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**Conference Paper**

# On or Off – Are Treatment Effects of Policy Changes Symmetric? Evidence from Unemployment Insurance Reform with Incomplete Information

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel - Session: Evaluation of Labor Market Policies, No. A20-V2

**Provided in Cooperation with:**
Verein für Socialpolitik / German Economic Association

*Suggested Citation:* Arni, Patrick; Liu, Xingfei (2016) : On or Off – Are Treatment Effects of Policy Changes Symmetric? Evidence from Unemployment Insurance Reform with Incomplete Information, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel - Session: Evaluation of Labor Market Policies, No. A20-V2, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

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# On or Off – Are Effects of Policy Changes Symmetric? Evidence from UI Reform with Incomplete Information ©

January 2016

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*Abstract:*

Does introducing or abolishing a policy measure affect the eligible individuals in the same way - just with opposite signs? Are the reform effects of moving to a more or less generous policy symmetric? This important question cannot be answered by standard program evaluation results and policy designers may thus implicitly assume symmetry of the effects. To address this issue, it is necessary to exploit specific information shocks, which preferably imply both *positive and negative* news to the target group. In this paper, we address the question of opposite policy changes by exploring a large-scale quasi-experiment in unemployment insurance (UI) with imperfectly informed UI claimers: Job seekers are confronted with either an upgrade or a downgrade of their benefit eligibility within their unemployment spell, without being initially fully informed about the change. They face, however, exactly the same size of treatment: an increase or decrease of the potential benefit duration (PBD) by 200 days. We first compare the treatment effects of these update cases with the reference case, in which individuals are fully informed about their PBD. We identify the treatment effect around the threshold of age 25 where PBD rules change in the Swiss UI system. We find substantial differences in the treatment effects across cases with different expectations on benefit change. Secondly, the effects are asymmetric both quantitatively and qualitatively. The differences are consistent with patterns of loss aversion or of consumption commitment behavior. We also show that policy uncertainty reinforces the job finding effect of a downgrade shock.

JEL codes: J64, J68, H41, D03, D83, D84

Keywords: asymmetric treatment effects, natural experiment, incomplete information, job search, policy evaluation, unemployment insurance

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©We would like to thank David Card, Rajeev Deheija, Lorenz Goette, Peter Kuhn, Thomas Lemieux, Jonathan Meer, Robert Moffitt, Arnaud Chevalier, Gerard van den Berg, Bart Cockx, Marco Caliendo, David Gray and seminar participants at UC Berkeley, the University of St.Gallen, University of Lausanne, LISER (CEPS/INSTEAD) Luxemburg, CEA (Ryerson University, Toronto), LEW (University of New South Wales, Sydney) and IZA for their valuable comments. Patrick Fechner and Christoph Schwerdtfeger provided excellent data and research assistance. Moreover, we would like to acknowledge the helpful support and data provision by the Swiss State Secretariat of Economic Affairs (SECO), in particular by Jonathan Gast.

# 1. Introduction

Does introducing or abolishing a policy affect the eligible individuals in the same way – by causing the same treatment effect, just with opposite signs? Are the reform effects of moving to a more or less generous policy symmetric? This question remains open and unanswered by standard evaluations of social policy reforms. It is, though, of crucial relevance if we want to learn from existing evaluation results for future policy making (external validity). In the standard evaluation cases, not all necessary counterfactuals are observed to address the symmetry question: we either see the introduction or the abolition of a measure, the increase or the decrease of a policy parameter – but not both. If changes at eligibility thresholds (age, income etc.) are exploited to identify the treatment effect, we do not know whether affected individuals *perceive* that they are better or worse off than the peers of the control group. In particular, we would need to know more about their information set in order to address the symmetry question. Optimally, we would need (natural) experiments in which an information "shock" *within* an individual case is observed. This paper exploits a unique natural experiment where such intra-individual information shocks are present with same magnitude but opposite signs.

Information shocks imply *incomplete information* at the individual level. In practice, it is indeed often realistic to assume that people are not perfectly informed about the upcoming changes in social policy rules. Social policy transfer schemes – like social security, welfare, unemployment or health insurance, childcare subsidy programs, income tax credits etc. – often involve plenty of detailed rules that crucially determine the net financial entitlements and implied incentives of these policies. Thus, often it is realistic to assume that affected people are aware of the general rules of the policy, but not of the relevant implementation details or the net impact of combinations of rules. Acquiring the relevant detailed information involves additional effort. The fact that individuals are imperfectly informed or do not entirely understand the rules that come with policy changes is usually ignored in the empirical literature. Moreover, the question of whether policy news with opposite signs would generate symmetric responses has not been addressed in the literature. Because, it is very rare to observe both shocks to the same group of individuals at the same time. This paper bridges the gap between the policy evaluation literature and the behavioral economics literature on reactions to potential gains and losses of the same size. We address and answer the question: whether treatment effects of opposite situations result in symmetric outcomes or not? We provide novel empirical evidence on this issue which is based on large-scale administrative data and "clean" exogenous policy shocks.

Information and expectation play essential roles when individuals make decisions facing certain changes in the economic environment that will potentially affect their future economic outcomes. The literature provides different examples of the relevance of information and knowledge in the context of various types of public policies. Chetty, Friedman and Saez (2013) shows that knowledge about policy rules (EITC is the policy subject in their paper) is an important key factor when individuals make optimal decisions and take advantage of the policy in the ways that they are aware

of. In particular, they use variation of the degree of "sharp bunching" by self-employed individuals at the first kink across areas with different ZIP codes to identify a "causal" impact of EITC on earnings. Their findings show that the "diffusion of knowledge" about EITC is a crucial mechanism through which a previously under-documented if not ignored "intensive margin" effect is identified. Their empirical results provide additional insights on the role of information played in policy evaluation. Ignoring asymmetric information or incomplete information (from the perspective of targeted groups) about intended changes in policy rules would bias the measured effects of such policy rules. An earlier research that addresses similar points is Alm (1988). He shows that greater uncertainty about income tax policies in the U.S. in the 1960s and 1970s generate behavioral changes among rational individuals. Risk in tax policies may generate less optimal decisions as well as loss of welfare in the absent of symmetric information.[1] He mentions as well the option that policy uncertainty could be used strategically by policy makers.

In this paper, we empirically analyze the role of information in the context of unemployment insurance. We assess how potential unemployment benefit duration (PBD) changes impact on job seekers' dependence on UI and their exiting behaviors. Thereby we focus on individuals who have limited information about the changes in UI benefit rules, and see whether and how information and uncertainty about future policy changes affect their behaviors. Access to both large scale administrative unemployment data set and a clean natural experiment that generates a substantial PBD cut for young job seekers allows us to identify the causal impact of changes in policy rules on various groups of targeted individuals with different information exposure about the policy rules. Furthermore, the nature of the policy change and its implication rules allow us to evaluate policy impacts of same-size changes in benefit but with opposite signs. We first show that facing uncertainty about future UI benefit rules, individuals behave differently in their (non-)employment decisions relative to the perfect information case. We then show that the reactions are not symmetric when facing exactly the same policy shocks but with opposite signs. In particular, job seekers react more to negative shock than they do to positive shock.

