2006 rules and the 2009 rules. We then average each PBD over quarter. This translates the actual tenure distribution into potential treatment intensity. The Figure shows that there are no changes in the tenure distribution that would imply a change in treatment intensity on top of the reform shock.

Figure B.5: *Reduced form effect of the reform on the reservation wage and on the actual duration of benefits*





Source: FNA-FH (Pole emploi). Note: Each panel plots the change in PBD for every tenure group (circles in blue) and the reduced-form effects (squares in red) on either the reservation wage (upper panel) and the actual benefit duration (lower panel). The reduced-form effects correspond to the β_j coefficients, where j indicates the group with j months of tenure, from the following regression.

$$\log Y_{i,n} = \sum_{j=6,excl.7,12,23}^{26} \beta_j D(Tenure_{i,n} = j) \times After_{i,n} + \sum_{j=6,excl.7,12,23}^{26} \delta_j D(Tenure_{i,n} = j)$$
$$+ \gamma X_{i,n} + Year \times Quarter F.E. + Indiv.F.E._i + \nu_{i,n}$$

$Y_{i,n}$ is either the reservation wage or the number of days of benefit receipt for the n-th claim of individual i. $X_{i,n}$ controls for the job seeker's observable characteristics. These include gender, age and experience, age square and experience square, number of years of schooling, marital status and number of children, a dummy for being foreign born, and dummies for 20 bins of the previous wage. We also include dummy variables for the year $\times$ quarter at which the previous job ended.

B.3 Additional Tables

Table B.1: *Summary statistics*

Variable	Mean	Std. Dev.	N
Male	0.599	0.49	180670
Foreign born	0.111	0.314	180670
Age	31.301	7.873	180670
Married	0.353	0.478	180670
Divorced	0.068	0.252	180670
Has a child	0.363	0.481	180670
Education (in years)	11.59	3.272	180637
Occupational Experience (in years)	4.628	5.149	180670
Past Contract is long-term	0.353	0.478	166486
Sum of past tenures over the last 2 years (in days)	427.708	218.351	180670
Past tenure at last employer (in days)	393.648	573.158	180670
Potential Benefit Duration (in days)	413.156	208.855	180670
Actual Benefit Duration (in days)	192.403	163.184	180670
Past Monthly Wage (gross, in euros)	1721.631	388.383	180670
Unemployment Benefits (in euros)	1006.869	226.521	180670

Source: FNA-FH (Pole emploi).

Table B.2: *Placebo elasticities - Difference-in-difference strategy*

	(1)	(2)	(3)	(4)
	2007	2008	2010	2011
VARIABLES	Log of reservation wage			
Log PBD	0.00979	0.00709	0.00755	0.00512
	(0.00655)	(0.00654)	(0.00582)	(0.00566)
Obs.	30,603	30,603	36,422	36,422
Indiv. F.E.	yes	yes	yes	yes

Source: FNA-FH (Pole emploi).

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the placebo tests. To avoid contamination, we split our main sample into two subsamples: before and after the 2009 reform. We use the before subsample to test for potential effects of placebo reforms at the end of March 2007 (Column 1) and of March 2008 (Column 2). We use the after subsample in Columns (3) and (4). In each column, we estimate the fixed effects model (2) where we replace actual PBD by a placebo PBD and we instrument the placebo PBD by tenure group dummies interacted with a dummy indicating whether the separation date is after the placebo reform date. For example, in Column (1), the placebo PBD is equal to the PBD according to the 2006 rules for claimants who register before the 31st of March 2007 and it is equal to the PBD according to the 2009 rules for claimants who register after the 1st of April 2007.

