Labor Market Subsidy and Unemployment Exits*

Tomi Kyyrä†

28th January 2020

Abstract

Many countries have a two-tiered unemployment compensation system which provides earnings-related unemployment insurance (UI) for a limited period and some less generous unemployment benefit thereafter. In Finland, unemployed individuals who are not entitled to UI benefits can claim labor market subsidy. This paper studies the effects of this subsidy on unemployment exits. In 2012, the level of the subsidy was raised by 22%. The reform led to a drop of 9% in the unemployment exit rate, which was due to reductions in exits to both employment and non-participation. The implied elasticity of benefit duration with respect to the benefit level is about 0.4, which is less than previous elasticity estimates for unemployment insurance benefits.

Keywords: Unemployment benefit, labor market subsidy, hazard rate, unemployment duration.

JEL codes: J64, J68.

*I gratefully acknowledge research funding from the Economic Policy Council of Finland.

†VATT Institute for Economic Research, Helsinki; IZA Bonn; phone: +358 403045542; email: tomi.kyyra@vatt.fi

1 Introduction

Many European countries have adopted a two-tiered unemployment compensation system which provide earnings-related unemployment insurance (UI) for a limited period of time and some less generous unemployment benefit thereafter. In Finland, job losers who meet eligibility conditions regarding the past employment and contribution history are entitled to UI benefits up to 300, 400 or 500 days, depending on their age and employment history. Unemployed workers who exhaust their UI benefits and labor market entrants without sufficient employment and contribution history can claim labor market subsidy. This benefit is much lower than the average UI benefit, but it is available for an indefinite time.[1] Labor market subsidy recipients are a large and growing group: their share of all unemployment benefit recipients grew from 45% at the end of 2000 to 56% at the end of 2018 (Statistical Yearbook on Unemployment Protection in Finland 2018).

While a large body of literature has studied the effects of UI benefits,[2] much less is known about the effects of the second-tier unemployment benefits. It is quite possible that the effects of the two benefit types differ due to differences in the underlying populations. Unemployed workers on the second-tier benefits are either long-term unemployed or labor market entrants whose employment prospects are relatively weak compared to UI recipients with more recent work experience. They are also more likely to receive other means-tested income transfers such as housing allowance or social assistance. For these reasons, this group may be less responsive to changes in their unemployment benefits than UI recipients.

This paper studies the effect of labor market subsidy on unemployment exits using data for Finland. On January 1, 2012, the benefit level was raised by 22%, 121 Euros a month. The reform affected all unemployment benefit recipients, indirectly also those on earnings-related UI benefits. By implication, there is no unaffected group of the unemployed that could be used as a comparison group. However, since the reform also affected ongoing spells, I estimate the effect of the benefit level by comparing the

[1] In 2018, the average UI benefit was 61.83 Euros a day, while the average labor market subsidy was 34.67 Euros a day, 44% less (Statistical Yearbook on Unemployment Protection in Finland 2018).

[2] See Tatsiramos and van Ours (2014) and Schmieder and von Wachter (2016) for surveys.

unemployment exit hazards of labor market subsidy recipients before and after the day when the reform came into effect. To remove the seasonal component from the hazard rate, I use changes in the hazard rates at the beginning of 2011 when the benefit level did not change as a comparison period. The analysis is based on comprehensive data that combines information from different administrative registers and cover all labor market subsidy recipients.

The results point to a decline of 9% in the unemployment exit hazard due to the increase in the benefit level. This decline is mainly driven by lower hazard rates to employment and non-participation. There is also a small negative effect on the hazard rate to subsidized employment but the effect is not statistically insignificant at the 5% risk level. The overall decline in the exit hazard implies that the elasticity of benefit duration with respect to the benefit level among labor market subsidy recipients is 0.4.

According to the survey by Tatsiramos and van Ours (2014), the elasticity of non-employment duration with respect to the UI benefit level is typically between 0.4 and 1.[3] Uusitalo and Verho (2010) find an elasticity of 0.8 for UI recipients in Finland,[4] which is twice as large as our estimate for labor market subsidy recipients.[5] Our results suggest that also labor market subsidy recipients respond to benefit changes but their response is likely to be weaker than the response of UI recipients to the similar benefit changes. This finding is related to the literature on the optimal time profile of unemployment benefits over the unemployment spell. The two-tier unemployment compensation system

[3] The median of the elasticity estimates for UI recipients in 18 studies summarized by Schmieder and von Wachter (2016) is 0.53. 11 of these estimates are from the United States, and the median of the US estimates is 0.38.

[4] They analyzed a reform in 2013 that abolished the severance pay and increased the UI benefits of unemployed workers with long work histories for the first 150 days of unemployment.

[5] It is should be stressed that Uusitalo and Verho (2010) and most other studies estimate the effect of UI benefits on the job finding hazard or directly on non-employment duration (the time to the next job). This focus is understandable as a great majority of UI recipients eventually find a new job and therefore non-employment duration and unemployment duration are very closely related for them. However, this is not the case with labor market subsidy recipients because only one-fourth of them leave benefits by moving into employment in the open labor market. As a consequence, the unemployment duration of labor market subsidy recipients depends in large part also on hazard rates to non-participation and subsidized employment, not only on the job finding hazard (i.e. exits to employment in the open labor market without wage subsidies). Therefore, I compare the effect of labor market subsidy on the unemployment exit rate, which determines the effect on the duration of labor market subsidy receipt, to the effect of UI benefits on the job finding rate or non-employment duration, which gives an approximate effect on the duration of UI receipt.

produces a declining profile as the benefit level drops at the time when the UI benefits expire. Most of the existing literature on the optimal time profile has been theoretical, and the results from these studies have been mixed: declining, increasing, flat and hump-shaped profiles have all been found to be optimal in different studies. Given this ambiguity in the theoretical work, there is an obvious need for empirical evidence. However, only two empirical studies have addressed the issue.

Kolsrud et al. (2018) analyze how individuals respond to changes in UI benefits at different phases in the unemployment spell. At the time of their analysis, the unemployed in Sweden could receive UI benefits for unlimited time. However, the benefit level varied over the course of the unemployment spell due to duration-dependent caps, which were subject to changes over time due to reforms. Using this variation for identification, Kolsrud et al. (2018) show that unemployed individuals respond more strongly to changes in their UI benefits paid earlier in the unemployment spell than to the benefits paid later in the spell. They also provide evidence that the consumption smoothing value of UI is higher for the long-term unemployed. Together these findings suggest that the declining benefit profile is not optimal but a flat or increasing benefit profile might be more desirable.

Linder and Balázs (2020) came to the opposite conclusion by examining a reform in Hungary that raised the UI benefit early in the unemployment spell and cut it later in the spell by the same amount, so that the total amount of the UI benefits the new benefit claimant was entitled to remained (approximately) constant. This kind of benefit frontloading makes short-term unemployed better off without affecting the UI benefits received by long-term unemployed, and thereby increases the average benefit generosity. Linder and Balázs (2020) find that, as a result of frontloading, unemployment spells shortened and re-employment wages rose. Moreover, these behavioral changes were large enough to offset the mechanical increase in the costs of the change in the benefit profile. Since both unemployed and employed workers were made better off — the unemployed because of higher average benefits and the employed because of lower tax burden — the results give strong support for the declining benefit profile.