The change and implementation of UI benefit rules in the Swiss case provides clean natural experiments in which younger job seekers are confronted with exactly the same size of treatment effects (a PBD change by 200 days) but are exposed to incomplete information about the specific rules that will apply to them. Different patterns of reactions on cutting PBD are documented among three treatment groups. These groups are all potentially affected by a PBD change of the same size. The first group we consider, our reference group, is perfectly informed about the eligibility rules that apply to them right at the beginning of the unemployment spell. For this group, there is no uncertainty about the PBD they are entitled with. The second group, more interestingly, is initially uncertain about the specific rules applied to them since the implementation of a PBD cut arrives during their unemployment spell. Thirdly, we consider another interesting group of individuals who

[1] Other studies include Blundell, Francesconi, and van der Klaauw (2011) on announcement of reforms in in-work benefits and its impacts on female labor supply; Luttmer and Samwick (2012) on welfare costs and uncertainty in policy; Giavazzi and McMahon (2012) on policy uncertainty and household savings and labor supply.

have limited information about changes in eligibility status: in this case they expect a potential increase of PBD during their unemployment. The latter case is even independent of the timing of the policy reform. The comparison of the treatment effects among the three groups reveals that information and uncertainty about changes in eligibility rules generate different behavioral responses. And these responses depend also on the nature of the expected outcomes i.e. potential decrease/increase in UI benefit durations.

After revealing the differences in the reactions among different treatment groups, we try to provide potential channels/mechanisms through which one can interpret these results. These channels relate to the literature of loss aversion (Tversky and Kahneman 1991; Yechiam and Telpaz 2013), where the general conclusion is that, facing uncertainty in the future, people dislike losses more than they like gains. Other channels that could also explain our results are related to the literature on endowment effects proposed by Kahneman, Knetsch, and Thaler (1990). Finally, the consumption commitment theory discussed in Chetty (2003) could also intuitively explain our results.

The case of PBD change that we consider here to assess the role of information is related to a broad empirical policy evaluation literature. Evaluation of unemployment insurance programs has become popular in the past decades because such programs are most commonly used policy tools to help job losers with their economic hardship while searching for new employments. Unemployment insurance benefit level and unemployment insurance benefit eligible period are key parameters to consider when it comes to evaluate the impact of UI policy changes. Theoretical background on the impacts of potential UI benefit period on duration of unemployment and search outcomes originated from two streams of methodologies. Mortensen (1977) formulates a dynamic job search model incorporating the potential unemployment benefit duration. The model predicts that increasing potential benefit duration increases the value of being unemployed so that eligible unemployed individuals would stay longer in unemployment.[2] Moffit and Nicholson (1982) provide an alternative approach to theoretically incorporate potential benefit duration. They set up a labor-leisure model, where unemployed individuals have preferences over income and unemployment. Unemployment is attractive because of the leisure that comes with it[3]. They show that increases in the level and length of the UI benefit generate both income and substitution effects that are the forces to increase average duration of unemployment. Both afore mentioned theoretical backgrounds offer disincentive effects of prolonged potential UI benefit durations on job search outcomes, mainly employment outcomes. Job seekers stay longer in unemployment and claim unemployment benefit longer if they can.

[2]On the other hand, increasing potential benefit duration also generates incentives for those unemployed who are not eligible for UI benefit to leave unemployment, because the value of employment is higher, since the unemployment benefit value that comes with the risk of "laid off" in the future is higher due to longer benefit coverage.

[3] Upon entering unemployment, individual chooses optimally income and duration of unemployment subject to a convex budget constraint. The budget constraints are separated into three sections, indicating leaving unemployment before the eligible benefit period is exhausted, leaving unemployment right at the exhaustion period, or to leave unemployment after the eligible benefit period.

There is also a rich literature of empirical studies on potential benefit duration and its impact on unemployment spells or job search outcomes. For example, a short list of studies on the impacts of extending potential benefit duration include Katz and Meyer (1990), Card and Levine (2000), Lalive (2008), and Schmieder, von Wachter and Bender (2012). In general, their results show that a longer potential benefit period creates longer UI benefit dependence and hence longer unemployment spells. Moral hazard is usually the main behavioral explanation behind these results. On the other hand, Van Ours and Vodopivec (2006), provide empirical evidence on shortening the potential unemployment benefit duration using a natural experiment in Slovenia. They find that a decrease in potential benefit period results in higher exit rate of unemployment and shorter unemployment spell. Most of the literature on evaluation of UI potential benefit duration changes assume that information about UI benefit rules are complete and commonly shared between the policy maker and target groups. Individuals are assumed to have perfect foresight. Incomplete information and uncertainty about potential policy changes are usually ignored or not modeled. However, the results from an increase in PBD and a decrease in PBD from the mentioned papers are not readily comparable. Because, first of all, they are not the same group of individuals. Secondly, the magnitudes of the benefit changes are not the same. More importantly, there is no counterfactual environment where both potential upgrade and downgrade in PBD are observed. Once there is perfect information about PBD rules, individuals know exactly their entitlements making it impossible to compare their reactions to a positive *change* and to a negative *change* because there will be no change in PBD.

We believe that our paper contributes to the literature on policy evaluations using natural experiments, by incorporating explicitly the "fuzzy" cases, where individuals are not fully informed about the policy change. The behavioral responses from these relevant subgroups help us learn about how individual change their response to policy incentives if they are exposed to incomplete information. We go one step further to show that behavioral responses from expecting a positive benefit change are not symmetric to the behavioral responses from expecting a same size negative benefit change.

Our results are important from implementation of public policy point of view for two reasons: 1) we show that incomplete information about policy parameters will have different behavioral responses that are usually not taken into account by the policy maker who normally assumes perfect information from the perspective of target population. Individuals react to potential policy changes even with uncertainties by forming expectations about the upcoming changes in the rules. The way how information is spread and managed in public policy schemes can potentially reduce, improve or destroy planned policy effects. 2) We also show that it is misleading to assume symmetric effects from imposing a public policy that promises a benefit gain and from removing an ongoing policy that promises the same benefit gain. This is especially the case when people form expectation about the change before the implementation of the change.

The structure of the rest of paper is the following: section 2 provides institutional background of the Swiss unemployment insurance system and the specific policy change and implementation rules;

section 3 describes the data; section 4 presents the empirical model; section 5 provides results and some discussions; finally section 5 concludes the paper.

## 2. Institutional Background and the PBD Reform

In order to assess the impact of varying exposure to (incomplete) information, we exploit a natural experiment within Swiss unemployment insurance which has generated several comparable treatment groups that differ with respect to available information as well as upgrade and downgrade shocks.

The Swiss unemployment insurance (UI) system is quite typical in its policy design and well comparable to other UI systems within OECD. The potential duration of unemployment benefits (PBD) for prime aged individuals who fully contributed (18 out of the last 24 months) is 400 days. Job seekers who contributed less in the pre-unemployment period (12 out of the last 24 months) are eligible for 260 days. From age 55 on, benefits are extended by additional 120 days. The reform that we exploit here and will discuss below introduced an additional threshold of PBD change at age 25. The replacement ratio is 80%; and 70 % for job seekers whose insured earnings would correspond to a daily benefit payment of more than 140 CHF[4] and who are not caring for children. After the end of the entitlement period the unemployed have to rely on social assistance. Social assistance is means tested and replaces roughly 76% of unemployment benefits for a single job seeker with no other sources of earnings (OECD, 1999).