Table B.3: *Estimates of discontinuities in covariates*

VARIABLES	(1) Log of Past wage	(2) Male	(3) Education	(4) Number of children	(5) Foreign born	(6) Married	(7) Open-ended contract	(8) Experience	(9) Replacement rate
A. All observations included around the cutoff									
$1(age \geq 50)$	0.0156***	-0.00903	0.258***	-0.0309*	-0.0280***	0.00430	0.0333***	0.212	-0.000770
	(0.00355)	(0.00666)	(0.0574)	(0.0167)	(0.00565)	(0.00611)	(0.00797)	(0.175)	(0.000922)
Obs.	481,871	481,871	481,850	481,871	481,871	481,871	439,678	481,871	481,871
B. Excluding observations with age in $[49.9, 50.1]$									
$1(age \geq 50)$	0.00957**	-0.00849	0.228***	-0.00680	-0.00731	-9.27e-06	0.00597	-0.339**	-0.000493
	(0.00452)	(0.00859)	(0.0687)	(0.0173)	(0.00490)	(0.00673)	(0.00744)	(0.168)	(0.00112)
Obs.	472,209	472,209	472,188	472,209	472,209	472,209	430,855	472,209	472,209
C. Excluding observations with age in $[49.75, 50.25]$									
$1(age \geq 50)$	0.00604	-0.00445	0.185**	-0.0109	-0.00151	-0.00523	-0.00120	-0.301*	-0.00123
	(0.00544)	(0.00749)	(0.0767)	(0.0229)	(0.00606)	(0.00769)	(0.00764)	(0.176)	(0.00132)
Obs.	458,337	458,337	458,316	458,337	458,337	458,337	418,204	458,337	458,337
D. Excluding observations with age in $[49.5, 50.5]$									
$1(age \geq 50)$	-0.00270	0.00539	-0.0833	-0.00718	0.00414	0.00409	-0.00875	0.00497	-0.00110
	(0.00327)	(0.00691)	(0.0537)	(0.0174)	(0.00457)	(0.00689)	(0.00688)	(0.144)	(0.00109)
Obs.	434,387	434,387	434,367	434,387	434,387	434,387	396,322	434,387	434,387

Source: FNA-FH (Pole emploi).
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the estimates of a discontinuity in the distribution of covariates around the 50-year-old age cut-off, i.e coefficient δ of the following equation:

$$X_i = \alpha + \delta 1(age_i \geq 50) + P_0(age_i - 50) \times 1(age_i < 50) + P_1(age_i - 50) \times 1(age_i \geq 50) + \epsilon_i$$

where notations have already been defined in the main text. We follow the estimation strategy of Calonico *et al.* (2014). The kernel used for the local polynomial estimation is triangular.

Table B.4: *RDD estimates with different age exclusions*

Age excluded	(1) $[49.9, 50.1]$	(2) $[49.75, 50.25]$	(3) $[49.5, 50.5]$
	Log of Reservation Wage		
log PBD	-0.0141	0.00482	0.0119
	(0.0138)	(0.0128)	(0.0103)
	Log of Benefit duration		
log PBD	0.199**	0.215***	0.161**
	(0.0852)	(0.0797)	(0.0696)
Obs.	470,082	456,280	432,431

Source: FNA-FH (Pole emploi).

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. RDD estimates obtained with the corrections of Calonico *et al.* (2014). The kernel used for the local polynomial estimation is triangular. We control for all covariates listed in Table B.3: past wage (log), gender, education (in years), number of children, foreign status, marriage status, type of past labor contract (open-ended), experience in the occupation sought and replacement rate.

Table B.5: *Estimates of discontinuities in reservation wages at placebo age cutoff*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Placebo Age cutoff	44	45	46	47	48	49	51	52	53	54
$1(age \geq placebo)$	-0.000210	0.00464	-0.00159	0.00194	0.00149	-0.000106	-0.000254	0.0123**	-0.00552	0.0147**
	(0.00302)	(0.00353)	(0.00315)	(0.00327)	(0.00329)	(0.00365)	(0.00396)	(0.00591)	(0.00417)	(0.00692)
Obs.	590,095	564,914	543,235	521,034	499,192	478,334	441,441	427,481	412,624	392,336

Source: FNA-FH (Pole emploi).
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the estimates of a discontinuity in reservation wage at placebo age cutoffs, i.e coefficient δ of the following equation:

$$\log resW_i = \alpha + \delta 1(age_i \geq placebo) + P_0(age_i - placebo) \times 1(age_i < placebo) + P_1(age_i - placebo) \times 1(age_i \geq placebo) + \gamma X_i + \epsilon_i$$

where notations have already been defined in the main text. We follow the estimation strategy of Calonico *et al.* (2014). The kernel used for the local polynomial estimation is triangular. We control for all covariates listed in table B.3: past wage (log), gender, education (in years), number of children, foreign status, marriage status, type of past labor contract (open-ended), experience in the occupation sought and replacement rate. Consistently with our preferred specification in the main text, we exclude individuals with an age in a centered 6-month window around the placebo age.