The rest of the paper proceeds as follows. The next section describes relevant institutions and the 2012 reform. The following section discusses the research design. The subsequent sections describe data and sample selection, and discuss macroeconomic conditions at the time of the reform. These are followed by a section presenting the results. The final section concludes.

2 Institutional setting

Our observation period covers the years 2009–2014. At that time the entitlement period of earnings-related UI benefits was 500 payment days, about two calendar year as the benefits are paid for five days a week.[6] The UI benefits are paid by unemployment funds. Membership in these funds is voluntary but as many as 90% of employed workers were members in 2015. Fund members who loose their job are awarded a new entitlement period of the UI benefits provided that they register as an unemployed job seeker at the public employment service and satisfy certain eligibility conditions regarding employment and contribution history. The level of the UI benefit has no cap but the replacement rate declines with the level of past earnings.

Unemployed who are not entitled to UI benefits can claim labor market subsidy, which is paid by the Social Insurance Institution.[7] The subsidy is means tested against certain other benefits and spouse's income (up until 2012) but, as long as the eligibility conditions are met, it can be received for an indefinite period. At the beginning of each year, the level of the labor market subsidy is adjusted according to an index measuring the inflation. In 2011, the subsidy was 25.74 Euros a day, without child supplements. On January 1, 2012, it was raised by 21.8% to 31.36 Euros a day.[8] The increase in the monthly benefits was 121 Euros, of which 100 Euros was due to the reform and 21 Euros due to the annual

[6] In 2014, the maximum duration of UI benefits was reduced to 400 for workers whose employment history is shorter than three years. This change only affected new UI spells. In 2017, the maximum duration was cut by 100 days for all workers except for those who are at least 58 years old and have sufficiently long employment history.

[7] Unemployed who do not belong to an unemployment fund but satisfy the employment-history condition are eligible for a basic unemployment allowance, which is paid up to 500 days without means testing. The level of the basic unemployment allowance is the same as for the labor market subsidy.

[8] The regular index increase for 2012 would have been 3.8%.

index adjustment. At the same time also the income limits for housing allowance were adjusted in such a way that the benefit hike did not affected the amount of the housing allowance the labor market subsidy recipient was possibly entitled to. In most cases, the 2012 reform therefore probably raised the income of labor market subsidy recipients by the full amount of the benefit increase. According to the government's law proposal to the parliament, the aim of the reform was to reduce poverty and marginalization among labor market subsidy recipients, and minimize income inequality.

Other changes in the labor market subsidy and related benefits in our observation period were moderate. In 2013, the subsidy was raised by 3.5%, but in 2010, 2011 and 2014 the benefit changes were less than 1% due to the low inflation rate (see table 1). In 2010, training subsidies that were paid for the duration of labor market and certain education programs were abolished. Since this reform, program participants have been receiving UI benefits or labor market subsidy. On December 15, 2010, the means testing was slightly relaxed by increasing the threshold above which the spouse's income reduces the labor market subsidy. Since January 1, 2013, the spouse' income has not affected the amount of the labor market subsidy anymore. At the beginning of 2013, two other changes took place. First, an employment bonus experiment started in 60 municipalities. In these municipalities labor market subsidy recipients were allowed to keep their subsidy for one extra month if they took up a new job with the duration of at least three months. Second, new labor market programs were introduced.

In the main analysis, I model unemployment exits among labor market subsidy recipients who had no children, whose benefits were not reduced due to means testing or other benefits and who were unemployed close to the turn of the years 2011 and 2012. For this group of labor market subsidy recipient during the considered periods, there were no other notable changes than the 22% increase in the benefit level at the beginning of 2012. As a robustness check, I also provide evidence on the changes in unemployment exits in other years, yet one should keep in mind that these results may be affected by other reforms listed above, especially at the beginning of 2013.

3 Research design

Since the UI benefit is defined as the sum of the basic component equal to labor market subsidy plus the earnings-related supplement, the 2012 reform did not only increase the subsidy but also UI benefits and, therefore, all unemployment benefit recipients were affected by the reform. Due to the lack of a comparison group not affected by the 2012 reform, I have to rely on time series variation in the unemployment exit hazard around the day of the benefit hike. This approach is motivated by the fact that the reform affected all ongoing benefit spells from January 1, 2012 onward.

We could compare the hazard rate in the last week of 2011 to the hazard rate in the first week of 2012 but there are some confounding factors that would bias such a comparison. First, the flows into and out of labor market subsidy are subject to seasonal variation. New jobs (and education programs) often start at the beginning of the month, whereas old jobs typically terminate at the end of the month. Therefore, the unemployment exit hazard is elevated in the end of each month while the composition of the unemployed may change at the beginning of the month. Moreover, at the turn of the year, this kind of seasonal variation can be pronounced due to the Christmas and New Year. Second, in anticipation of the forthcoming benefit increase, some unemployed may have reduced their search effort and/or increased reservation wages at the end of 2011, in which case the unemployment exit hazard may have been abnormally low just prior to the reform. Third, since the unemployment benefits are paid in four-week or one-month periods, and the benefits are claimed afterwards, the higher benefits did not show up in individuals' bank account on January 1, 2012, but on some later day in January or even in the next month, depending on an individual-specific payment schedule. Thus, unemployed workers who were not aware of the reform may have noticed the benefit increase with some delay, in which case the unemployment hazard may have have been "too high" just after the reform.

Because of these reasons, the effect of the reform cannot be identified from a discrete change in the hazard rate on January 1, 2012. Instead, I will compare the average hazard rates before and after the benefit increase within the 13-week window around the first week of 2012. Due to the possible anticipation effects and delayed responses, it might be advisable to ignore the observations in near vicinity of the reform day, yet it is not obvious which time interval should be excluded from the analysis. On the one hand, a longer time interval for excluded observations around the reform day eliminates more surely the confounding effects due to anticipatory behavior and delays in the benefit payments. On the other, when the exit hazards far away from each other are compared, the hazard rates are more likely to differ due to the seasonal variation and business cycle conditions. The seasonal component of the hazard rates can be estimated by comparing the hazards rates around the turn of 2011 when the benefit level remained nearly constant. Although the resulting difference-in-differences (DID) approach eliminates the common seasonal component, it does not eliminate possible changes in the business cycle conditions within the time window used in the analysis. To mitigate this concern I mainly use the window of 13 weeks in the analysis. As robustness checks, I also use a shorter window of 9 weeks as well as a "donut-hole" sample that excludes observations within 4 weeks of the reform day.

4 Data

The registers of the Social Insurance Institution provide detailed information on paid benefits and some background characteristics for benefit recipients. These data are complemented by merging information on active labor market programs and employment spells from the registers of Ministry of Economic Affairs and Employment and of the Finnish Centre for Pensions. This supplementary information is used to detect exits to job placement programs (i.e. subsidized employment) and employment in the open labor market. Unemployment spells are defined as the time the worker receives labor market subsidy, allowing for short breaks of four weeks or less in benefit receipt within the spell. That is, I do not consider the time spent on UI benefits before receipt of labor market subsidy. Individuals who take up a new job that starts within four weeks from the end of the benefit period and lasts for at least four weeks are classified to be exited to employment.

