Azonosító: 135962

2023. szeptember 11.

Kutatási beszámoló

A kutatás a magyar jóléti ellátórendszer két radikális reformjának munkapiaci (és jövedelmi) hatásait vizsgálja adminisztratív adatok alapján. Az egyik reform 2011 őszén történt, ennek során az álláskeresési járadék időtartama 3 hónapra csökkent. A másik reform 2012-ben történt és a rokkantsági ellátások egyszerűsítését, illetve a hozzáférés szigorítását célozta. Ugyanazt az adatbázist, különbségek különbsége és matching módszereket használunk a két reform hatásának vizsgálatára.

A munkanélküli járadék rövidítésének hatása

Ebben az alprojektben a járadék igénybevételét és a munkanélküliségből való kilépést rövidtávon, a munkába állók jövedelmét és az új munkahely stabilitását középtávon elemezzük. Stratégiánk a regressziós diszkontinuitás technikáján alapul, amellyel összehasonlítjuk azoknak a kimeneteit, akik a reform hatályba lépése előtt veszítették el munkahelyüket azokéval, akiket a reform után bocsátottak el. Az elemzéshez a Közgazdaság- és Regionális Tudományi Kutatóközpont Adatbankjának adminisztratív adatbázisát használjuk a 2003-2017-es időszakra.

A kutatás második évében a következő feladatokat végeztük el:

Mivel ezt az adatbázist a projektünkhöz hasonló célra még nem használták, az adatok részletes ellenőrzését és előkészítését mi végeztük el, ennek befejeződése áthúzódott a kutatás második évére is (a feladat nehézségeit az első évi beszámolóban részleteztük).

Elkészítettük a tervezett statisztikai elemzések nagyobb részét, elkezdtünk dolgozni a publikálható kutatási jelentésen, illetve előkészítettük a 2023-ban tervezett konferencia jelentkezéseket.

A 2022-es évben két írás első változata készült el. Mindkét írásban ugyanazzal a sokasággal dolgozunk: azon javakorabeli férfiakkal, akik legalább egy évet dolgoztak folyamatosan, mielőtt az alkalmazotti munkaviszonyuk véget ért. Tehát ők mind a szabályváltozás előtt, mind utána jogosultak voltak álláskeresési járadékra. Kimutatható, hogy az álláskeresési támogatások összege igen visszaesett, és a szabályváltozások különösen a hosszú munkaviszonnyal rendelkező, de nem magas keresetű állásvesztőket érintették kedvezőtlenül.

A 2023-as évben tovább finomítottuk az elemzésünket, számos ponton beiktatva heterogenitás és robosztusság vizsgálatokat. A 2023-as évben a korábban elkészített tanulmányok újabb változatát készítettük el, mindkettő 2023 szeptemberében jelenik meg KRTK-KTI Műhelytanulmány formátumban.

Az elsőben két alkérdést vizsgáltunk meg. Először is: mennyiben csökkentette az álláskeresési járadék igénybevételét a jogosultsági idő radikális lerövidítése, és kik nem vették fel a járadékot? Becslési eredményeink szerint csak kis mértékben, mintegy 5% -al esett vissza a járadékba belépők aránya. Ugyanakkor éppen azok körében csökkent az igénylés, akik a legtöbbet vesztettek a szabályváltozáson. Ebben az elemzésben a 2011 ill. a 2012-es évek első hat hónapjában állásukat vesztő férfiak mintáján dolgoztunk.

Másodszor: akik a szabályváltozás előtt közvetlenül vesztették el az állásukat, igen sokat veszíthettek azzal, ha kivártak, és ezáltal a csak a jelentősen megkurtított álláskeresési járadékra váltak jogosulttá. Megvizsgáljuk, kik azok, akik mégis kivártak, akár információhiány miatt, akár azért, mert túlságosan költséges volt számukra (várakozási időben stb.) gyorsan beadni az álláskeresési járadék kérelmüket. Azt találjuk, hogy azok „késték le" a 2011 augusztus 31 -i határidőt, akiknek kevesebb veszítenivalójuk volt (rövidebb munkatörténettel rendelkeznek és alacsonyabb a keresetük), másodsorban azok, akik az ország legfejlettebb területein éltek. Míg az első eredmény rámutat az anyagi ösztönzők szerepére, a második arra utalhat, hogy ezeken a területeken nagyobb a járadék-igénylés szubjektív költsége (azaz nagyobb szégyennek minősülhet „segélyen élni").

A második írásunkban megvizsgáltuk, mennyivel gyorsabban helyezkedtek el a járadékosok a 2012-es évben. Ezt először a 2011 ill. a 2012 első hat hónapjában munkájukat vesztő, és 2 hónapon belül álláskeresési járadékot igénybe vevő férfiak mintáján becsültük meg. Annak érdekében, hogy figyelembe vegyük, a két évben eltérő jellemzőkkel bíró emberek vették igénybe az ÁKJ-t, a statisztikai párosítás módszerét használtuk.

A becslési eredményeink alapján éppen a munkavesztést követő 6-9 hónap között helyezkedtek el a 2012-es évben nagyobb arányban, mint egy évvel korábban. Ugyanakkor ez a gyorsabb elhelyezkedés átlagosan 16 nappal több állásban töltött napot jelentett a munkahely elvesztését követő 1 éven belül, amely igen csekély hatás ahhoz képest, hogy átlagosan 145 nappal kurtították meg az álláskeresési járadék jogosultságát (amelynek az értéke a korábbi keresetük arányában körülbelül 60 napnyi munkajövedelemnek felelt meg). Ugyanakkor azt is megmutatjuk, hogy az álláskeresők csak kis mértékben voltak kénytelenek a 2012-es évben a 2011-es évhez képest jelentősen rosszabb munkákat elvállani. Azt is megmutattuk, hogy ez az enyhe negatív hatás az eredeti állásvesztést követő egy évvel később már eltűnik.

Ugyanakkor azt is megmutattuk, hogy jelentős különbségek vannak az álláskeresők között, például az iskolai végzettség szerint. A felsőfokú végzettségűek körében igen pozitívak voltak a megrövidített ÁKJ-t igénybe vevők kimenetei. Egy éven belül akár 25 nappal többet tudtak dolgozni, viszont azon az áron, hogy körülbelül 10 százalékkal nagyobb fizetés-veszteséget voltak kénytelenek elkönyvelni. Azonban a következő évben tovább stabilizálódott a helyzetük, többet tudtak dolgozni, mint a 2011-es állásvesztők, és felzárkóztak a kereseteik is. Az érettséginél alacsonyabb végzettségű álláskeresőket összességében igen kedvezőtlenül érintette a reform, mert bár többet dolgoztak és így magasabb volt a munkajövedelmük, de ez még két évvel az állásvesztést követően sem ellensúlyozta az ÁKJ megrövidítése által okozott jövedelem kiesést. Ezzel szemben a felsőfokú végzettségűek körében már az állásvesztést követő első év végére az addicionális munkajövedelem kompenzálta az ÁKJ-n „elveszített" összeget.

A fenti elemzési feladatokat elsősorban Munkácsy Balázs és Greskovics Bori (junior kutatók) és Csillag Márton (szenior kutató) végezték el, Scharle Ágota (kutatásvezető) pedig a műhelytanulmányok elkészítésében működött közre.