Appendix C

Appendix to Chapter 3

Table C.1: *Dataset and variable definitions*

Title	Unit of observation Time frame	Variables used
Employer-employee registers Workplace level (*IDAS*)	Workplace * Year 1990-2012	# of employees Workplace status in the following year
Employer-employee registers Individual level (*IDAS*)	Individual * Job * Year 1990-2012	Primary job, tenure, occupation # of years of employment/ UI fund membership
Tax data	Individual * Year 1996-2012	Wage earnings, post-tax post transfer income Unemployment income
Unemployment insurance	Weekly claims 1996-2007	# of weeks of unemployment
Death records	Death event 1996-2013	Causes of death: Cardiovascular issue: ICD-10 code I00-I99 Cancer: ICD-10 code C00-D48 External cause: V01-Y89 Alcohol-related: F10, K29, K70-K74, K85-K86, X45, X65 Suicide: ICD-10 X60-84; Y87
Prescription drugs (*Laegemiddelstatistikregisteret*)	Individual * Type of drugs * Day of purchase 1996-2013	Annual probability to purchase or # of days/year under treatment for i) Antidepressants, sleeping pills and anti- anxiety medication: ATC code N06A, N05B, N05C ii) Opioid painkillers:ATC code N02A iii) Diabetes-related drugs: A10
Doctor visits (*Syegesikringsregistret*)	Individual * Type of visit * Week of visit 1996-2013	Annual probability to visit a psychiatrist a psychologist, a physiotherapist etc Annual # of visits to General Practioner
Hospital admissions (*LandsPatientregister*)	Individual * Diagnosis * Entry date 1996-2013	Annual probability to receive inpatient care Annual probability to visit the emergency room Annual probability to be diagnosed with i) disease of the circulatory system; ii) cancer

Table C.2: *5 years mortality rate (Deaths/100) of unemployed v. non-unemployed*

	(1)	(2)	(3)	(4)	(5)
Sample	No restriction		Never unemployed prior to year t	Males	Females
Mortality rate	5 years	10 years	5 years	5 years	5 years
UI recipient	0.310***	0.713***	0.544***	0.435***	0.177***
in year t	(0.0518)	(0.0809)	(0.135)	(0.0810)	(0.0614)
Control Mean	0.95	2.46	0.98	1.24	0.64
Observations	187,636	187,636	30,833	88,566	99,070
R-squared	0.019	0.042	0.031	0.017	0.024

Note: The coefficients shown are those of a dummy for being a UI recipient in year t (where t=2002) in a linear probability model. Dependent variables are the probability to die between year t+1 and year t+5 (or t+10 for column 2). The sample consists of all UI recipients in 2002 with no UI in the year prior and of a control group matched on observables (see main text for more details). Additional controls include an immigrant dummy, very detailed education (over 1,000 categories), time varying detailed marital status, number of years of employment in year -1 (as measured by number of years of social security contribution) and number of years of UI fund membership in year -1). Levels of significance: *** 1%, ** 5%, * 10%.

Table C.3: *5 years mortality rate (Deaths/100) of unemployed v. non-unemployed - By cause of death*

	(1) Cancer	(2) Circulatory disease	(3) External cause	(4) Alcohol related	(5) Suicide
UI recipient	0.036	0.086***	0.067***	0.091***	0.049***
in year t	(0.0349)	(0.0224)	(0.0181)	(0.0164)	(0.0130)
Control Mean	0.48	0.16	0.10	0.06	0.05
Observations	187,636	187,636	187,636	187,636	187,636
R-squared	0.014	0.009	0.004	0.008	0.004

Note: Same sample and specification as for Table C.2, except that the dependent variable is the probability to die between year t+1 and year t+5 of a specific cause of death, which varies by column.