The second key eligibility criterion, in addition to sufficient contributions, is that a job seeker must be "employable". In particular, job seekers must possess the capability to fulfill the requirements of a regular job. If an individual is found not to be employable there is the possibility to collect social assistance. Upon registration at the Public Employment Service (PES) office, the job seeker is subject to further obligations: he needs to attend regular meetings with the caseworker (usually monthly); fulfill individually specified job search requirements (usually 6 to 12 applications per month, see Arni et al., 2015); attend regularly programs of active labor market policy (job search assistance, training, workfare programs); follow up on referrals that the caseworker provides him. The Swiss UI system is above OECD average in terms of monitoring intensity (Venn, 2012). In particular benefit sanctions – temporary benefit cuts of usually 5 to 10 days – are regularly used and can be imposed in case of observed non-compliance with one of the mentioned obligations. Empirical studies (Lalive et al., 2005, Arni et al., 2013) show that the effect of monitoring strictness and sanctions on unemployment duration and earnings is substantial in the Swiss case.

The UI system features four organizational layers. At the national level, the UI law defines all the mentioned eligibility rules, obligations and means of support. The Swiss system is characterized by a substantial amount of federalist leeway in the implementation of the common rules. The second

[4] 1 Swiss Franc (CHF) = 1.07 USD = 0.96 EUR

layer is constituted by the Cantons which are responsible for the organization of the agencies of the UI funds as well as of the PES agencies. About 160 PES offices feature the third level of the system; they are responsible for registering, supporting and monitoring the unemployed job seekers. Finally, about 2500 caseworkers build the personal backbone of the PES offices and execute tasks of job search assistance, monitoring of the requirements and acquisition of job vacancies among local employers.

In the context of this study it is important to note that there is a clear organizational separation between the tasks of support and monitoring – fulfilled by caseworkers and PES – and the tasks of benefit payments and eligibility checks. The latter are performed by the agencies of the UI funds which are also locally separate from the PES. Their databases are, however, connected. As a consequence of this separation, information exchange concerning monthly benefit payments and eligibility issues are handled by the UI funds (usually via monthly letters like payment statements etc.). Thus, caseworkers are not responsible to inform job seekers about their eligibility status with respect to PBD. In fact, they are not allowed to provide legally binding information on eligibility status (updates), this is in the sole responsibility of the UI funds.

The PBD changes we exploit as natural experiments originate in a reform of the Swiss UI law that has been implemented in April 2011. The reform introduced an additional age threshold in the benefit eligibility scheme at age 25[5]. Since April 2011, fully eligible[6] individuals below that age (at registration) were subject to only 200 days of potential benefit duration, whereas job seekers above age 25 keep the right to collect 400 days of benefits. This reduction applies only to persons without dependent children. The background and political aim of the UI reform was the reduction of expenses (and increase of the contributions) in order to balance out the funding of the UI funds in the longer run.

We will exploit the information variation and policy uncertainty that has been generated through the implementation of the new rules, in particular two sources of incomplete information (which correspond to the two cases discussed in section 4). First source is the rule that the PBD eligibility status is updated *within* ongoing unemployment spells. This means in our context that job seekers who enter unemployment slightly (max. 9 months) before their 25$^{th}$ birthday will first be subject to 200 days of PBD and then will be upgraded to 400 days of PBD *at* their 25$^{th}$ birthday. This particularity is, however, not common knowledge and job seekers would only find out about it in

[5] Note that there have been other smaller policy changes introduced within the UI reform 2011, in particular a slight increase in strictness of the sanctioning rules as well as a larger definition of „suitable" jobs that a job seeker is required to accept in order to avoid sanctions. Note that both of these changes apply generally (the first to everybody, the second to people below age 30), such that they are cancelled out by the natural experiment and the use of diff-in-diff estimation.
[6] Full eligibility means in this context that individuals must have contributed to unemployment insurance taxes at least during 18 out of the last 24 months before unemployment registration. If they contributed less – at least 12 out of the past 24 months – they become eligible for 260 days of benefits (below and above age 25). We do not consider this case in the empirical analysis and sample on fully eligible job seekers.

advance if they would actively inquire at the UI funds[7]. Otherwise, the UI funds will only communicate the update of their eligibility status to 400 days of PBD within the mentioned regular mail exchange (monthly payment statements) from the date of 25th birthday onwards.

The second source that introduced policy shock is the implementation of the reform. Due to a referendum there has been a public vote on the UI reform by end of September 2010. For this vote, information about the intended adaptations of the general PBD eligibilities has been spread by the usual official information bulletin (Schweizerische Eidgenossenschaft 2010). The details of the implementation – and even the implementation date – were, however, not yet defined and known. Because of this late referendum, implementation rules and their internal communication (to the UI funds and the PES) have been realized very late[8]. Finally, the implementation date has been fixed to be the first of April 2011[9], and the government decided that the revised eligibility rules were applied immediately to *ongoing* spells. This timing and the immediate implementation created therefore a situation of incomplete information in the months before the reform where the general elements of the reform were known but not the timing of the implementation and to whom they exactly applied.

# 3. Data

For this study we use a very rich micro level data extracted from the Swiss Unemployment Insurance Register (UIR). We have access to individual data that covers 100% of the population of registered unemployed. The data contains all the usual socio-demographic information (age, gender, civil state, nationality, characteristics of the last job, education, mother tongue, foreign language skills, household size, insured earnings, eligibility state and conditions). The spell information (entry, exit) and all the labor market policy and sanction events are registered on daily basis. Based on this, we construct variables on the unemployment history in the 3 years before the current spell (incidence and duration of the spells), as additional controls. Moreover, the data feature fine-grained aggregation information: identifiers of each job seeker's Canton and municipality of residence as well as the PES agency (and caseworker) she is assigned to. We use PES fixed effects as control variables that take into account differences in economic and cultural conditions as well as in PES-

[7] Note that the general PBD eligibilities by age are communicated in the compulsory introductory information event 1 to 2 weeks after unemployment registration (and in the brochure that is handed out). The special case of updating *within* the spell is, however, not mentioned. Moreover, even the caseworkers may not be aware of this special case because it is only mentioned in one sub-paragraph of the implementation directive (see SECO 2011, paragraph 2a, page 20, related to Art. 27, Abs. 2,4,5 and 5bis of the Swiss UI law (AVIV)) that has been distributed by the Swiss State Secretariat of Economic Affairs (SECO) in 2011. Moreover, caseworkers are not responsible and not allowed to provide legally binding information on individual eligibility status to the job seeker, as mentioned above.

[8] The implementation directive (SECO 2011) has been sent to the Cantonal UI funds and to the PES in February 2011.

[9] The original plan was to implement the reform in January 2011, which was not possible any more due to the late referendum. Note that in Switzerland political opponents of a law can collect 50'000 signatures to urge a referendum.

level policies. These fixed effects cover even small regional areas: Switzerland is decomposed in about 160 PES regions.

To exploit the quasi-experiment of a PBD change by 200 days, we will thus focus on the fully eligible young job seekers around the age of 25 without children (which is the large majority[10]). Sampling on full eligibility, no children, an age window from 22.25 to 27 and three unemployment inflow windows between April 1st, 2009, and April 1st, 2013, leaves us with a gross estimation sample of 53'705 unemployment spells (37'119 individuals). The details on the age and inflow samplings for the specific treatment and control groups within our (diff-in-diff) analysis will be reported in section 4. The end of the observation window for the spells is August 31st, 2014.