I restrict the analysis to labor market subsidy recipients aged 25 to 60 with no children, and consider their unemployment spells that were ongoing in the period 2009–2014. I drop spells during which the claimant's benefit were reduced due to means testing or receipt of other benefits. The resulting sample contains 195,629 individuals, of whom 54,625 were unemployed at the time of the 2012 reform. Given the sample restrictions, the benefit level of these individuals increased by 21.8% in January 2012.

I model unemployment duration by means of weekly exit hazards. The number of unemployed during a calendar week varies between 45,805 and 77,113, with an average of 57,032 individuals over the period 2009–2014. Sample members are on average 41 years old, and 44% of them are female. The average duration of benefit spells (i.e. labor market subsidy spells) that ended in the observation period is 12.3 months.

5 Economic environment

Unfortunately, our observation period was rather turbulent. In 2009, the gross domestic product plunged by 8.1% due to the global financial crisis. In 2010 and 2011, the economy grew by 3.1% and 2.5%, respectively, but then turned on a declining path again. As seen in figure 1, the volume of production remained was rather stable in 2011 but started to shrink at the beginning of 2012, that is, at the time when the reform came into effect. This poses a potential problem for our evaluation task due to the lack of a comparison group not affected by the reform.

From figure 2 we see that the number of UI recipients follows the aggregate growth rates. The labor force share of UI recipients declined in 2010 and 2011 but turned on an increasing path at the beginning of 2012. For labor market subsidy recipients the pattern is quite different. Except for a short decline in the first half of 2010, the labor force share of labor market subsidy recipients increases smoothly over the observation period. In particular, the turn in the economic environment in 2012 has no visible impact on the trend of labor market subsidy recipients.

Figure 3 shows the flow into labor market subsidies, i.e. the number of new benefit spells, in our sample. Apart from seasonal variation, the inflow increases smoothly up to the end of 2013, after which it stabilizes. There are no large seasonal variation in the inflow. Importantly, we do not see evidence of excess inflow to labor market subsidy at the time of the 2012 reform due to anticipatory behavior. This is not very surprising, as many new labor market subsidy recipients are unemployed whose UI benefits expired and, thus, they have been unemployed for the two preceding years.

It appears that the stock of labor market subsidy recipients is less responsive to changes in the business cycle conditions than that of UI recipients. Yet this does not remove the problem that the economic environment changed at the time when the benefit increase came into effect.

6 Empirical analysis

6.1 Hazard functions

I consider weekly hazard rates for unemployment exits, as well as for exits to employment, subsidized employment and non-participation. The sum of the last three hazards equals the overall exit hazard, so that they provide a useful decomposition for the overall exit hazard. The weekly hazards exhibit a large degree of variation across months within years as well as across weeks within months. As a result, long series of weekly hazards are very noisy and therefore difficult to interpret. To ease the detection of breaks in the trends of the hazard rates, instead of weekly hazards, figure 4 shows seasonally adjusted average weekly hazards. The seasonal variation was removed as follows: first, weekly exits were regressed against calendar month dummies, using individual-level data and linear probability model; second, the mean of the residuals from this regression for each month was computed; and, finally the average exit rate over the years 2009–2014 was added to the mean residuals. Thus, the curves show the average weekly hazard rates during a given month, net of systematic seasonal variation across calendar months.

In the period 2009–2011, the seasonally adjusted unemployment exit hazard varies around 0.015, suggesting that on average 1.5% of individuals receiving labor market subsidy on Monday exited from unemployment by the end of the week. From the other hazard rates we see that about one-half of them left the labor force, roughly one-quarter found a new job in the open labor market, and one-quarter started a subsidized job. That is, unlike for UI recipients, entering employment is not the main reason of unemployment exit. While the job finding hazard is increasing and other hazards are relative stable until the end of 2011, all the hazards decline smoothly over the later part of the observation period, reflecting the change in the business cycle conditions. However, we are mainly interested in changes at the time of the reform. The unemployment exit hazard evolves smoothly at the beginning of 2010, 2011, 2013 and 2014, but shows a clear, discrete drop in January 2012, which is consistent with the hypothesis that the 2012 reform reduced exits from unemployment among labor market subsidy recipients. A similar yet less clear drop can also be seen in the hazards to non-participation and subsidized employment. The evolution of the job finding hazard at the time of the reform is more difficult to interpret as the hazard rate started to decline already in the last weeks of 2011, possibly due to anticipation of the forthcoming benefit hike.

In the subsequent analyses, I focus on changes in the weekly hazard rates around the turn of the years. Figures 5–7 show the unadjusted hazards within the 13-week window from the first week of a given year (= the first calendar week starting in January), which corresponds to the value of 0 on the horizontal axis.[9] Note that all the hazard rates are multiplied by 100. Figure 5 compares changes in the hazard rates around the first weeks of 2010 and 2011, that is, over the two periods when the benefit level remained roughly constant. The hazard rates are almost overlapping and marginally higher hazard rates around the first week of 2011 are due to better business cycle conditions. More importantly, changes in the hazard rates from the last 13 weeks of the year to the first 13 weeks of the next year are similar in the two periods: the increase in the average unemployment exit hazard is 0.092 (7.5%) and 0.125 (9.6%) at the turn of the years 2010 and 2011 respectively. This roughly 0.10 increase in the hazard rate at the beginning of

[9]The first week begins on the 4th, 3rd, 2nd, 7th and 6th day in January 2010, 2011, 2012, 2013 and 2014 respectively.

the year describes the seasonal variation in unemployment exits, which we need to take into account when estimating the effect of the benefit hike.

In figure 6, the unemployment exit hazard remains stable from the last weeks of 2011 to the first weeks of 2012 (a decrease of 0.003 or 0.2%), whereas the unemployment exit hazard increase by 0.125 (9.6%) from the last weeks of 2010 to the first weeks of 2011. Thus, unlike at the beginning of the two previous years, the unemployment exit hazard does not increase at the beginning of 2012. This finding is consistent with the hypothesis that the benefit hike in January 2012 reduced unemployment exits.

Figure 7 compares the hazards in the two periods after the 2012 reform. The hazard rates in these periods are highly similar. The increase in the unemployment exit hazard from the last 13 weeks of 2013 to the 13 first weeks of 2014 is 0.068 (7.0%), which is very close to the changes at the beginning of 2010 and 2011. However, at the beginning of 2013 the unemployment exit hazard increases somewhat less, by 0.038 (3.6%). The smaller increase over the first weeks of 2013 may not come as a surprise because the benefit level was raised by 3.5% in January 2013 (see table 1). Also the employment bonus experiment and the introduction of new labor market programs at the beginning of 2013 may have affected the difference in the hazard rates between the last weeks of 2012 and the first weeks of 2013. Because of these changes, the time interval around the first week of 2013 is a less valid comparison period than other considered periods.