A második írást az ESPAnet 2023-as évi konferenciáján, szeptember 8-án prezentálta Csillag Márton az 'Oksági hatások elemzése a szociálpolitikában' c. szekcióban. Korábban az első írást Csillag Márton a KRTK-KTI Oktatás és Munkagazdaságtan csoportjának szemináriumán adta elő június 7-én.

A két tanulmány KRTK Műhelytanulmányként jelennek meg 2023/26 és 2023/27 sorszámmal.

A rokkantsági ellátások rendszerének szigorítása

Ebben az alprojektben a rokkantsági támogatásokra való jogosultság hatását vizsgáljuk a foglalkoztatottságra és a jövedelemre, illetve a támogatás elvesztésének és a munkapiacra való visszatérésnek a hatását az érintettek egészségi állapotára. Fő hipotézisünk az volt, hogy a reform megnövelte a foglalkoztatottak arányát a komplex felülvizsgálaton átesett jogosultak között. Mivel azonban a tartós inaktivitás erodálhatta a jogosultak humántőkéjét, azt feltételezzük, hogy a rokkantsági ellátást elvesztők között sokan jövedelem nélkül maradtak, vagy nem a képesítésüknek megfelelő állásban helyezkedtek el. Azt is feltételeztük, hogy a reformban érintettek egészsége romlott a megemelkedett stressz és a biztos jövedelem elvesztése miatt.

A felhasznált adatbázis, a Közgazdaság- és Regionális Tudományi Kutatóközpont Adatbankjának ún Admin3 adatbázisa, a járulékbefizetések, rokkantsági ellátások folyósítása, a jövedelemadó, és az egészségügyi ellátások kapcsolt adatait tartalmazza, a rokkantsági ellátások igénylésének, illetve a felülvizsgálatnak a részleteit azonban nem.

A rokkantsági támogatás 2012-es reformja a korábban tartós jogosultságot szerzőket is egészségügyi felülvizsgálatra kötelezte, aminek eredményeképp sokakat visszairányítottak a munkapiacra. Az identifikációs stratégiában az első év során Garcia-Mandicó et al (2020) megközelítését követjük, akik a magyarhoz hasonló 2004-es holland reform hatásait vizsgálták.[1] Ez a stratégia a különbségek különbsége módszerre épül, és azt használja ki, hogy a kötelező felülvizsgálat csak az ellátottak bizonyos körét érintette, azokat akiknél a rokkantság mértéke nem érte el a 80 százalékot és 2011 végéig nem töltötték be az 57 évet. Mivel a felülvizsgálat tényéről nincs közvetlen információnk, azoknak a kimeneteit vetjük össze a reform előtt és után, akik 57 és 60 között voltak 2011 végén (vagyis nem érintette őket a reform), azokéval, akik még nem töltötték be az 57 évet. Mivel ez a két csoport az életkor mellett számos más, a munkapiaci esélyeket befolyásoló tulajdonságban eltérő lehet, egy további összevetési alapot is bevezetünk: a köztük lévő különbséget a súlyosan (legalább 80%-ban) megváltozott munkaképességű ellátottak azonos korcsoportjai között megfigyelt eltérésekkel vetjük össze. Első eredményeink szerint a reform szignifikánsan növelte az érintettek munkakínálatát, de sokan csak rosszul fizetett munkát találtak.

Az első évben elvégzett becslések robusztusságának ellenőrzése során azonban azt találtuk, hogy a kiválasztott kontroll csoportok reform előtti munkatörténete (pretrend) szignifikánsan eltér a kezelt csoportétól. Emellett az elvégzett placebo tesztek is arra a következtetésre vezettek, hogy a becslésben nem tudjuk megnyugtatóan kizárni a reform melletti egyéb tényezők torzító hatását. Ezért egy módosított megközelítéssel folytattuk a becsléseket, továbbra is a külömnbségek különbsége módszert alkalmazva. Hogy a becslési eredményeket potenciálisan torzító tényezőket kiküszöböljük, a továbbiakban a becslést egy jóval szűkebb (kor)csoportra, a reform idején 56 éves férfiakra szűkítjük. Ez az eljárás valamelyest kisebb becsült hatásokat eredményezett, ugyanakkor tisztább és jobban védhető identifikációs stratégiát jelent.

Eredményeink szerint a rokkantsági biztosítást a felülvizsgálat miatt elhagyni kényszerülők 57%-a az elsődleges munkaerőpiacon dolgozott, átlagosan 38%-uknak azonban sem foglalkoztatásból, sem a rokkantsági ellátásból nem volt jövedelme a reformot követő négy évben. A rokkantsági ellátásokból való kilépés következményei nagyban különböztek a reform előtti foglalkoztatási státusz szerint. A reform előtti munkaviszonnyal nem rendelkezők 62%-a rokkantsági biztosításból való kilépés után sem tudott elhelyezkedni, míg ez az arány csak 14% volt azoknál, akiknek a reform előtti évben volt valamilyen munkaviszonyuk. A felülvizsgálat az érintettek aktiválásában szerény eredményt ért el, ami erősen függött a reform előtti foglalkoztatási státusztól. Ez arra utal, hogy a rokkantsági

[1] Garcia-Mandicó, Sílvia, Pilar García-Gómez, Anne C. Gielen, and Owen O'Donnell (2020) "Earnings Responses to Disability Insurance Stringency." Labour Economics, 66: 101880

ellátásban részesülők munkapiaci integrációja akkor lehet sikeres, ha a pénzügyi ösztönzők mellé a foglalkoztatási esélyeket javító támogató munkapiaci programok is társulnak.

A kutatásban Krekó Judit és Scharle Ágota mellett Bíró Anikó, Hornok Cecília, és Prinz Dániel is részt vesz. A második évben két nemzetközi (ESPE 2022, Cosenza; Compie 2022, Mannheim) és egy hazai (KRTK KTI kutatói szeminárium) tudományos fórumon prezentáltuk az eredményeket. 2023-ban a Royal Economic Society éves konferenciáján, Glasgow-ban prezentáltuk (2023. április 3-5.).

A kutatás eredményeit KRTK-KTI Műhelytanulmány formájában 2023 júniusában közzétettük (KRTK-KTI WP 2023-19), 2023 szeptemberében pedig a cikket elbírálásra benyújtottuk egy Q1-es besorolású nemzetközi folyóirathoz.

Következtetések

A jóléti ellátások hozzáférését illetve bőkezűségét korlátozó mindkét reform jelentős jövedelmi veszteséggel járt, csaknem minden, a reformban érintett egyén számára. A jövedelmi veszteség egyedül a magasan iskolázott állásvesztük esetében volt kismértékű, abban az értelemben, hogy a reform miatt elvesztett járadékot kompenzálta a hamarabbi munkába állással szerzett többlet-munkajövedelem (a reform miatt elvesztett szabadidőt viszont ez nem pótolta). A reformok feltételezett célját, a munkábaállás ösztönzését tekintve az eredmények hasonlóak: reform nem jelentős mértékben, de szignifikánsan növelte a munkába állás esélyét, és nagyobb volt a hatás az eleve jobb munkapiaci helyzetű egyének (a járadék esetében az iskolázottabbak, a rokkantsági ellátás esetében a frissebb munkatapasztalattal rendelkezők) esetében.