[Figure 1 around here]

The left panel of Figure 1 reports the distribution of the realized durations of registered unemployment for the gross estimation sample (censored after two years). The median unemployment duration for this sample of young job seekers is 133 days. The figure shows the typical shape of unemployment exit behavior: unemployment exit rates are clearly highest in the first four to six months. In the period of interest, 2009 to 2014, the labor market conditions in Switzerland have been quite stable. The yearly median unemployment durations for the sample vary between 121 and 155 days[11]. Note that we will censor all the individual unemployment spell durations after one year. This is due to the fact that we consider the PBD of 200 days as a treatment. These benefit days are working days which translate into a calendar duration of about 11 months. Thus, considering durations after that point becomes meaningless since the treatment group has left the register by default[12]. The right panel of Figure 1 shows the Kaplan-Meier survivor function for our gross sample. After 12 weeks of unemployment, 30% of the sample population has left the register (for jobs or without job), after 40 weeks a bit less than 25% of the job seekers are still unemployed.

[Table 1 around here]

[10] Within the chosen inflow and age windows which generate our gross estimation sample, the group of fully eligible individuals without children represents 69% of the registered job seeker population.

[11] The official unemployment rate (published by the Swiss State Secretariat for Economic Affairs SECO) for individuals between age 20 and 30 was around 4% in these years.

[12] Note that the difference between 200 days (9 months) of PBD and 11 months of „real" duration is explained by weekends, public holidays, „benefit holidays", possible periods of sickness etc. Note as well that there are minor quantities of individuals subject to 200 days of PBD who stay longer in the register than 11 months; in specific cases it is possible to follow ALMP programs even after benefit exhaustion. Since we are interested in the unemployment duration while being eligible for benefits we censor these cases after one year.

Table 1 reports a selection of important socio-demographic characteristics of the estimation sample (aged 22.25 to 27 at unemployment entry). 46% of the sample is female job seekers. The highest education of two thirds of these young job seekers is at the secondary level; the vast majority possesses a vocational degree (apprenticeship of three to four years). One third of the registered unemployed in this age window are foreign born. 75% of the sample report a national language (German, French, Italian) as their mother tongue. 79% of the individuals live in urban or sub-urban municipalities. Lower skilled occupations dominate the pool of unemployed. On average, the insured monthly earnings (gross earnings subject to social security) amount to about 4000 CHF within the sample. 54% of the job seekers in the sample did not experience any unemployment in the past 3 years; this explains why the reported mean duration of unemployment experience is relatively low. Note that we use is the full population of registered job seekers (within the mentioned sampling frame), thus there is no issue of selectivity that could harm external validity.

# 4. Empirical framework

The Swiss unemployment insurance PBD reform was implemented on April 1st of 2011. The reform affected only people who are below age of 25 at the time of unemployment registration, and their eligible benefit duration is cut by half from 400 days to 200 days. Such a dramatic cut in PBD is quite unique as most public policy adjustments are small in magnitudes. The "sharp" treatment difference around age 25 provides an excellent "exogenous" shock for the young job seekers (below age 25) in the post reform period. This allows us to implement a clean difference-in-difference estimation procedure to identify the "causal" links between the policy change and the exiting behaviors of the registered job seekers.[13] Following Cameron and Trivedi (2005), our empirical econometric model takes the following form:

$$Y_i = \alpha + x_i^{'}\beta_1 + \gamma^1 I_i^{post} + \gamma^2 I_i^{treat} + \delta D_i^{DID} + \pi^1 age_i^{pre,c} + \pi^2 age_i^{pre,t} + \pi^3 age_i^{post,c} + \pi^4 age_i^{post,t} + \eta_t + \mu_r + \varepsilon_i \quad (1)$$

We model outcome variable Y in a standard linear diff-in-diff set up. We consider three outcomes for individuals who are registered at unemployment agencies in Switzerland between April 1st of 2009 and April 1st of 2013. In particular, we look at probability of leaving unemployment to a non-job state; probability of finding a job and the duration of the registered unemployment spell. As usual, we control for individual socio economic characteristics with X.[14] $I_i^{post}$ and $I_i^{treat}$ are

[13] Similar large scale policy changes in welfare benefit levels in Quebec 1986 are also utilized by Lemieux and Milligan (2008). They focus on the sharp discontinuity around the age threshold and adopted RDD approach. In an earlier version of the current paper, we also exploit this nature of our natural experiment and found similar results.

[14] Such characteristics include gender, education, previous occupation, previous jobs tasks, language, insured earnings etc.

indicators for the post policy reform period and the "treatment" status respectively. We also control for age trends before and after the policy change for the treatment and control groups separately. $\eta_t$ captures common time trends, for example, seasonal dummies. $\mu_r$ captures geographical differences among unemployment individuals, it accounts for any local economic environment that may affect the job search outcomes of unemployed. By controlling for both time and geographic location of the unemployed together with a rich set of observed socio-economic characteristics and separate age trends, we believe that the exogenous policy shock will help us to identify a causal effect. Finally $\delta$ is the parameter of interest that gives us the treatment effect on the treated.

In order to accurately identify the treatment effect of the PBD change, we select individuals who were between age 22 and 27, and who entered unemployment spells between April 1st of 2009 and April 1st of 2013. This way, on top of a rich set of control variables, we have a balanced sample of young job seekers who share similar observed and unobserved heterogeneity and hence are more comparable. The unemployment inflow time window is also "balanced" by using 2 years before and after the implementation of the new unemployment PBD rules.

We now construct three pairs of control and treatment groups depending on individual's perception and expectation of the implemented PBD rules. From now on, we will use case 1 2 and 3 to refer to these comparison groups. The first case is where there is no incomplete information about the PBD reform and the implementation rules. Individuals are certain about the different treatment of unemployment benefit duration entitlement below and above (inclusive) age 25. In particular, we choose from the universe of all registered unemployed individuals who enter unemployment between April 1st of 2009 and April 1st of 2010 (before the reform), and between April 1st of 2011 and April 1st of 2013 (after the reform). Our control group is comprised of those who at the entry of unemployment are 25 and above. Our treatment group contains individuals who are between age 22.25 and 24.25. The treatment age window may appear arbitrary. However, age 24.25 is crucial to us, since by selecting this age threshold, we rule out the possibility that after the reform individuals who are closer to but not yet age 25 will be updated with longer PBD of 400 days instead of 200 days.[15] Therefore in case 1, the controls are those who would have 400 days of PBD before and after the reform, and the treated are those who would have a cut of PBD to 200 days after the reform. This comparison group will serve as our reference group when we discuss the results in the next section, as this group provides us the benchmark results without changes in expectations. Both treated and control know about their eligibilities with certainty.