6.2 Difference-in-differences estimates

Next, I test statistical significance of the hazard changes between the year pairs in figures 5–7, using a simple difference-in-differences model. To assess whether the benefit hike in 2012 affected the hazard rates I compare changes in the hazard rate at the beginning of 2012 to changes at the beginning of 2011 within the 13-week windows. The difference between these changes eliminates the effect of the seasonal variation around the turn of the years (the systematic difference between the last 13 weeks of the year and the first 13 weeks of the next year) but not the effect of differently changing economic conditions within the time window around the turn of the year between different periods (between the 26-week time interval around the first week 2011 and the 26-week time interval around the first week of 2012). To mitigate the effect of the business cycle conditions I use only observations that are at most 13 weeks away from the turn of the year. I estimate the following equation:

$$Y_{it} = \alpha + \beta Post_{it} + \gamma Reform_{it} + \delta_{DID} \left(Reform_{it} \times Post_{it} \right) + \varepsilon_{it},$$

where i indexes individual and t, $-13 \leq t \leq 12$, denotes the calendar week since the first week of the relevant year. $Y_{it} = 1$ if individual i who was unemployed at the beginning of week t left unemployment by the end of the week, and $Y_{it} = 0$ otherwise. $Post_{it} = 1\{t \geq 0\}$ is a dummy variable for the first weeks of the year. $Reform_{it}$ is a dummy for the period during which the reform took place (i.e. the weeks around the turn of 2012).

It follows that α is the average hazard rate at the end of 2010, and β captures its change at the beginning of 2011, i.e. the difference in the average hazards between the last 13 weeks of 2010 and the first 13 weeks of 2011. That is, β measures the seasonal component of the hazard rate, which is assumed to be the same in the comparison and reform periods. γ is the general difference in the hazard rates between the reform and comparison periods, for example, due to different business cycle conditions. The parameter of interest is δ_{DID}, which is equal to the change in the hazard rate at the beginning of 2012, net of the seasonal component. Under the assumptions that the seasonal variation is constant across years and that the economic environment evolved similarly *within* the 13-week windows around the turns of 2011 and 2012, this parameter identifies the causal effect of the benefit increase. It is worth emphasizing that the economic environment, and thus the level of the hazard rates, can be different between the two periods, as captured by γ, but divergent changes in the business cycle conditions within the considered time intervals between the comparison and reform periods are ruled out by assumption. This is a rather strong assumption in the light of the evidence presented in section 5. However, the possible bias due to divergent pattern of the business cycle conditions is likely to be small when the short windows are used.

As a robustness check, I also estimate the model using data around the turns of 2010 and 2011 as well as data around the turns of 2013 and 2014, treating the latter period as a placebo reform period. In these cases, the estimate of γ_{DID} provides a simple test for the underlying assumptions of the DID setting; it should be close to zero if the seasonal component is roughly constant across years and the business cycle conditions do not change differently within the 13-week windows around the turn of different years.

The point estimates of δ_{DID} along with their 95 % confidence intervals are shown in figure 8. The effect of the benefit increase on the unemployment exit rate is -0.1275, which corresponds to a decline of 9% from the counterfactual hazard rate of 1.4217 for the first 13 weeks of 2012. Since the benefit level was raised by 22%, the implied elasticity of the unemployment exit hazard with respect to labor market subsidy is 0.4. As the hazard rate is constant, also the elasticity of benefit duration (i.e. the duration of labor market subsidy receipt) with respect to the benefit level is 0.4.

The decline in unemployment exits after the benefit increase in 2012 is mainly due to lower hazard rates to employment and non-participation. There is also a small negative effect on the hazard rate to subsidized employment, but the effect is statistically significant only at the 10% level (p-value 0.0765). The estimated effects are generally rather imprecise, even though the number of worker-week observations is as large as 2.7 million. This is because the relevant variation in the underlying data takes places at the hazard level and our estimation sample only covers two periods, both of which contain 26 distinct hazard values. Due to the wide and overlapping confidence intervals, we cannot really say whether the effects of the benefit hike on the hazard rates to employment, job placement programs and non-participation differ from each others.

It should be stressed the estimated effects measure *absolute* changes in the underlying hazard rates. This is convenient as the absolute changes in the cause-specific hazards add up to the change in the unemployment exit hazard, providing a useful decomposition for the overall effect. Even though the estimated absolute changes in the cause-specific hazards are on the same ballpark, they do imply different *relative* changes due to differences in the levels of the underlying counterfactual hazards. Namely, the estimates correspond to a decline of 14% in the job finding rate and declines of 7% in the hazards to subsidized employment and non-participation. It follows that the elasticity of the job finding rate with respect to labor market subsidy is 0.6, which is twice as large as the elasticities of the other two cause-specific hazards rates, 0.3.

In figure 8, the placebo effects on the unemployment exit hazard obtained from the periods 2010–2011 and 2013–2014 do not differ from zero at the 5% risk level. Except the effect on the non-participation hazard from the year pair of 2013 and 2014, also the placebo effects on the cause-specific hazards are close to zero and statistically insignificant. Recall that the time around the turn of 2013 is a problematic period due to the benefit increase of 3.5% and certain other reforms in January 2013. That said, the results of the placebo reforms are reassuring as they give support for the validity of our research setting.

Table 2 reports the results of additional robustness checks. Our baseline DID estimates (the red points in figure 8) are shown on the top of the table. These are followed by DID estimates obtained from two different model specification and from two different estimation samples. First, I add a set of control variables to the analysis. These include the elapsed duration of the ongoing unemployment spell (9 categories), gender, age (7 categories), region (19 provinces), the log of the municipal unemployment rate, and a dummy for the last week of the month. Second, I allow distinct linear trends in the hazard rates over the comparison and reform periods. This specification relaxes the assumption that the business cycle conditions did not change differently within the 13-week windows around the turns of 2011 and 2012. Third, I exclude the last four weeks of 2011 and the first five weeks of 2012 from the estimation sample. Estimates from this "donout-hole" sample should be less affected by possible anticipatory behavior or delayed responses. Finally, I exclude the furthermost observations from the analysis using a shorter window of 9 weeks around the turns of the years. These estimates should be less sensitive with respect to possible asymmetric changes in the business cycle conditions around the turn of the two years (yet more sensitive with respect to possible anticipatory behavior and delayed responses). As seen in table 2, all of these estimates are rather close to the corresponding baseline estimate on the top of the table. This finding together with the small and mainly insignificant placebo effects in figure 8 show that the results are reasonably robust. In particular, since controlling for the local unemployment rate, allowing for period-specific linear trends and using a shorter time window do not change the results, it seems that our estimates are not significantly biased due to differently changing business cycle conditions within the two time intervals.

DID estimates by gender, age and elapsed unemployment duration are reported in table 3. The overall effect of the benefit hike on unemployment exits is roughly the same for women and men. However, whereas one-half of the overall effect among men stems from the reduction in the job finding rate, women's job finding rate is hardly affected as the decline in unemployment exits comes from lower hazards to subsidized employment and non-participation. For workers between the ages of 20 and 34 the only effect is a reduction of the job finding rate. In the older groups, the overall effect on unemployment exits is larger while the drop of the job finding rate plays smaller role. Among the oldest workers, the change in the job finding rate explains less than one-fifth of the overall effect on the unemployment exit hazard. The smaller role of the changes in the job finding rate among older workers is partly due to a lower counterfactual level of the job finding hazard (not reported in the table). On the other hand, the counterfactual level of the unemployment exit hazard declines with age. Taken together with the larger absolute overall effect on unemployment exits for the older group this suggest that older workers respond more strongly to the level of labor market subsidy.