Ez arra utal, hogy önmagában az ellátásokhoz való hozzáférés, illetve az ellátások összegének vagy időtartamának szűkítése nem elegendő a foglalkoztatottság ösztönzésére, miközben jelentős jövedelmi veszteséget okoz, a hátrányos helyzetű munkavállalók esetében megnöveli a szegénység és kirekesztettség kockázatát (és az ezzel járó további kockázatokat, például az egészségi állapot romlását, vagy a szegénység átörökítését illetően). A hátrányos helyzetű csoportok foglalkoztatásának növeléséhez (a kevésbé drasztikus anyagi ösztönzők mellett) a munkavégző képességet javító szolgáltatásokra és képzésekre, illetve a munkakereslet ösztönzésére (például a megváltozott munkaképességű álláskeresőkkel szembeni diszkrimináció csökkentésére) lenne szükség.

Az elkészült tanulmányok

A Bíró – C. Hornok – J. Krekó – D. Prinz – Á. Scharle: The Labor Market Effects of Disability Benefit Loss KRTK-KTI WP – 2023/19 https://kti.krtk.hu/wp-content/uploads/2023/06/KRTKKTIWP202319.pdf

M.Csillag – B. Munkácsy – Á. Scharle: Evaluating the effect of a drastic cut in unemployment benefit duration on re-employment and wages of jobseekers, presented at the Espanet Conference, 8 September 2023 https://espanet-warsaw2023.org/wp-content/uploads/Programme-12.pdf (teljes szöveg mellékelve)

M.Csillag – B. Munkácsy – Á. Scharle: Does cutting the value of unemployment insurance benefits affect take-up? Evidence from Hungary (előzetes kézirat, teljes szöveg mellékelve)

Evaluating the effect of a drastic cut in unemployment benefit duration on re-employment and wages of jobseekers[2]

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Abstract

We evaluate the effect of a drastic cut in potential benefit duration, reducing the maximum length of UI benefits from 9 to 3 months in Hungary at the end of 2011. We rely on rich longitudinal matched administrative data, which allows us to obtain information on a large sample of UI benefit claimants, and we use matching methods to evaluate the effect of the benefit cut. While UI claimants found jobs more rapidly as a result of the reform, we find only negligible negative effects of reemployment wages overall. The notion that changes are due to the reform is reinforced by the result that the effect on employment is largest for the group where the 'bite' of the reform was the largest. Our heterogeneity analysis reveals that the drastic cut seems to have reduced moral hazard for the most employable (those with tertiary education) and forced them to be 'less picky'. This means that they took up lower wage jobs, but this effect was only temporary. Overall, the reform led to significantly lower income for over 60 percent of jobseekers, while only benefiting less than 10 percent of jobseekers, over a two-year horizon.

[2] We thank the Databank of the Centre for Economic and Regional Studies for access tot he data used here; Zsuzsanna Sinka-Grósz and Melinda Tir for their advice on using the data. We also thank Bori Greskovics for expert research assistance, as well as the participants of the ESPAnet Conference in 2023 (Warsaw). The financial support from the research grant OTKA 135962 is gratefully acknowledged.

Introduction

The Great Recession and the rise of long-term unemployment in its wake has once again directed attention on the design of unemployment insurance benefit systems. Unemployment insurance (UI) schemes face a classic trade-off of providing sufficient insurance for workers in case of a negative income shock due to a loss of employment, while potentially inducing moral hazard among the insured. This incentive problem means that the generosity of benefits is positively related to the length of time spent on unemployment, as jobseekers can afford to search for the optimal job for longer, or lower their job search efforts. In principle, since moral hazard is less of an issue during recessions – since during these times it is rather the lack of job offers that leads to prolonged unemployment, rather than low job search effort on the part of the unemployed -, there is a rationale for extending UI benefit durations[3]. However, many European governments, due to budgetary considerations have chosen to decrease UI benefit generosity in the aftermath of the Great Recession.[4]

Recent advances on the effect of unemployment insurance benefits point out two aspects which is ignored by the moral hazard interpretation of the effect of benefit generosity.[5] The first is that unemployed may face financial difficulties, which force them to find a job as soon as possible, and unemployment benefits can ease these liquidity constraints. The second aspect is that with more time and resources that can be devoted to job search, unemployed can potentially find better jobs in terms of earnings and stability. Thus, a cut in the generosity of benefits will worsen liquidity constraints, which can lead to worse job matches. Not only can this have a negative effect on workers' welfare, but it can also be counter-productive from the point of view of the public budget, due to potentially recurring unemployment and associated benefit payments.

We investigate the effect of a drastic cut in the generosity of unemployment insurance benefits which happened in September 2011 in Hungary, when the maximum length of entitlement was slashed from 9 to 3 months. While this cut has been often criticized among labour economists, and restoring the adequacy of unemployment insurance benefits has featured among the European Commission's country-specific recommendations for four years in a row (2014-2017), little research has gone into providing reliable statistical evidence on its effects. Our study offers three contributions. First, we provide evidence on the effect of the decrease in the generosity of benefits (potential benefit durations, PBD) based on a clear identification strategy and reliable, large-scale administrative datasets. Second, we can examine not only unemployment durations, but also employment stability and incomes in the short-run.[6] Third, we take into account that the composition of UI beneficiaries might have changed due to the benefit reform, by performing a matching analysis. [7] What makes our paper of special interest is that while there are a number of articles looking at the effect of

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[3] Note also that unemployment benefits have a stabilizing effect on aggregate consumption during recessions.

[4] These include France, Hungary, Ireland, Portugal, Slovenia, the Netherlands, and Spain.

[5] See Tatsiramos -van Ours (2014) for an overview of the earlier literature; and Schmieder – Wachter (2016) for a review of the effect of UI benefit extensions.

[6] In a companion paper, we estimate and assess the extent of non-take up of UI benefits in Hungary, and whether the UI benefit cut had an effect on these.

[7] The decreased generosity (PBD) can substantially alter the number and pool of UI claimants, one is likely to underestimate the incentive effect of results due to this selection. Note however that the effect of PBD on the take-up of UI benefits is likely to be more modest than a decrease in (daily) UI benefits, and the effect is dependent on nonemployed persons' (subjective) expectations about their job finding speed.

cuts in the generosity of unemployment benefits on workers' post-unemployment outcomes, none of these have looked at situations where the cut in benefits has a real 'bite' and thus might have large and statistically significant effects.

We use a large linked employer-employee administrative dataset to evaluate the reform. This allows us to examine various aspects of job quality after re-employment, including the quality (wage setting) of the new employer. We use a sample of prime-age males with stable employment, for whom the UI benefit change lead to a loss of 142 days of UI benefits, on average.[8] We primarily use matching methods to evaluate the reform, as we have no credible control group that we can rely upon. We believe that our matching produces reliable results, as we can capture detailed information on employment history (and other characteristics, including health spending) prior to job loss. In this setting, we compare individuals who lost their jobs before the UI benefit reform was legislated (January to June 2011), to individuals who lost their jobs one year later (when all aspects of the benefit reform have taken effect).

As two robustness checks, we also use samples of UI benefit claimants closer to the cutoff date of 1st of September 2011. This approach has the advantage that macro-economic changes might affect outcomes less. However, besides having to contend with smaller samples, there is a higher probability of including individuals who acted strategically when applying for UI benefits.