In addition to case 1, we then construct two other comparison groups with less complete information about the PBD rules. When implementing the new PBD rules, the Swiss government decides to update individual's benefit period upon age 25. For example, if an individual enters unemployment spell before age 25 after the implementation of the new rules, he/she will be

[15] This is actually the case, after consulting relevant documents and staff members at the Swiss Unemployment Insurance Register Office. Case workers will update PBD for individuals from 200 to 400 days, while they reach age of 25 during the unemployment spell.

upgraded to have 400 days of PBD when he/she turns 25 while still in the same unemployment spell. The Offices of Unemployment Insurance Funds (UI payment agency in Switzerland) only updates the individuals' eligible PBD in the month of their 25th birthday and does not inform the job seekers in advance. This creates an uncertainty or lack of information for unemployed individuals who are close to age 25 after the reform. Based on the uncertainty about the future pbd, individuals form expectations about potential pbd. Hence those who are close to age 25 when entering unemployment spell expect that there is a chance that they may get longer pbd soon enough after they reach age 25. To them, there is a positive shock with some probability in the near future. Such a "fuzzy" case would usually be ignored in the literature on evaluation of unemployment insurance PBD changes in the past. We explicitly take account of this unique population and form the second comparison group, in which the "control" group includes people who are between age 22.25 and 24.25 at the time of entering unemployment spell. Similar to the treatment group in case 1, the inflow time windows are between April 1st 2009 and April 1st 2010 and between April 1st 2011 and April 1st 2013. In this case, the "treated" group is older than the treated but slightly younger than 25 when entering unemployment. Hence their potential benefit period could be upgraded from 200 days to 400 days. Therefore, we are comparing individuals who are entitled with 200 days (younger ones without possible changes in pbd) with individuals who are entitled with 200 days but will be upgraded to 400 days if reach age 25 (slightly older individuals with potential increase in pbd). Our diff-in-diff estimator would then causally identify the effect of a potential 200 days increase in benefit duration on those who will be approaching 25 soon enough. The double differences in outcomes before and after the policy change should eliminate any fixed unobserved heterogeneity between the two age groups.[16] To further focus on the role of incomplete information and show whether expectations about pbd would generate actual reactions, we censor outcome variables (exit rate and unemployment duration) at age 25 for both control and treated. This is necessary, because before age 25, the treated expect a change (increase) in PBD with a probability. After age 25, there is no potential change in pbd anymore for both control and treated. To compare the results from the censored outcomes for case 2 with the results from case 1, we censor outcomes in case 1 the same way as we did to case 2. The censored result from case 1 would serve as a good reference point with perfect information on PBD.

As a final step, we form our last comparison group, which is case 3. In this case, we consider the inflow time window that is before and not far from the implementation date. This inflow window is between July 2010 and February 2011. Consider for example an individual who enters unemployment spell in the above mentioned time frame at age of 23. Initially she expects 400 days of PBD upon entry, although she is young but the policy of shortening PBD has not been put into place yet. As she continues to rely on the UI benefit, the reform hits her so that she suddenly loses

[16] It is easier to interpret the results as the effects of an upgrade of pbd on those who initially thought that they would have 200 days but later updated to 400 days. Econometrically, it does not change the results quantitatively by switching control and treatment groups, for example, we could use individuals who are younger and have 200 pbd with certainty as the treated and use the people who would face an increase from 200 to 400 as control. Theoretically, we are comparing those who are facing an upgrade of 200 pbd with those who face no changes in pbd.

half of her potential benefit dates in the future. This is because she is still younger than 25 when the PBD reform takes place. Such a "negative" surprise gives us an opportunity to form another informative comparison group which contains individuals who could potentially face negative news while unemployed, and individuals who will not be affected by such news anyways. In particular, we consider two inflow windows: from April 1st 2009 to April 1st 2010, and from July 1st 2010 to February 1st 2011. The control group is comprised by those who enter unemployment above age of 25 in the above mentioned inflow window. Our treated individuals are those who are between age 22.25 and 24.25 and enter unemployment between July 2010 and February 2011. The treated is facing a PBD cut on April 1st of 2011. This cut is a valid "shock" for the treated because the entry period for the treated (between July 2010 and February 2011) is selected to guarantee that individuals will potentially "experience" the PBD cut within 9 months (200 days) of unemployment.[17] In this case, we are comparing unemployed individuals who have 400 days of PBD with individuals whose PBDs are likely to be cut by 200 days. One should notice that the "post" indicator $I_i^{post}$ now equal to 1 for the period that is before the implementation date, i.e. July 2010 to February 2011. In the diff-in-diff framework, usually, the post means after the reform. But in this case, our definition of post refers to the entry period that is exposed to potential cuts in PBD. Because we are interested to find out whether relatively younger individuals who enter the post period exhibits different behavioral patterns than the unaffected older individuals. In this case the natural experiment should help us identify the effects of expecting a negative PBD change on individual's behavior. Furthermore, to pin down how information may alter the effect of PBD cut, we analyze censored outcomes (probability to exit to employment, probability to exit to non-employment, and duration) before the arrival of the PBD change in April 2011. To form a meaning comparison group, the censoring on outcomes is also applied to the control group where there is no uncertainty involved.

[Table 2 around here]

Both case 2 and 3 consider individuals who face incomplete information regarding PBD rules and form expectations about the potential changes in PBD. However the nature of the expectations differs between the two cases. First of all, the post period in case 2 is after the implementation of the PBD change. So people should know about the age distinction about PBD days in general, the only uncertainty comes from the specifics of how such a PBD change is implemented. People entering unemployment when under age of 25 form expectations about future PBD rules in the beginning. If the government does not inform them an update of changes in PBD entitlement individuals are less sure or even ignore the possibility of being upgraded when reach age 25. We should then expect a minor or even negligible effect for this group compared with the controls that

[17] Note that there is a 9 month gap between July 1st 2010 and April 1st 2011.

will for sure have 200 days in any event. On the other hand, for case 3, the negative news is more pronounced since the post is defined before the actual policy reform was in place. The Swiss government has passed a law to mandate PBD reform about 9 months before the implementation of the reform so that young individuals have already form an expectation of such a PBD cut in the future. They just don't know when exactly the reform will hit. Secondly, uncertainty in case 2 makes people expect a potential gain in PBD. However, uncertainty in case 3 makes people form a negative expectation about PBD loss in the future. It turns out that such a difference in nature of the future prospects revealed by case 2 and 3 provides us additional insight about how rational individuals react in face of potential policy changes with opposite signs. The next section will present the estimation results of the empirical model throughout 3 cases and offer a discussion.

# 5. Results

We estimate equation (1) for case 1, 2 and 3 respectively. For each case, we run two sets of regressions with the non-censored outcomes and censored outcomes separately. Table 3, Table 4 and Table 5 document the estimated treatment effects on three outcomes: probability to exit from unemployment to employment; probability to exit from unemployment to non-employment (temporary leave labor force); and unemployment durations in days. We also report estimates on the coefficient of treated, post, and constant terms. The estimates on socio-economics as well as geographic variables are not reported to save space. But these estimates are available upon requests from the authors.