The results by the elapsed duration of unemployment are surprising. For individuals who have been on labor market subsidy for 6–24 months the overall effect on unemployment exits is very small and does not differ from zero, while much larger effects are found for those who have been benefit recipient for shorter or longer time. It is also surprising that one-half of the overall effect of the long-term unemployed can be attributed to the decline in the job finding rate.

7 Concluding remarks

According to the results, the unemployment exit hazard declined by 9% at the beginning of 2012 following the benefit hike. Taken together with the benefit increase of 22% this suggests that the unemployment duration elasticity with respect to labor market subsidy is about 0.4. The overall effect on unemployment exits can be decomposed into the effects on the job finding hazard, subsidized employment hazard, and non-participation hazard. In absolute terms, the effects on these cause-specific hazards are roughly of the same magnitude. However, since exits to employment are relatively rare among labor market subsidy recipients, the implied elasticity of the job finding hazard is 0.6, which is twice as large as the elasticities of the subsidized employment and non-participation hazards. All in all, the labor market subsidy recipients responded to the benefit increase by exiting from unemployment to all considered destinations at lower rates.

These results are based on the simple difference-in-differences setting where the changes in the hazard rates at the beginning of 2012 are compared to the changes at the beginning of 2011. The approach should eliminate the seasonal variation in the hazard rates but not asymmetric changes in the business cycle conditions between the periods. This is a matter of concern as aggregate unemployment turned on a growing path at the time of the reform, which may bias our estimates upward. Although our results proved to be robust with respect to various deviations from the basic setup, the estimates should be interpreted as an upper bound for the true effect.

Compared to the previous evidence of the effect of UI benefits, the estimated effects of labor market subsidy, especially if they are biased upward, are relatively small. This implies that benefit increases lead to relatively small undesired behavioral responses among labor market subsidy recipients.[10] Analogously, benefit cuts are unlikely to lead to large employment effects among this group.

[10] Of course, forward-looking UI recipients may respond to changes in labor market subsidy, even when the UI benefits remain fixed.

References

Kolsrud, J., Landais, C., Nilsson, P. and Spinnewijn, J.(2018). The Optimal Timing of Unemployment Benefits: Theory and Evidence from Sweden. *American Economic Review*, 108(4-5): 985–1033.

Lindner, A. and Balázs, R. (2020). Frontloading the Unemployment Benefit: An Empirical Assessment. Forthcoming in *American Economic Journal: Applied Economics*.

Price, B. M. (2019). The Duration and Wage Effects of Long-Term Unemployment Benefits: Evidence from Germany's Hartz IV Reform, unpublished manuscript.

Schmieder, J. F. and von Wachter, T. (2016). The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation. *Annual Review of Economics*, 8(1):547–581.

Statistical Yearbook on Unemployment Protection in Finland 2018. www.kela.fi/statistics.

Tatsiramos, K. and van Ours, J. C. (2014). Labor Market Effects of Unemployment Insurance Design. J*ournal of Economic Surveys*, 28(2):284–311.

Uusitalo, R. and Verho, J. (2010). The Effect of Unemployment Benefits on Re-employment Rates: Evidence from the Finnish Unemployment Insurance Reform. *Labour Economics*, 17(4), 643-654.

Table 1: Benefits in 2009–2014

			Child supplements		
Year	Daily benefit	Change	1 child	2 children	3+ children
2009	25.63		4.86	7.13	9.19
2010	25.63	0%	4.86	7.13	9.19
2011	25.74	0.43%	4.88	7.16	9.23
2012	31.36	21.83%	5.06	7.43	9.58
2013	32.46	3.51%	5.24	7.69	9.92
2014	32.66	0.62%	5.27	7.74	9.98

Table 2: Robustness of estimated treatment effect (×100)

			Hazard rates to		
	Number of observations	Unemployment exit hazard	Employment	Subsidized employment	Non-participation
	(1)	(2)	(3)	(4)	(5)
Counterfactual hazard (×100)		1.4217	0.3619	0.3175	0.7423
Baseline estimate	2,704,254	-0.1275***	-0.0496***	-0.0235*	-0.0545***
		(0.0272)	(0.0127)	(0.0133)	(0.0205)
Control variables	2,704,254	-0.1095***	-0.0444***	-0.0212	-0.0439**
		(0.0276)	(0.0127)	(0.0133)	(0.0207)
Linear trend	2,704,254	-0.1414**	-0.0304	-0.0178	-0.0932**
		(0.0556)	(0.0250)	(0.0270)	(0.0423)
Donut-hole	1,765,964	-0.1401***	-0.0630***	-0.0326*	-0.0446*
		(0.0339)	(0.0165)	(0.0167)	(0.0251)
Shorter window	1,873,712	-0.1274***	-0.0409***	-0.0248	-0.0618**
		(0.0323)	(0.0147)	(0.0155)	(0.0247)

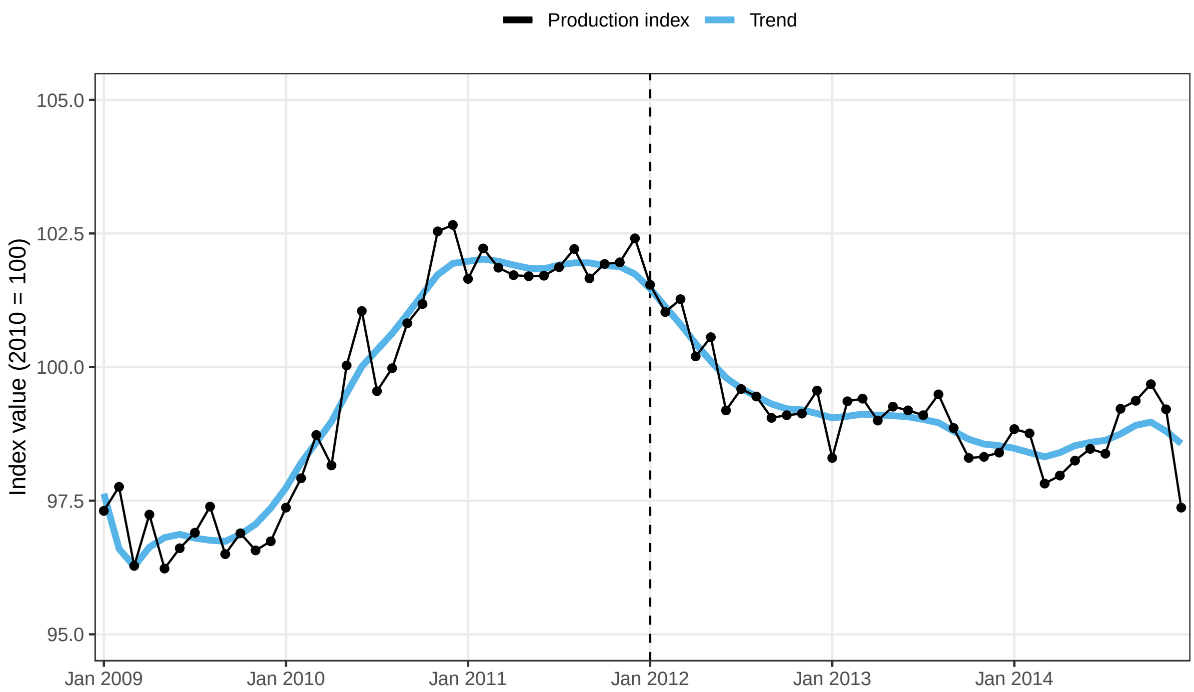
Notes: The estimating sample contains observations within the 13-week window from the turn of 2011 and 2012 (the last specification uses a narrower window). The number of observations refers to the number of worker-week observations, i.e. the total number of workers at risk of exiting from unemployment over the period used in the estimations. The counterfactual hazard is the predicted average hazard over the first 13 weeks of 2012 in the absence of the reform ($= \alpha + \beta + \gamma$) based on our baseline specification. The first estimates are from our baseline model specified in equation (1). The same estimates are also illustrated in figure 8. The second set of the estimates are from an otherwise similar model but with controls for the elapsed duration of unemployment, gender, age, region, local unemployment rate and a dummy for the last week of the month. The subsequent estimates are from a model (without controls) that allows for distinct linear trends over the reform and comparison periods. These are followed by the estimated from a donut-hole sample, excluding closest 4 weeks from the first week of the year. The last set of the estimates were obtained using a shorter window of 9 weeks around the turn of the year. The standard errors clustered at the individual level in parentheses. Significance levels: *** 1%, ** 5% and * 10%.