Changes in the unemployment benefit policy in Hungary

In Hungary, the unemployment benefit scheme is traditionally not very generous. In 2010, the net replacement rate of unemployment benefit (defined as the ratio of an average production worker's net benefit during the first month of unemployment to their previous net monthly wage) was around 41% according to Esser et al. (2013). This number was the 6th lowest in the European Union where two-thirds of members states had 50% net replacement rates or above, and close to half of them had 60% or above. This was primarily due to a low benefit cap in Hungary: daily benefit was calculated as 60% of mean daily earnings from the last four quarters before job loss with a maximum amount of 1.2 times the minimum wage.[9]

During the period between 2005 June 1st and 2011 August 31st, the UI benefit system worked as follows. First, the number of eligible base days (working days) were converted with a 5:1 ratio (thus 5 eligibility base days[11] counted for 1 UI benefit day); with the maximum benefit duration being 270 days. The base eligibility period was four years, and the minimum number of insured days to qualify for UI benefit was 365. Second, there were two periods of UI benefits, a first one, proportional to pervious earnings, and a second one, with flat-rate benefits – the unemployment assistance. The first period was equal to half potential duration of benefits, with a maximum of 91 days, and the daily benefit amounted to 60% of earnings

[8] In terms of the value of UI benefits „lost" as a ratio of daily earning (prior to job loss), this amounts to about 56 days' earnings.

[9] This has been the case ever since the UI benefit system has been instated with the fall of communism. See Micklewright – Nagy (1994).

from the previous year. The second period ('unemployment assistance' for the remainder of the potential benefit duration) paid a flat-rate set at 60% of the minimum wage in 2011.[10]

In 2012, the Hungarian government decided to cut the unemployment benefit even further. It was a complicated reform which, at its core, introduced four changes to the regulation, with the most significant changes affecting the length of the potential duration of UI benefits. First, for each day of benefit, the number of required eligibility base days[11] were doubled, thus 10 working days make the beneficiary eligible for 1 day of benefit. Second, the maximum number of benefit days dropped to 90 from 270 during the reform, and the flat-rate benefit period (the unemployment assistance) was abolished.[12] Third, the base period for eligibility shrank to 3 years from 4 years before, with a 360 minimum insured days for qualification. This meant that after the reform people who worked consistently years ago but started working more erratically in the recent past had a lower chance of eligibility for unemployment benefit.

In contrast to the changes to the potential benefit duration, the daily benefit amount was modified only slightly. Specifically, the daily benefit cap changed from 1.2 times the minimum wage to the amount of the minimum wage. Note however, that the nominal maximum daily benefit did not change significantly from 2011 to 2012, as the reform was accompanied by a substantial, 119.2% increase to the minimum wage. All other rules regarding the calculation of daily benefits were unchanged. It is based on daily earnings during the last four calendar quarters (prior to the initiation of the UI benefit claim), where total monthly earnings were divided by the number of days employed. The daily UI benefit is equal to 60% of the daily earnings in this base period, with no (daily) minimum, but a very low daily maximum (as highlighted above).

To understand the impact of the reform on potential benefit duration and the total value of unemployment benefits, we present an illustrative table[13]. In *Table **1*** Benefit losses as a function of previous earnings and employment stability we measure losses relative to (previous) earnings. Thus, the question is: how many days' worth earnings did an individual lose as a result of the reform? In our sample, an average person lost 1.9 days from the first (proportional) period and 147.2 days from the second (flat rate) period with the median being 1 day and 179 days

[10] There were a few other features of the benefit system which are worth noting. (1) The reference date for calculating UI benefits was the day when the jobless individual registered as unemployed. (2) Voluntary quits entailed a waiting period of 90 days. (3) If a person was on UI benefits during the base eligibility period, these days were not directly subtracted from the potential benefit duration, rather they were subtracted from the eligible base days (with 1 day of UI = 5 insured days). (4) There was a re-employment bonus scheme was in place with a bonus amount equal to 50% of the remaining total first-tier benefits, if the individual found a job on her own. However, this meant that if the bonus was claimed, all remaining benefit days were annulled.

[11] Essentially, these are days when the individual was insured. There are some complications, however. First, days when was on long-term sick leave (a) do not count as base days, but (b) the extend the base period for calculating eligibility. Second, days when the individual did not receive pay (due to missing work, for workplace temporary shutdown, for unpaid leave) do not count towards base days.

[12] Note that a means-tested minimum income benefit still existed, eligibility however was set at a very low treshold.

[13] It is also worth noting that those with high earnings face a replacement rate well below the nominal 60% rate.

(the maximum) respectively. To grasp the effects, we present this for two levels of earnings and two employment stability 'types'.

Table 1 Benefit losses as a function of previous earnings and employment stability

	Benefit lost (in previous working days)	
Mean prev. income	Continuously employed	Employed 50% of days
2011 minimum wage	108.7	27.35
twice the MW	54.65	13.67

As the table above indicates, the people who lost the most on the reform were low-income people with stable work history. Higher income people (e.g. at 2 times the minimum wage) and those with less unstable working history (e.g. less than 500 days in the past 3 years) lost relatively little with the reform. It is straightforward that relative losses decrease with prior income, as 'days lost' come from the flat-rate benefit period (it comes exclusively from losing unemployment assistance for those with full eligibility).

Another channel through which the reform impacted people is the reduction in the base period. The new regulation looks back only on 3 years of job history (instead of 4) to determine the number of benefit days the jobseeker is eligible for. 7.7% of our sample lost at least two days of benefit due to this change in 2012 (30% of those who were eligible for less than the maximum days). [14]

This already complex reform was further complicated by a regulatory mistake. Most of the new rules were implemented in September 2011 except for the reduced base period, which was instead increased to 5 years in September, only to be reduced to 3 years four months later in January. This lead to an intermittent period in the last four months of 2011 where most of the reform was implemented (cutting the benefits of most people). Besides the straightforward consequence that more people were eligible to UI benefits due to the extended base period (during September-December 2011), the modification of the law gave an opportunity to game the UI benefit system. This was specifically possible for those with long stable unemployment histories, due to the fact that past receipt of UI benefits is not subtracted from current UI benefit entitlement days, rather it is subtracted from eligible base days. More specifically, it was possible to receive UI benefits in the Fall of 2011 based on working days from year t-5 and t-4; de-register and re-register (and claim UI benefits) in the beginning of 2012. In that period, the individual could use eligible days from years t-3 to t-1. For this reason, we decided to only include people in our sample if they spent their last year working.