[Table 3 around here]

The implemented PBD cut has significant effects on job seekers behavior. We can see from Table 3, cutting PBD by half makes unemployed individuals more likely to leave unemployment for both jobs and temporarily out of labor force. The effect is more prominent on leaving unemployment but not for employment reasons (5.2%). As expected, unemployment duration is also lower for the treated. On average they stay 9.7 days in unemployment less than their statistical counterparts in the control group. The censored outcomes also exhibit expected patterns in treatment effects for the complete information in case 1. After censoring, we look at outcomes at early stages of the unemployment spell: the effects are weaker in terms of magnitude but still significantly different from zero. For example, exiting to non-employment is 1.5% higher for the treated, and unemployment duration is 4.2 days shorter for the treated. This suggests that young unemployed job seekers facing a confirmed PBD cut react early. A PBD cut of 200 days makes unemployment insurance benefit less attractive in terms of expected UI benefit income streams. It hence makes other alternatives, such as going to school or leaving unemployment benefit temporarily more attractive to the job seekers. Interestingly, the employment effect of such a PBD cut is not as

obvious as the effects on other two outcomes. Young job seekers tend to leave labor force (temporarily) and rely less on unemployment benefit.

[Table 4 around here]

Without restricting ourselves to much, we could assume that people form expectations about future outcomes and act accordingly. In case 2 they potentially face a "positive" shock in the future. In addition, they do not expect a "negative" shock in this case the reduction of PBD anymore since they are already younger than 25 and have 200 days of PBD. The results from case 2 are quite revealing (see Table 4). For the censored case, the job seekers who could potentially face an upgrade of PBD do not react to such an expected gain of 200 days in PBD. The coefficients of the diff-in-diff variables on three outcomes are not significantly different from zero. However, when we look at the non-censored case, we see an impact on existing rate. This effect is mainly driven by individual responses after they reach age 25 but not before that. The minor effects from the censored case show that even facing expected gains in PBD by 200 days individuals do not react differently than those who would not expect any changes in PBD.[18]

If we compare results from case 1 and case 2, we can see that expectation about PBD rules play an important role in driving differences in the treatment effects. *Changes* in PBD entitlement implied by case 2 is the only reason that individuals reacts differently from case 1. The "uncertainty" about this future shock due to lack of information about PBD rules would rather make them not responding to the shock even though it is a significant positive shock.

[Table 5 around here]

Table 5 tells us about behaviors responding to "negative" shocks in the future. For the censored case, the treated in case 3 are both more likely to find jobs and more likely to exit to non-employment. The effects are significant and statistically different from zero. This suggests that facing a potential "negative" shock in the future, people do react.[19] Again, we can assume that people form expectation about the future given their current information set. The law on PBD change has been passed a year before the implementation of the actual PBD change. Individuals

[18] This could be that the government did not inform them about the updating rules, or could be that they do not know about it even if the information is available. But from the perspective of UI claimers, they do care about their benefit entitlement and should have incentives to find out soon enough.

[19] The non-censored effects are even more prominent than those in case 1. For example, relative to control, the treated are 2.2% more likely to find job and 5.8% more likely to exit to non-employment states, and stay on average 15 days less on unemployment. These results are driven by both before and after the information about PBD cut is shared (implementation date of PBD cut is the censoring point).

share common information about potential cut in PBD for the young. If a young job seeker (our treated in case 3) enters unemployment before the implementation of the PBD cut, he is entitled with 400 days of PBD, however, he expects that in the future there is a good chance that he will lose 200 days of entitlement. Our diff-in-diff results show that he does react on the negative expectations about the huge PBD cut.

[Table 6 around here]

To summarize the findings in an illustrative way, we document in Table 6 the effects of the PBD change across three cases in an intuitive way. An upward arrow means a positive effect and double arrows means the effects are stronger in magnitude. "0" means that the treatment effect is not significantly different from 0. A comparison of the effects shows that especially for the censored cases, expectations of upgrading in PBD by 200 days does not generate any responses from individuals, while expectations about downgrading in PBD by 200 days makes people nervous about the future and triggers a positive exiting effect. The "natural experiment" on PBD in our context not only provides clean identification conditions but also provides a unique scenario, in which individuals are exposed (expectedly) to both a gain and a loss of same size in PBD, i.e. either a gain of 200 days or a loss of 200 days. Our results further reveal that an average risk-averse individual is more sensitive towards potential losses than potential gains facing uncertainty in both cases. This finding is in line with the behavioral arguments about "loss aversion" in Tversky and Kahneman 1991 and Yechiam and Telpaz 2013. The causal effect showed in our case 3 can also be explained by consumption commitment theory proposed by Chetty 2003, who argues that if the magnitude of income "shock" is larger, individuals will react by changing their durable goods consumption patterns. A PBD reduction of 200 days could be considered as a substantial drop in the "income" stream that is derived from unemployment insurance payments, such a negative shock is big enough to make our treated individuals in case 3 to decide to leave unemployment early to avoid the potential income loss and "smooth out" the current consumption.

# 6. Conclusion

The literature on public policy evaluations and/or social policy schemes has flourished for many years. This literature has offered many theoretical and empirical evidences from various types of natural experiment settings around the world. Yet most of the existing studies have focused on the "treatment" effects identified based on the assumption that information is symmetric and there is no changes in expectations of policy rules from the perspectives of targeted population, i.e. the clean case. That has led to a lack of empirics that offer evidences on the importance of information and expectation in the context of public policy evaluations. More importantly, there has been no research (as far as we know) that analyze symmetric/asymmetric policy impacts from public policy

changes with the same size but opposite signs in nature. For example, the impact of implementing a social welfare policy change that brings more generous benefits could be asymmetric to the impact of removing a welfare policy that implies the same benefit levels. We fill this gap by taking advantage of a nationwide unemployment insurance policy reform that has been implemented in Switzerland together with access to a large scale administrative data set that records all registered unemployed individuals throughout Switzerland.

The natural experiment we exploit provides an excellent context to show how information matters in directing individual responses under different scenarios. First of all, the policy reform is "clean" and "sharp" because the only condition for different treatment in PBDs is whether one reaches age 25. Newly unemployed individuals receive 400 days of PBD if they are at or above age 25 and receive 200 days if they are younger than 25. There are no economic reasons to believe that people just above age 25 and people just below 25 share very different preferences in job search behaviors. The different PBD rules around age 25 offers sufficient identifying condition to estimate causally the policy impacts. Secondly, the selected three comparison groups (case 1 2 and 3) only differ in terms of age of entry into the unemployment spell and the calendar time at which they enter. We do not observe different patterns of entering unemployment spell across all three cases and between all control and treatment groups. This is reassuring, because the only differences between the treatment group and the control group are the difference in the entitled PBD days (either 400 days or 200 days) and potential changes in such rules depending on age. Furthermore, the natural experiment and its implementations in the Swiss context provides rare and valuable expectation shocks of the same magnitude (200 days in PBD) but opposite signs. It allows us to see whether expectations of opposite policy changes would generate symmetric treatment effects.

Empirical results from the diff-in-diff regressions across all three cases show that incomplete information and expectations on policy changes are the key to explain differences in the treatment effects among individuals who face the same policy reform but have different knowledge about the specific rules that apply to them. Traditional evaluation of policy change assumes perfect information and that individuals are clear about the rules applied to them. We further find that individuals respond to potential policy changes by forming expectations. We document the treatment effects of the perceived changes in policy. Such treatment effects are different than those effects under perfect information and normally reported in the literature. Policy makers should be aware of the treatment effects of potential policy change even before its implementation because rational agents react on expectations. This can be seen for example, by comparing results from censored case 2 with results from censored case 1. A comparison between results that are based on censored outcomes from case 1 and case 3 also delivers the message: information does matter and it generates different treatment effects among different groups given the same reform.