Table 3: Estimated treatment effects ($\times 100$) by group

	Number of observations	Unemployment exit hazard	Hazard rates to		
			Employment	Subsidized employment	Non-participation
	(1)	(2)	(3)	(4)	(5)
Baseline estimate	2,704,254	-0.1275***	-0.0496***	-0.0235*	-0.0545***
		(0.0272)	(0.0127)	(0.0133)	(0.0205)
Women	1,037,664	-0.1145**	-0.0127	-0.0427*	-0.0591*
		(0.0469)	(0.0216)	(0.0241)	(0.0347)
Men	1,666,590	-0.1366***	-0.0726***	-0.0120	-0.0520**
		(0.0330)	(0.0155)	(0.0154)	(0.0253)
Age 20–34	699,351	-0.0761	-0.0825**	0.0037	0.0028
		(0.0624)	(0.0340)	(0.0244)	(0.0474)
Age 35–49	833,608	-0.1336***	-0.0533**	-0.0016	-0.0787**
		(0.0478)	(0.0216)	(0.0238)	(0.0360)
Age 50–60	1,171,295	-0.1545***	-0.0289**	-0.0550***	-0.0705**
		(0.0374)	(0.0144)	(0.0209)	(0.0281)
Unemployed $<$ 6 months	834,750	-0.2564***	-0.0843**	-0.0774**	-0.0948**
		(0.0634)	(0.0329)	(0.0308)	(0.0458)
Unemployed 6–24 months	950,538	-0.0184	-0.0129	0.0097	-0.0152
		(0.0445)	(0.0198)	(0.0215)	(0.0339)
Unemployed $\geq$ 24 months	918,966	-0.1071***	-0.0532***	-0.0075	-0.0464*
		(0.0332)	(0.0102)	(0.0159)	(0.0275)

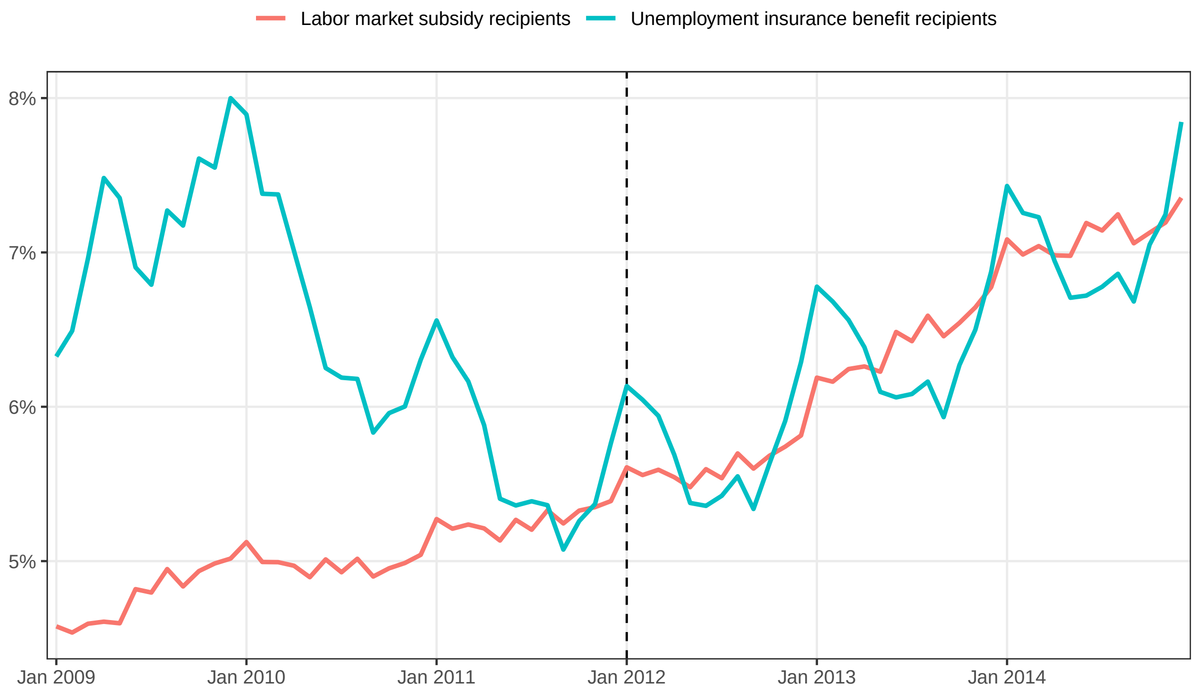
Notes: The estimating sample contains observations within the 13-week window from the turn of 2011 and 2012. The number of observations refers to the number of worker-week observations, i.e. the total number of workers at risk of exiting from unemployment over the period used in the estimations. All estimates are obtained from our baseline model specified in equation (1). The standard errors clustered at the individual level in parentheses. Significance levels: *** 1%, ** 5% and * 10%.

Figure 1: Seasonally and working day adjusted volume of production



Source: Statistics Finland.

Figure 2: Labor force share of unemployment insurance and labor market subsidy recipients by month



Notes: The numbers of labor market subsidy and unemployment insurance benefit recipients are based on data obtained from the benefit registers of Social Insurance Institution and Financial Supervisory Authority, respectively. These numbers are then divided by the size of the labor force obtained from the Labor Force Survey of Statistics Finland.