The fact that the UI benefit system is to be reformed was relatively widely known and debated. The initial plans for reform were made public on 13th of April 2011, which contained all of the main elements of the overhaul, however, it proposed to institute the changes starting from the 1st of January 2012. After a public debate, a version proposed on the 14th of June 2011 by the government included (a) drastically cutting the conversion ratio to 10:1

[14] On average, they lost 13 days of benefit due to this change in regulation alone.

for eligibility days; (b) maximizing the potential benefit duration in 180 days; and (c) extending the base period to 5 years. However, a week later (on the 22th of June) a modifying motion included cutting potential benefit duration to 90 days, and it was this version that was voted in parliament on the 11th of July 2011; this law became finalised and published on the 13th of July 2011. Thus, while there was considerable uncertainty about the details of the reform for 3 months, for those who were concerned, the radical cutting back of UI benefit duration was to be expected. We can note however, if an individual wanted to quit their job to be eligible for the longer potential benefit duration, they had to do that at the end of May, latest, due to the 90 day waiting period. Thus, given this uncertainty, we do not believe that voluntary quits purely motivated by the UI benefit schedule change would have increased significantly. [15]

Literature review

The effect of UI benefit design on later outcomes

Unemployment benefits are fundamentally designed to improve the job searching prospects of jobseekers, thus it is important to examine the effects of benefit design on later outcomes. The implications of more or less generous unemployment insurance has been analysed by economists for several decades, with the standard dynamic search model, developed by Mortensen (1977) being the most widely used in contemporary welfare analyses of UI (Baily, 1978; Chetty, 2008). This model is based on the idea that workers need to invest time and effort to find a new job, hence unemployment benefits have insurance value. During the decision process, individuals maximize the present value of expected utility, which is a function of income and leisure. The standard dynamic search model indicates that the amount of time and effort devoted to searching for a job should be constant or rising over the spell of unemployment, as benefits are exhausted, and assets become depleted[16]. However, various other forces, such as skill depreciation, can affect job search time in the opposite direction over the unemployment spell.

The early empirical examination of this relationship began in the US with studies by Moffitt (1985), Moffit and Nicholson (1982), and Meyer (1990). They generally show a positive relationship between job search time and the length or generosity of UI benefit. Card and Levine (2000) focused on exogenous variations in UI generosity caused by unanticipated policy changes, finding positive effects of potential benefit duration on unemployment duration. This finding has been supported by studies conducted in European countries, including Germany (Hunt, 1995), Austria (Lalive, van Ours, & Zweimüller, 2006), Poland (Puhani, 2000), Slovenia (van Ours & Vodopivec, 2006), Finland (Kyyrä & Ollikainen, 2008), and Portugal (Addison & Portugal, 2008). Other researchers have leveraged discontinuities in

[15] In principle, there was still the option of convincing one's employer to have the employment terminated by 'mutual agreement', but this might not have been an option for most individuals. It is worth mentioning that Hungarian Labour Code allows for the employment relationship to end by 'mutual consent' – not leading to the 3-month waiting period.

[16] For example, Meyer (1990) shows that the hazard of employment of unemployed US males from 1978-1983 increases as the time until exhaustion approaches, and that the hazard more than quadruples as one moves from 6 weeks to 1 week until exhaustion.

the UI system to identify the effects of UI, often using age thresholds (Lalive, 2007; Caliendo, Tatsiramos, & Uhlendorff, 2013; Schmieder, von Wachter, & Bender, 2012).[17]

By contrast, empirical evidence of the effect of UI replacement rate and length on match quality is more scarce and mixed. An early study by Ehrenberg and Oaxaca (1976) shows that for older male workers, a 10 percentage point increase in the replacement ratio leads to a 7% increase in post-unemployment wages with a lower impact for female workers. This idea that a more generous UI scheme can positively impact post-unemployment outcomes, such as increased job stability and higher wages, by allowing job seekers to be more selective and search for a better-fitting job has been supported by subsequent studies (Marimon & Zilibotti, 1999; Acemoglu & Shimer, 2000). [18]

One line of research on the impact of unemployment insurance benefit extensions on labour market outcomes was flag shipped by studies in Austria. Lalive et al (2006) examine the impact of a reform in the late 1980s, which increased the potential duration and gross replacement rate of unemployment benefits for certain groups of individuals. They find that extending the potential benefit duration and increasing the replacement rate both tend to reduce unemployment exits during the period covered by benefits. Their results indicate that extending the potential duration of unemployment benefits has a more detrimental effect on unemployment duration than an increase in the benefit replacement rate. In a subsequent paper, Lalive (2007) found that Austrian UI extensions reduced the transition to regular employment, prolonged the duration of unemployment and the time until a new job was taken, but had no effect on wages. Schmieder (2016) analysed a similar dataset from Germany and found that longer non-employment durations even led to a significant decrease in wage offers (-0.8%). Similar (zero or slight negative) results were found for Slovenia (van Ours & Vodopivec, 2008), France (Barbanchon, 2016), Germany (Fackler, Stegmaier, & Weigt, 2019), The Netherlands (Groot, Groot, & van der Klaauw, 2019), and Switzerland (Cottier, Degen, & Lalive, 2019). However, Dahl & Knepper (2022) identify a positive effect of UI benefit length on re-employment wages for the US (as a negative effect of UI cuts), and perhaps more surprisingly Nekoei & Weber (2017) also find positive effects on the same Austrian data that was used by Lalive (2007) using similar methodology but a different subsample. Nekoei and Weber attribute this apparent contradiction to theoretical shortcomings and suggests a new model with two separate channels of UI effect on wages: (a) the wage effect of job match quality, when UI benefit causes agents to seek higher-wage jobs where their skills are better compensated, and (b) the non-employment duration effect, which leads to a lower wage effect due to skills erosion, stigma, and decreased job search intensity.

Evaluation of the Hungarian UI benefit system

Due to the frequency and nature of UI benefit reforms and the availability of high quality administrative data from the unemployment register, the Hungarian UI benefit system have

[17] While these estimates end to vary, they are in the region of an 0.4 elasticity (meaning a 10 day extension of PBD is associated with a 4 day increase in non-employment).

[18] On the other hand, Addison and Blackburn (2000) find modest evidence in support of the UI benefit increasing the reemployment wages of recipients. They compare recipients with nonrecipients and find a weak and statistically marginally significant overall increase in reemployment wages.

been studied rigorously. Most of these studies exploit quasi-experiments created by changes to the system, which are always grandfathered, i.e. only affect new entrants.[19]

Several previous estimates on the disincentive effects of the UI scheme in Hungary focus on the reform (and quasi-experiment) of 1993, and one study examines the reform of 2000. The reform of 1993 raised the replacement rate but substantially cut the potential benefit duration, while the 2000 reform affected (slightly reduced) only the latter.[20] Analysing the 1993 policy change, Micklewright and Nagy (1995) found little evidence that UI benefit generosity would speed up returning to work, at the height of structural unemployment in the aftermath of the transition from socialism. Wolff (2001), who re-analysed the sample studied by Micklewright and Nagy (1995) by carefully considering the issue of workers on recall, finds no robust disincentive effects of UI benefits for men, and a small but robust effect for women aged below 30. He also finds that while transition rates to work increase in the month prior to benefit exhaustion (especially for women), there is little evidence that cutting PBD would substantially shorten non-employment spells. Köllő (2003) exploits cross section variation in data from the unemployment registry and detailed survey data for 1994 and 2001 to examine the PBD effect. He finds that the remaining entitlement period and the expected total benefit amount have a significant effect, in that exit rates rise towards the end of the entitlement period. However, the effect is very small for most workers except the small subgroup of job seekers with secondary or higher qualifications. Köllő and Nagy (1996) measure the impact of the length of a UI spells on the wages of reemployed workers, however, their paper is not focused on the impact of UI benefit rules, rather aims to provide a descriptive picture.