Comparison of the results between case 2 and 3 provides additional evidence on symmetric perceived policy changes and its impact on behaviors. In particular, the treated individuals with censored outcomes in case 2 and 3 share common elements of incomplete information from the same design of the reform but form opposite expectations about PBD changes. In particular, the

main difference between these two treatment groups in censored case 2 and case 3 is that one is facing a potential upgrade (PBD increase of 200 days in case 2), and the other is facing a potential downgrade (PBD decrease of 200 days in case 3). The reactions identified by the diff-in-diff approach in both cases are differ a lot. 1) The reactions are not symmetric even though the potential changes are symmetric in magnitude. 2) People take "negative" potential shocks more seriously than "positive" shocks. When there is a potential PBD cut in the future, individuals would adjust their behavior by having a higher probability to leave the unemployment spell even there is uncertainty involved in the advent of such shock. Part of the reason for this pattern could be due to the fact that the information about the implementation of a PBD cut was announced sometime before the actual implementation date by passing the referendum on the cut of PBD for the young job seekers. However the Swiss government did not provide any information on the specific date of the PBD reform to the public while passing the referendum. This in turn generates "expectation" effects of the PBD cut among the young before the actual implementation date. This effect is captured by the analysis of case 3.

In this paper, we provide not only evidence on the importance of incomplete information in public policy evaluations but also additional evidence on the asymmetric treatment effects of potential policy changes with the same size.

Our results support the argument of "loss aversion" mentioned in Tversky and Kahneman 1991 and in Yechiam and Telpaz 2013. The reason why the treatment effect is non-exist in case 2 (potential upgrade) and the treatment effect is significant and positive in case 3 (potential downgrade) can be that for an average risk averse individual, disutility from a potential gain outweigh the utility from a potential gain of the same magnitude. People hence will act accordingly to avoid the higher disutility from a loss, which in this context is a PBD cut. Alternatively, our results also echo the argument of "consumption commitment" by Chetty 2003. Facing a possible major loss in the future (a PBD cut), individuals adjust their behaviors to minimize the income variation and try to maintain their previous consumption levels. Therefore, they will seek for other alternatives to leave unemployment spell because the expected benefit from staying in unemployment is lower with a PBD cut. However, people may not adjust their behavior at all as in case 2, because the potential gain from the unemployment insurance benefit does not require additional effort or behavior change in order to keep the same consumption pattern in the event of the positive shock in the future.

To further disentangle the channel to interpret the results in this paper between "loss aversion" and "consumption commitment" additional analysis with richer individual level information is needed. For example more information about individuals' financial situations, such as family asset and liquidity holdings, consumption patterns on durable goods would help to determine whether consumption commitment fits better when it comes to interpret our results. This is beyond the objective of this paper but guarantees future research.

Finally our results offer important and general guidance to policy makers. Expectations on future policy changes among targeted groups would potentially alter individual responses in ways that are

usually not easily expected and ignored by policy makers. Such impacts of potential policy changes are difficult to avoid as implementing new rules or removing current public policy requires discussion period, voting period and other necessary political procedures before it is put in place. Individuals would have time to form expectations and respond as suggested by this paper. Ignoring such expectations could result in unexpected outcomes from the targeted population. It is recommended that designing and implementing new public policy rules should consider the distribution of relevant information in a way that it does not distort the intention of the policy change. It is also naïve for the policy makers to assume symmetry in the treatment effects of expected changes in policy parameters even the intended changes in the parameters are symmetric.

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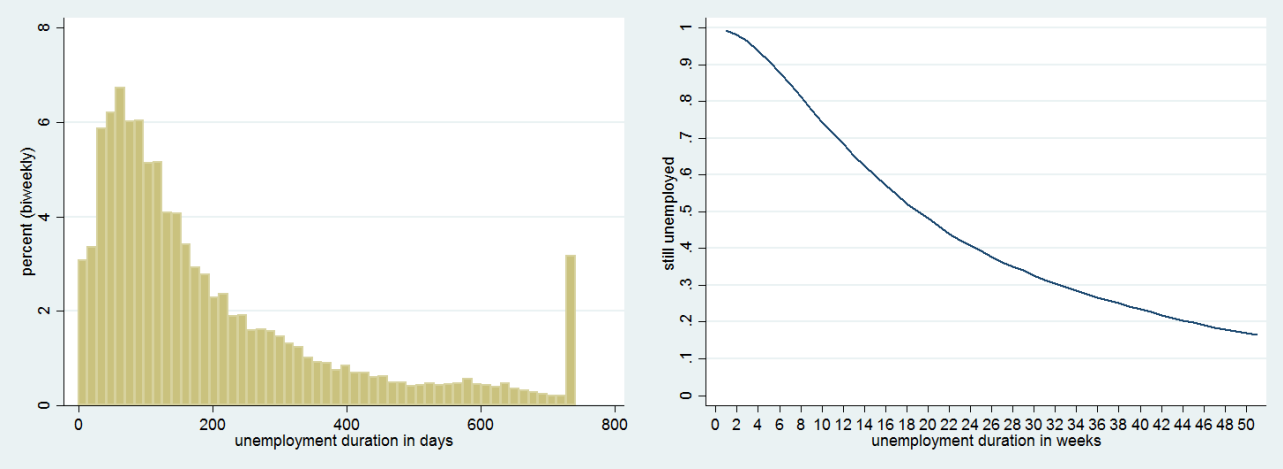
# Figures

Figure 1: Distribution of realized unemployment durations in estimation sample




*Note:* Full estimation sample (age at unemployment entry 22.25 to 27), 53'705 observations; unemployment durations censored after 2 years (730 days) in left panel and after 1 year in right panel

*Source:* Swiss Unemployment Insurance Register (AVAM/ASAL) dataset

# Tables

Table 1: Descriptive statistics on socio-demographic characteristics in estimation sample

| | | | St. dev. |
|---|---|---|---|
| Unemployment duration | (median in days) | 133 | 118.08 |
| | | | |
| Gender | Female | 0.455 | |
| Education | Primary (<=11y.) | 0.211 | |
| | Secondary (12-13y.) | 0.666 | |
| | Tertiary (>=14y.) | 0.053 | |
| Mother tongue | German | 0.487 | |
| | French | 0.212 | |
| | Italian | 0.049 | |
| Foreign born | | 0.318 | |
| Insured earnings | (mean, CHF) | 4049.71 | 1230.91 |
| Occupation | Sales | 0.156 | |
| (5 biggest) | Production (blue collar,etc.) | 0.128 | |
| | Gastronomy | 0.123 | |
| | Office & admin | 0.111 | |
| | Construction | 0.108 | |
| Job type | Apprentice | 0.039 | |
| | Support task | 0.225 | |
| | Professional | 0.713 | |
| Urbanization | Centers | 0.359 | |
| | Sub-urban | 0.427 | |
| | Sub-industrial & Touristic | 0.123 | |
| | Rural | 0.091 | |
| Previous unemployment experience | (duration in days for past 3 years) | 80.81 | 128.29 |
| | | | |
| N | | 53'705 | |

*Notes:* Descriptives are reported for the estimation sample of unemployed who enter registration between age 22.25 and 27. Proportions are presented if not otherwise stated.
*Source:* UIR dataset