Figure 3: The number of new labor market subsidy recipients in the estimation sample by calendar month

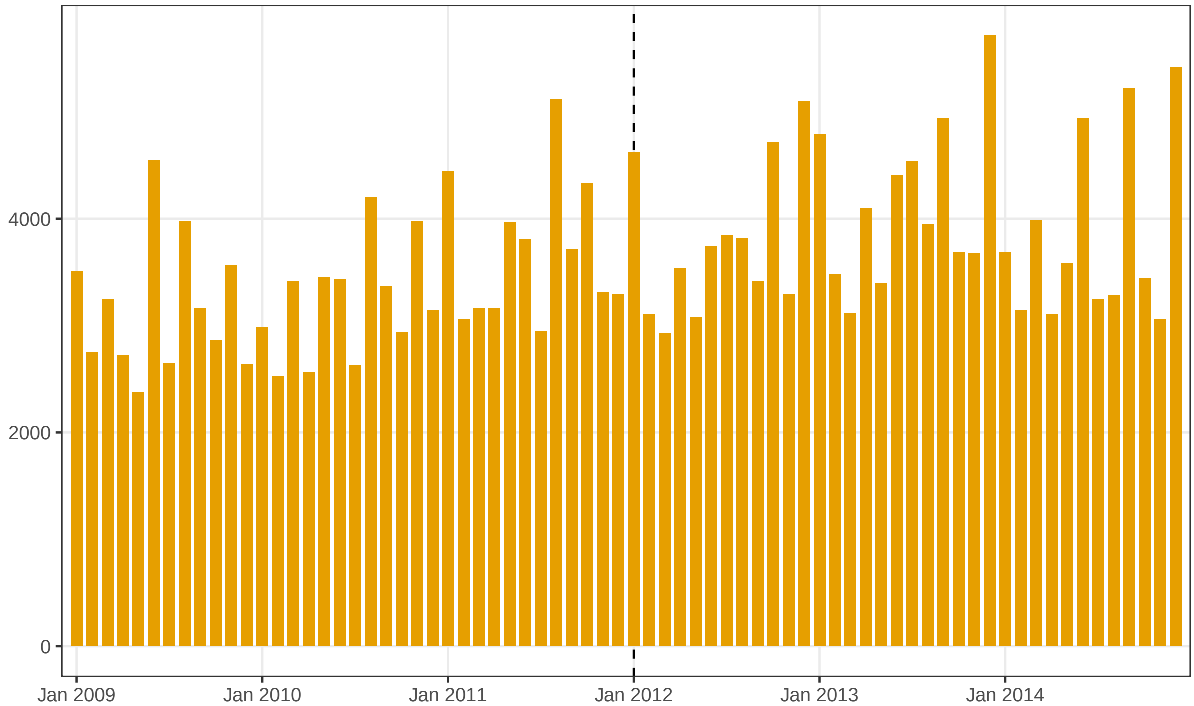
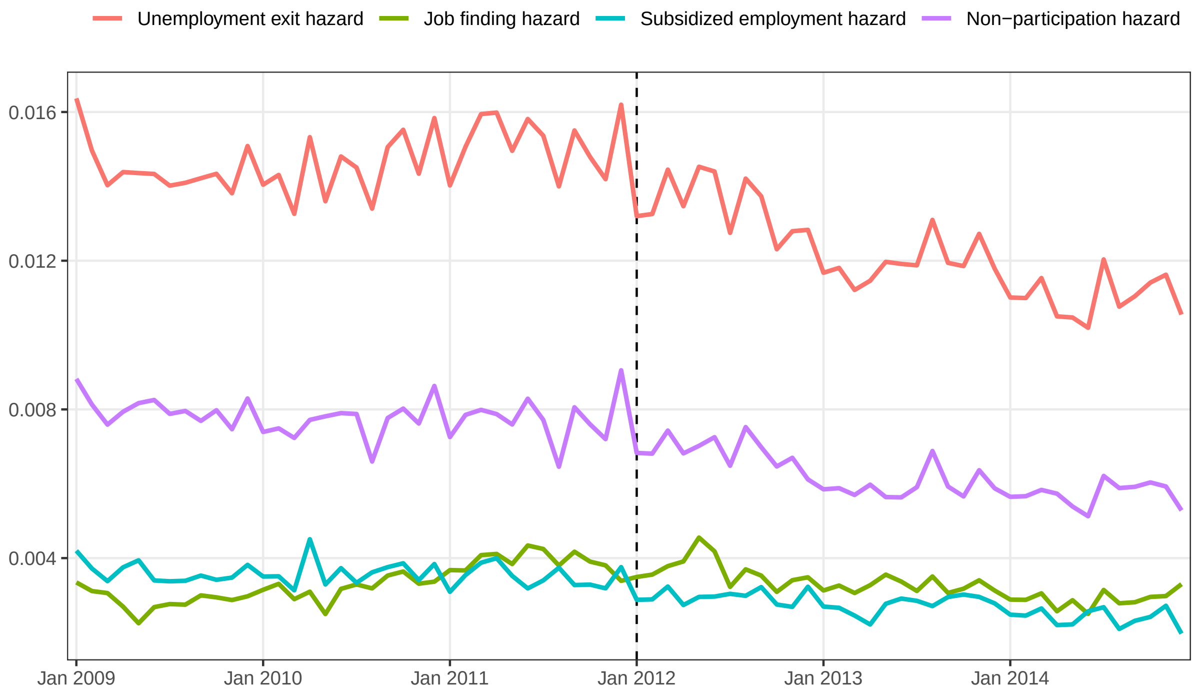


Figure 4: Seasonally adjusted average weekly hazards by month



Notes: The adjusted hazard equals the mean of residuals for a given month from the regression of weekly exits on calendar month dummies, plus the average weekly hazard rate over the whole observation period