Lindner -Reizer (2020) examine a change in the UI benefit rules in 2005 which changed the time-pattern of UI benefits. This meant a change from a schedule that replaced a fixed proportion (65%) of previous earnings with a very low UI benefit ceiling to a two-tier schedule. The first tier was equivalent to 60% of previous earnings, but with a higher UI benefit ceiling, and the second tier paied a fixed amount (equal to 60 percent of the minimum wage). Lindner -Reizer (2020) evaluate this change for a sample of jobseekers with (previously) stable employment and relatively high wages, for whom the total value of UI benefits was left unchanged, but for whom this meant a strong 'frontloading'[21]. They use the timing of the reform, and show that the duration of non-employment for those affected was significantly shorter than for those entering UI before the new benefit scheme went into effect. In fact, before individuals affected by the reform seem to find jobs around when the first (more generous) step of the UI benefit is exhausted, and there is a sizeable gap in re-employment until about 9 months' of non-employment. They also show that the re-employment wages of those affected by the reform was slightly higher (by 2%), and conclude that the (positive)

[19] We do not discuss estimates of the effect of means-tested SA benefits.

[20] Köllő (2001) and Wolff (2001) point out the issue of recalled workers; who typically lose their job at the end of December or early January, claim UI benefit in January and get rehired in March or April (roughly 3 months later).

[21] More precisely, for 3 months their benefits were 342USD/month; and for another 6 months, their UI benefits dropped to 171USD/month.

effect of avoiding negative duration dependence of wage offers is higher than the (negative) effect of lowering reservation wages.[22]

Data

Our empirical analysis is based on an individual-level administrative panel database from Hungary, owned by the Databank of the Centre for Economic and Regional Studies (see Sebők (2021) for a detailed description). The data cover half of the country's population aged 0-74 in 2003, who were randomly selected and followed-up until 2017.[23] The database consists of linked data sets of the pension, tax, and health care authorities and the public employment services (hereafter PES) and contains detailed individual-level information on employment and earnings history, use of the health care system, pension, and other social benefits. The PES dataset (Jobseekers' registers) contains information on all registered jobseekers, including UI benefits, and the employment histories required to calculate these. Linking the PES database to the databases of the pension and health care authorities enables us to observe individuals' background characteristics and employment histories of job losers (not only those registered as jobseekers at the PES), and their employment and earnings outcomes for up to 4.5 years following UI benefit take-up.

Sample selection and characteristics

During sample selection, we needed to account for the effects of policy design flaws and the imperfections of the data generating process, while ensuring that the sample comprised of genuine jobseekers.

In our main sample, we took data on people aged 25-54 who lost their jobs in the first half of 2011 or 2012; thus, we removed all those who could have ended their contract strategically, since the exact regulatory changes to UI benefits became public knowledge in early July 2011. Please note that typically, more active labour market programmes and other tax cuts for employing young individuals are present in Hungary. Furthermore, individuals who are withing 3 years of retirement age (which was 60 at the time) could apply for pre-retirement UI benefits.

We also filtered out those who were likely not actively looking for work during their UI benefit (non-employment) spell for one of two reasons. First, those who probably already found a job before the end of their current work contract, and started their new job at most one week after job-loss (similarly to Blasco – Fontaine (2021)). Second, we excluded those who were likely waiting for a recall, thus those who returned to their prior employer within a three-month timeframe (see Köllő (2003) for a more detailed analysis of this phenomenon).

The sample was further restricted to individuals for whom benefit eligibility could be accurately predicted. This necessitated the exclusion of women, as they constitute the

[22] In DellaVigna et al. (2017) the authors show that this is consistent with a model where jobseekers are loss-averse relative to recent income (which serves as a reference point). They show that jobseekers increase their search effort just after the first step of UI has been exhausted.
[23] For details, see Sebők (2021).

majority of parental leave recipients in Hungary and their benefit entitlements are therefore difficult to estimate. Additionally, individuals who were not employees in the primary labour market during their previous employment were excluded, as their work histories are often more complex and may lead to errors in the data generation process, resulting in less precise estimations of benefit eligibility. This also means self-employed individuals were excluded because of the difficulty in determining whether their unemployment was due to job loss, a pause between contracts, or working off the books. This exclusion was implemented not only because the focus of the study is primarily on employees, but also to ensure the accuracy and reliability of our estimates.

Practical considerations led to further adjustments of the sample. We excluded individuals who took the benefit more than 61 days after job loss, comprising approximately 10% of benefit recipients.[24] This was necessary to avoid incorporating the effects of the interim period between September and December 2011 during which most reform changes were implemented but the eligibility base period was extended to five years. To account for this interim period and eliminate the possibility of rent seeking[25], the sample was restricted to individuals with a relatively stable employment history, defined as those who worked at least 360 days in the past 12 months and received wages or salary for at least half of those days (a similar adjustment to what Schmieder, von Wachter and Bender (2016) made to drop those with fractured labour market histories). Additionally, outliers in terms of earnings, health variables, and potential available maximum benefit were excluded, as were jobseekers with very low estimated benefits or no entitlement period. Finally, we also did not use a sub-sample of individuals for whom we could not estimate Abowd-Kramarz-Margolis (1999) firm and individual fixed effects.

We retained all the sample restrictions listed above when we formulated our samples for the robustness analysis. We only changed the timing of UI benefit take-up (job loss), to include the period 2011 June-November.

Evaluation strategy

The effect of shortening the potential benefit duration on re-employment and wages

Given that the change in the UI benefit potential duration regulation affected all UI beneficiaries, it is not possible to construct a control group at this instance. One possibility is to use those who were not eligible for UI (but had worked in the last 6 months, for instance) as controls. These individuals differ however in one of the key determinants of re-employment probability: employment history. Another possibility is to use those similar characteristics (hence eligible for UI benefits), but who choose not to register as jobseekers. This group likely did not apply for UI benefits since they expect to be re-employed rather

[24] Note that this also amounts to leaving persons who quit their job out of the analysis, as they were subject to a 3 month 'waiting period' before becoming eligible for UI benefits. We did not see any spikes in UI benefit uptake at this point in time, so we can likely conclude that not many workers chose this option.

[25] Rent seeking was possible due to a regulatory mistake, allowing jobseekers to take the benefit twice: first in the fall of 2011 and then again in the first quarter of 2012.

swiftly, hence likely differ in some unobserved characteristics directly related to their (future) labour market outcomes. Furthermore, given the severity and the extent of the reform, we cannot rule out general equilibrium effects, which further complicates choosing a control group.

Thus, we will evaluate outcomes of UI beneficiaries starting their UI benefit spell between 2011 January-August versus those with a UI benefit spell starting in 2012 January-August. It is important to emphasize that we use men whose employment spell finished on June 30th latest, in order to avoid strategic scheduling of employment ending before the UI benefit schedule change at the end of August 2011.

There are two potential issues with this approach. First, potential macro-economic and labour market developments which could affect the employment outcomes of the two cohorts of non-employed. There was small, 1.5 percentage point increase in the male employment-to-population ratio, but the ILO unemployment rate stood at 11 percent in both years.[26] Second, there were some minor policy changes, which could potentially contribute to an improvement in the re-employment probability of the 2012 cohort. On the one hand, the public works programme was further expanded, however, at this point, its main target groups were those with low re-employment prospects. On the other hand, hiring subsidies targeted at unemployed were expanded, the take-up of this subsidy remained low and only marginally improved jobseekers' re-employment probability.[27]

However, it is possible that the composition of UI beneficiaries differed across the two years as a consequence of the changes in rules (as opposed to differences in patterns of job loss). In other words, we cannot exclude the possibility that the composition of those taking up UI benefits changes. In order to adjust for this, we used matching to adjust for the differing composition. In a first step we estimated a logit equation (with the outcome being that the individual took up UI benefit in 2011) with a rich set of background characteristics, to predict propensity scores. It is worth noting that we can condition on detailed labour market outcomes from the two years prior to job loss[28].