Table 2: Overview of the comparison cases generated by the natural experiment

| | | | |
|---|---|---|---|
| **1) Reference Case** | **200 pure** | **vs.** | **400 pure** |
| | [22.25; 24.25] | | [25; 27] |
| | treated | | control |
| **2) Upgrade** | **200 pure** | **vs.** | **update 200 → 400** |
| **200 → 400** | [22.25; 24.25] | | [24.25; <25] |
| | control | | treated |
| **3) Downgrade** | **update 400 → 200** | **vs.** | **400 pure** |
| **400 → 200** | [22.25; 24.25] | | [25; 27] |
| | treated | | control |

Table 3: Reference case (full information): treatment effects

| | *Non-censored* | | | *Censored* | | |
|---|---|---|---|---|---|---|
| | finding job | (temp.) exit labor force | UE duration | finding job | (temp.) exit labor force | UE duration |
| **DID TE** | **0.0112** | **0.0522*** ** | **-9.6830*** ** | **-0.0089** | **0.0145*** ** | **-4.229*** ** |
| | (0.0098) | (0.0072) | (2.5043) | (0.0103) | (0.0052) | (1.4579) |
| treated | 0.0231*** | 0.0008 | -7.2465*** | 0.0305*** | -0.00492 | -0.969 |
| | (0.0083) | (0.0058) | (2.1338) | (0.0086) | (0.0042) | (1.2386) |
| post | 0.0101 | 0.0133** | -11.1435*** | 0.0372*** | 0.00850* | -2.797** |
| | (0.0084) | (0.006) | (2.1571) | (0.0088) | (0.0044) | (1.2574) |
| Constant | 0.6993*** | 0.1190*** | 165.4480*** | 0.396*** | 0.0690*** | 92.97*** |
| | (0.0149) | (0.0114) | (3.7806) | (0.0155) | (0.0084) | (2.1944) |
| Observations | 38'737 | 38'737 | 38'737 | 38'737 | 38'737 | 38'737 |
| Covariates | YES | YES | YES | YES | YES | YES |
| Age trends | YES | YES | YES | YES | YES | YES |
| regional FE | YES | YES | YES | YES | YES | YES |

*Notes:* Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The spells for the early treatment effects are censored at the counterfactual time of the change of the PBD eligibility status (at 25th birthday in case 2; at reform date, April 1st 2011, in case 3). In reference case 1 the same censoring scheme than in case 2 is applied (by a mean-preserving uniform random spread; after 138 days in median), in order to allow for comparability across cases.
*Source:* UIR dataset

Table 4: Case 2, upgrade from 200 to 400 days of PBD (incomplete information): treatment effects

| | *Non-censored* | | | *Censored* | | |
|---|---|---|---|---|---|---|
| | finding job | (temp.) exit labor force | UE duration | finding job | (temp.) exit labor force | UE duration |
| **DID TE** | **0.00692** | **0.0404*** ** | **-5.234** | **0.00694** | **-0.00172** | **-0.0885** |
| | (0.0135) | (0.0098) | (3.4390) | (0.0138) | (0.0072) | (1.9012) |
| treated | 0.0121 | 0.00752 | -3.632 | -0.000599 | 0.00177 | -1.674 |
| | (0.0112) | (0.0079) | (2.8849) | (0.0117) | (0.0056) | (1.6655) |
| post | 0.0128 | 0.0266*** | -15.54*** | 0.0249* | 0.0227*** | -6.264*** |
| | (0.0129) | (0.0094) | (3.2659) | (0.0130) | (0.0069) | (1.7823) |
| Constant | 0.719*** | 0.107*** | 156.6*** | 0.440*** | 0.0637*** | 93.13*** |
| | (0.0193) | (0.0150) | (4.8702) | (0.0199) | (0.0108) | (2.7887) |
| Observations | 27'320 | 27'320 | 27'320 | 27'320 | 27'320 | 27'320 |
| Covariates | YES | YES | YES | YES | YES | YES |
| Age trends | YES | YES | YES | YES | YES | YES |
| regional FE | YES | YES | YES | YES | YES | YES |

*Notes:* The control group is composed by those who have a potential upgrade from 200 PBD to 400 PBD, the treated are those who have 200 PBD. For better interpretation, in the paper, we flip the two groups. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The spells for the early treatment effects are censored at the time of the change of the PBD eligibility status (at 25th birthday).
*Source:* UIR dataset

Table 5: Case 3, downgrade from 400 to 200 days of PBD (incomplete information): treatment effects

| | *Non-censored* | | | *Censored* | | |
|---|---|---|---|---|---|---|
| | finding job | (temp.) exit labor force | UE duration | finding job | (temp.) exit labor force | UE duration |
| **DID TE** | **0.0221**** | **0.0575***** | **-15.1226***** | **0.0234*** | **0.0214***** | **-2.023** |
| | (0.0112) | (0.0088) | (2.7892) | (0.0120) | (0.0069) | (1.4462) |
| treated | 0.0248*** | 0.001 | -8.6112*** | 0.0344*** | -0.00279 | -3.486*** |
| | (0.0083) | (0.0058) | (2.1345) | (0.0088) | (0.0046) | (1.0554) |
| post | -0.0095 | 0.0049 | -10.1000*** | 0.0224* | -0.00344 | -9.982*** |
| | (0.013) | (0.0095) | (3.33) | (0.0136) | (0.0071) | (1.8164) |
| Constant | 0.7193*** | 0.1355*** | 164.8166*** | 0.573*** | 0.112*** | 133.5*** |
| | (0.0198) | (0.0149) | (5.0514) | (0.0208) | (0.0121) | (3.1276) |
| Observations | 32'789 | 32'789 | 32'789 | 32'789 | 32'789 | 32'789 |
| Covariates | YES | YES | YES | YES | YES | YES |
| Age trends | YES | YES | YES | YES | YES | YES |
| regional FE | YES | YES | YES | YES | YES | YES |

*Notes:* Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The spells for the early treatment effects are censored at the time of the change of the PBD eligibility status (at reform date, April 1st 2011).
*Source:* UIR dataset

Table 6: Synthesis of the treatment effects across the three comparison cases. Effects for censored spells (unemployment exit before change) and full duration

| | | *Non-censored* unemployment exit | | *Censored* unemployment exit | |
|---|---|---|---|---|---|
| | | job | no job | job | no job |
| ***Case 1*** ***reference*** | *T: 200 days* *C: 400 days* | 0 | ↑↑ | 0 | ↑ |
| ***Case 2*** ***upgrade*** | *C: 200 days* *T: 200 → 400 days* | 0 | ↓↓ | 0 | 0 |
| ***Case 3*** ***downgrade*** | *T: 400 → 200 days* *C: 400 days* | ↑ | ↑↑ | ↑ | ↑ |

*Notes:* The table reports the diff-in-diff treatment effect coefficients (in percentage points) by means of arrows: ↑ = significant treatment effect of below .035, ↑↑ = significant treatment effect of above .035 and below .060. Coefficients are reported in Tables 3 to 5. The spells for the early treatment effects are censored at the time of the change of the PBD eligibility status (at 25th birthday in case 2; at reform date, April 1st 2011, in case 3). In reference case 1 the same censoring scheme than in case 2 is applied (by a mean-preserving uniform random spread; after 138 days in median), in order to allow for comparability across cases.