Figure 5: Hazard rates around the time of the turn of years 2010 and 2011

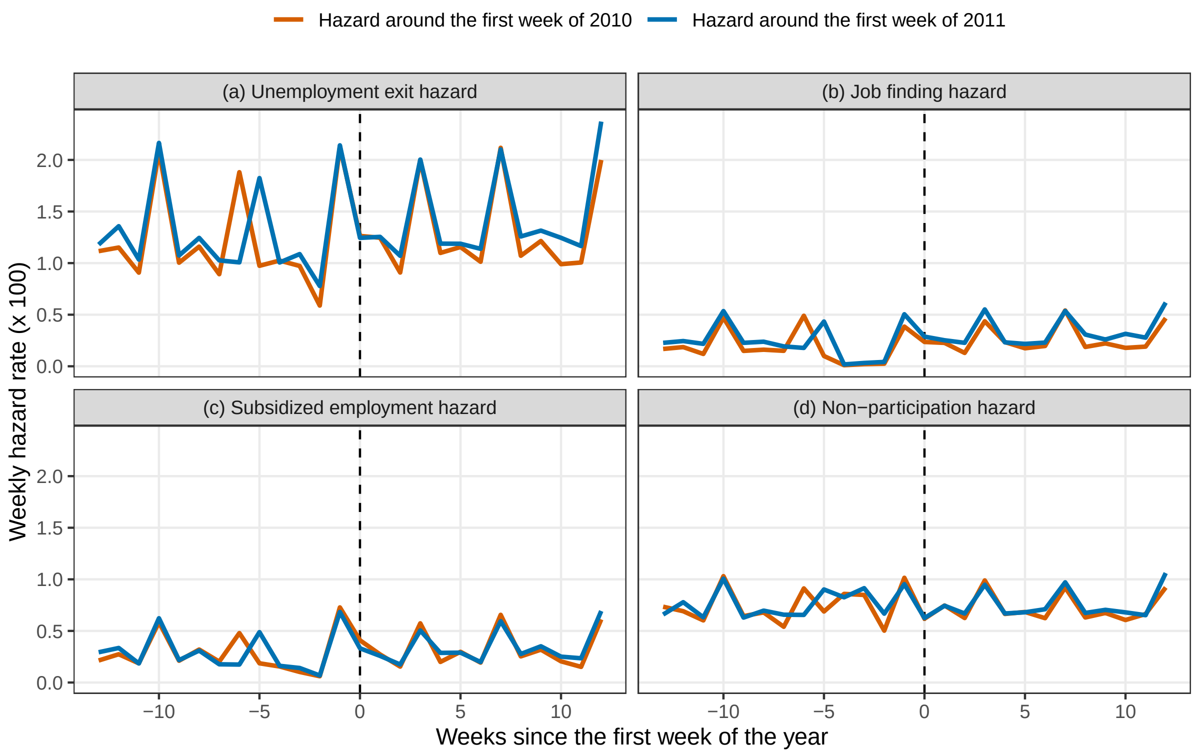


Figure 6: Hazard rates around the time of the turn of years 2011 and 2012

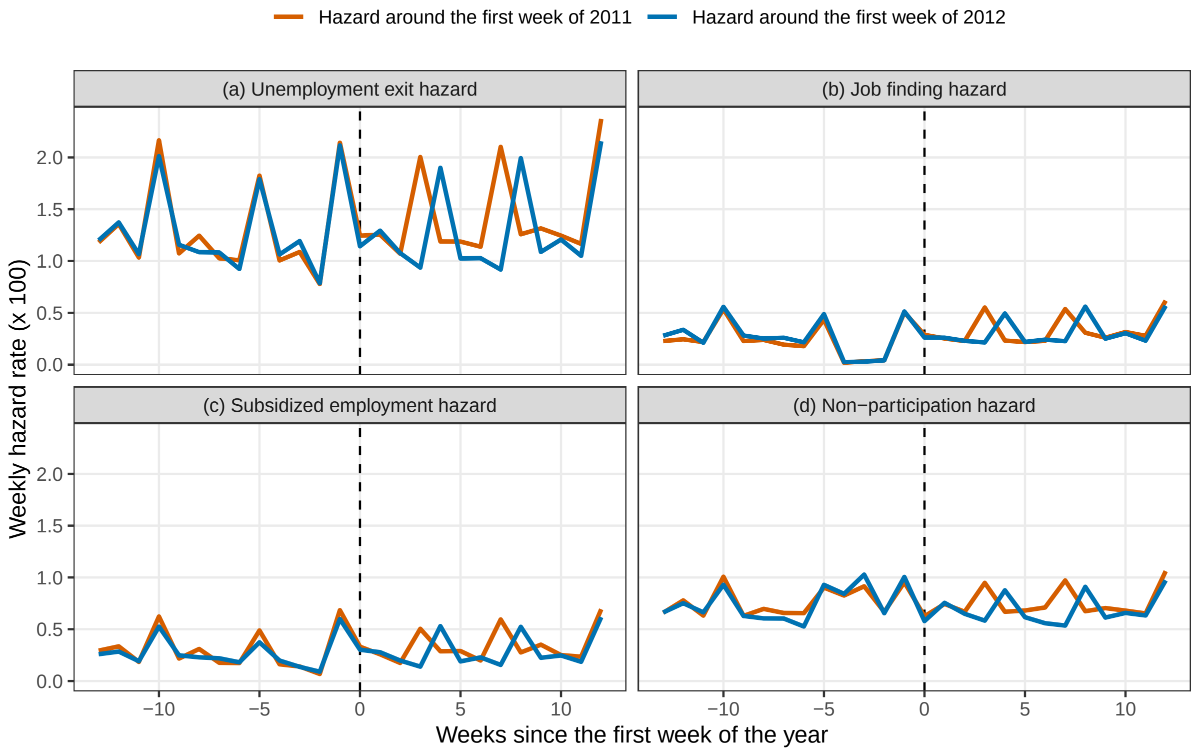


Figure 7: Hazard rates around the time of the turn of years 2013 and 2014

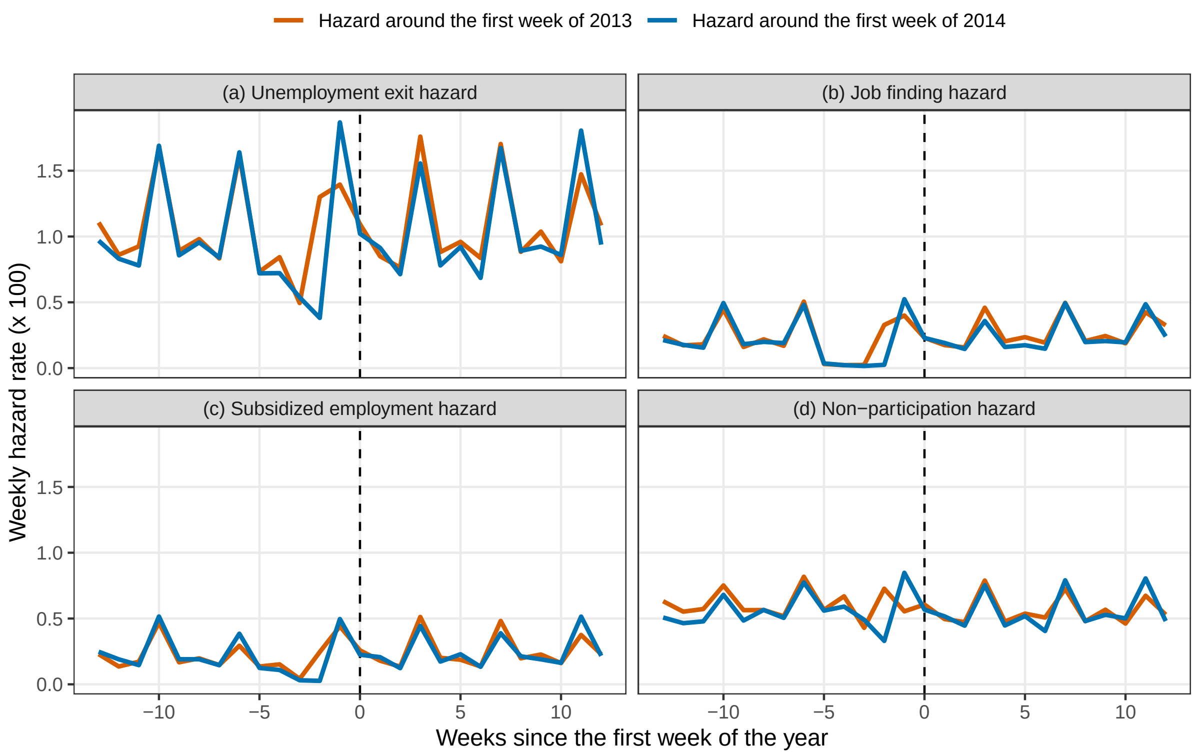


Figure 8: The effects of the 2012 reform and placebo reforms using the 13-week window