A central feature of our longitudinal matched employer-employee dataset is that it enables us to estimate Abowd-Kramarz-Margolis (1999) wage equations. These allow us to estimate individual and firm fixed effects (pertaining to the most recent employer); where the individual fixed effect contains all (time-invariant) determinants of earnings, including 'unobserved ability'; while the firm fixed effect pertains to the firm's wage setting strategy

[26] There was a small improvement in the registered unemployment rate.

[27] The previous voucher-based system changed to an employer tax credit system from January 1st 2013 which did lead to a marginal increase in take-up. Both were targeted at persons who had been registered unemployed for at least 6 months. See Svraka (2019) for more details.

[28] The complete list of variables used in the estimation of the logit equations are the following: month of job loss; age (and its square); number of days elapsed between jobloss-date and date of registration as jobseeker; 1 digit occupation held in last job; firm FE of last job; individual FE; number of days counting towards UI benefit in last 4 years; average (log) daily earnings last year; total insured days in four 6-month intervals prior to job loss, total labour income over last two years; development level of micro-region of residence interacted with NUTS2 level region; proportion of Roma population in micro-region; two health spending indicators (calculated over last two years).

(controlling for the composition of workers). In essence, these can be used as (imperfect) measures of worker and firm 'quality'. We only use data from before the job loss[29], to exclude the possibility of endogeneity contaminating our estimates.

In a second step, we use kernel matching applied to a propensity score [30], with the restriction that everyone should be matched to people from the same region and the same level of education.[31] The standard errors are computed based on estimated influence functions, as proposed by Jann (2019, 2020). [32] As a robustness check, we also present results based on minimum distance matching[33].

We show a number of outcomes, starting with duration until a stable employment[34], which is characterised by both a duration (censored at one year); as well as two binary variables: has the individual found a new job within 6 months, and within 1 year (after job loss). The next set of outcomes looks at the total number of days employed, and total labour income over four 6-month periods following job loss, as well as the cumulative labour income and days worked over a 24 month period.[35] We further look at two aspects of job quality: the number of days employed at a wage above 80% of the minimum wage, and the number of days employed in a public works contract.

We also examine the (daily) earnings of individuals after re-employment: we take the difference between the (log) average daily earnings in the 6 months prior to job loss and the (log) average daily earnings in months 7-12 after; as well as in months 19-24 after. The first of these is to measure wages immediately upon re-employment, while the second is to assess career progression. It is important to note that those who were not employed are not included in this measure, and average wages are calculated over days when the individual worked for pay (it does not include days when the individual was employed, but was on long-term sick pay etc.). We also use the difference in estimated AKM firm fixed effects over the

[29] More precisely, for individual fixed effects, we only use data inclusive of 2011, while for firm fixed effects, we use data for the whole sample period.

[30] We prefer kernel matching over the commonly used one-to-one or nth neighbour matching because it is more efficient, allowing us to exploit more variation from the control sample.

[31] Our chosen bandwidth is based on the method proposed by Huber et al. (2015). As the results are usually robust to changes in the shape of the weight function (see Caliendo & Kopeinig (2008)), we simply opt for the widely used parabolic (aka. Epanechnikov) kernel with the common support restriction.

[32] These errors are robust to heteroskedasticity; however, they assume fixed matching weights, which is an oversimplification. Monte Carlo simulations suggest that this bias usually leads to estimates with a relatively small bias if multiple matches are used (Jann, 2020) and this bias tends to be conservative (i.e. loo large standard errors) when using propensity score.

[33] In the minimum distance algorithm, we use number of days worked in the pre-job loss four-year period, age, number of days elapsed between the end of the employment spell and UI benefit take-up, as well as the individual and firm fixed effects (of the firm where the individual was employed in the year prior to job-loss) estimated from an AKM wage equation, along with the propensity score. These variables capture the labour market history of individuals well, and the propensity score summarizes all other information in a succinct way.

[34] Note that a new job is defined as an employment spell lasting at least 3 months, and we exclude workers on a temporary contract and on a public works contract.

[35] Note that this differs slightly from the definition of stable employment, as it encapsulates all insured employment.

same periods, which is to characterise 'firm quality'. In other words, we want to measure to what extent wage changes are due to moving down the firm pay ladder.

We include a robustness checks, where we use samples closer to the policy change. The first includes individuals who took up UI benefits between 1st of July 2011 and 20th of August 2011; and we compare them to individuals who started their spell between 11th of September 2011 and 31st of October 2011. Thus, we exclude UI spells in 'ball' around the 1st of September, as it is the most likely that rescheduling of registration as jobseeker was prominent among these individuals. [36]

Arguably, since there is on average a 2-month difference between the start of non-employment for the treatment and control individuals, the labour market which they face will only be slightly different. Clearly, there might be differences due to seasonal effects in the very short run, as four months after their UI benefit take-up, the control sample will have been facing a late Autumn (November) labour market when it is slacker than in March (when the treatment individuals reach four moths after the start of their UI benefit spell). A further complication is the seasonality of public works employment, which generally starts in February -March each calendar year, and tends to end in November-December of the same year. This creates an issue, since if an individual in the control group starts (say) their UI benefit spell on the 1st of July 2011, assuming that they are eligible for 270 days, by the time their benefit will have expired, the probability of entering public works will be low. By contrast, an individual in the treatment group, who enters into unemployment on the 1st of November, at the point when their 90 day UI benefit runs out will have a high probability of entering public works (in February 2012).

The effect on employment and earnings

Descriptive evidence

We first present descriptive evidence on how the outcomes of UI beneficiaries differed across the two policy regimes (2011 vs 2012). Our focus is on the speed of re-employment, and labour market outcomes in the initial 24 months following job-loss.[37] In what follows, we use a slightly restricted sample of individuals for whom we have estimates of individual and firm fixed effects from an AKM-type wage regression.[38]

In the first graphs we show the re-employment paths of UI beneficiaries and how they differed across 2011 and 2012. From the survival curves, we can note that the shortened PBD lead to quicker re-employment, with a significant gap opening up from the fourth month of non-employment. By 6 months after job loss, about 42 percent of UI claimant were re-employed

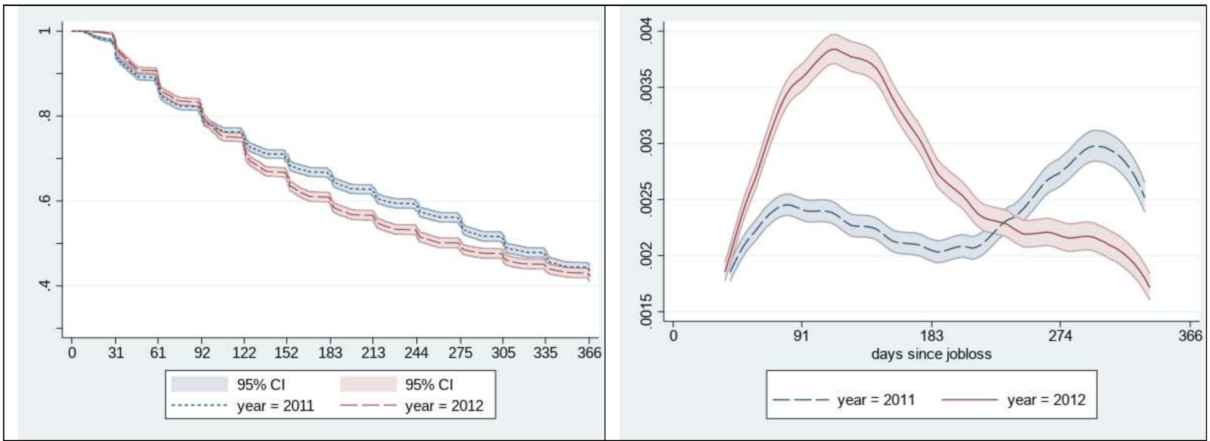
[36] Wealso experimented with a second, more conservative approach. We used individuals who started their UI benefit spell between the 1st of June 2011 – 31st of July 2011 and we contrast their outcomes with those who started their UI spell between 1st of October 2011 – 30th of November 2011.

[37] Please note that we do not model the timing of UI benefit claims.

[38] We lost roughly 5% of the sample. We ran all our analysis for the full sample, and the results did not differ qualitatively.

in 2012, while only about 32 percent found a job in 2011. This gap is fairly stable between the 6th and the 9th month of non-employment. By the end of a year, after the UI benefits ran out in 2011, the gap between the survival curves is much smaller, albeit there remains a significant 3 percentage points difference. From the hazard curves it is easy to discern that re-employment probability is the highest in the months shortly following UI benefit exhaustion, with a peak between 3-5 months of non-employment in 2012 and around 10 months in 2012.

Graph 1 Kaplan-Meier estimates of time in unemployment, and hazard out of unemployment, by year



(a) survival curves	(b) smoothed hazard function

In the next Table, we present descriptive evidence on the main outcomes. First, as in line with the graphs above, the re-employment rate of UI claimants in 2012 is 42 percent after 6 months elapsed since job-loss, 10 percentage point higher than for the UI beneficiaries in 2011. By 12 months' elapsed duration close to 62 percent of jobseekers found stable employment in 2012, and the gap to 2011 is only 3.5 percentage points. By 1.5 years' duration, the gap further shrinks, but remains statistically significant.

Table 2 Share of jobseekers finding stable employment within 6, 12 and 18 months after job loss

	6 months	12 months	18 months
2011	0.317	0.579	0.619
2012	0.417	0.614	0.639

The following outcomes we look at are number of days worked and total labour income in three 6-months periods after job-loss, as well as the total days of work (and labour income) by the end of 18 months. These results tell a similar story as the one above: after the policy reform, UI claimants work significantly more in the first year after job-loss than before the radical shortening of the PBD. They work roughly 8 days more in each of the first two 6-month periods, and their labour income is 18-10 percent more. However, all differences in the outcomes of (former) UI claimants across the two regimes disappear after one year. This means that over a 1.5 year period, unemployed worked 15 days more after the policy change, a 6 percent increase.[39] This went along with a significant increase in total labour income, amounting to an 8 percent increase over the 18 month period. It is important to emphasize that in terms of total income, UI claimant unemployed were likely slightly worse off after the policy change - considering only the short-term consequences – as the total of claimed UI benefits and labour income were lower after the policy change.

Table 3 Change of descriptives for 2012 (for the given period, months after job loss)

	Days employed				Cumulative earnings	UI benefits
	1-6 months	7-12 months	13-18 months	19-24 months	1-24 months	
Year: 2012	9.00***	9.42***	0.735	6.93***	156.61***	-136.25***
	(0.763)	(1.256)	(1.038)	(1.021)	(8.654)	(1.674)
Constant	41.96***	84.61***	113.06*	113.63***	1569.50***	307.23***
	(0.698)	(0.877)	(0.733)	(0.721)	(6.041)	(1.182)
Observations				22577		

Robust standard errors in parentheses Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In the next table, we present some preliminary evidence on the effect of the cut in the PBD on job quality. We look at (a) the changes in the (log of) real daily wages from before the unemployment spell to after the unemployment spell and (b) changes in firm fixed effects. Evidently, this can only be calculated for those who worked for pay, a selected subsample of all unemployed (and this means that the number of observations for the different periods will vary). All wages (and firm fixed effects) are weighted by number of days worked, and calculated over 6 months periods.[40] Arguably, these are simplistic indicators of job quality, but they are useful for some preliminary evidence.

For the first two outcomes, we find no discernible difference between the UI claimants from the different PBD regimes. For the next two outcomes, we find some interesting phenomena. All UI beneficiaries had to contend with a 13 percent decrease in wages upon re-employment initially, which decreased to about 9 percent 12 months later. This is largely because those who remained employed progressed in terms of pay, as if we keep the sample constant (only using those who worked for pay in all of the periods), the wage cuts disappear by the 13-18 month period. Equally important is the finding that the 2012 UI claimants have a roughly 3-4 percent lower wage upon re-employment (relative to their wage before job-loss) than the 2011 claimants. Finally, we can see that the wage cuts are not primarily due to taking up jobs at worse paying firms, but the additional negative consequence for re-employment wages of the shortened PBD is largely a result of having to move to low-paying firms.

Table 4 Change of change in (log) daily real wage (relative to wage in months [-6; -1])

	Change in daily wages	Change in firmFE

[39] Notice however that they were only employed slightly more than half of the time.

[40] Thus, for example, if an individual worked for two companies, for 3 and 2 months, respectively, then the firm fixed effect of the first firm will get a 60% weight, while the FE for the second one will get a 40 percent weight.

	Months 7-12	Months 19-24	Months 7-12	Months 19-24
Year: 2012	-0.023**	-0.004	-0.020***	-0.018***
	(0.009)	(0.009)	(0.005)	(0.005)
Constant	-0.144***	-0.108***	-0.025***	-0.019***
	(0.006)	(0.006)	(0.004)	(0.005)
Observations	16075	17883	16075	17883

Robust standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Matching results

We turn to the propensity score matching results, which correct for the possibility that the composition of UI beneficiaries changed across the two years. This might be important, since we showed in the previous section that the composition of those claiming UI benefits improved slightly, as proxied by their prior earnings. The matching approach has two limitations. First, that we assume that we captured all relevant determinants of labour market outcomes, in other words, there were no differential changes in the distribution of unobservables across the two inflows. Notice however that we have an exceptionally rich set of covariates (including individual fixed effects from earnings regressions) at our disposal. Second, and perhaps more problematically, we need to assume that there were no changes in the labour market (or in other labour market policies) across the two years used. This likely true since employment started to grow (and the unemployment decrease) very slowly until 2013 in Hungary. We present ATT estimates, thus we weight differences by the distribution of characteristics of the 2012 inflow into UI benefits.[41]

Table 5 Proportion found a stable job in the primary labour market 2012 vs 2011

	within 183 days	within 366 days	within 548 days
ATT	0.0868***	0.0259***	0.0155**
	(0.00737)	(0.00749)	(0.00703)
	(0.009)	(0.009)	(0.008)
Number of treated	10522	10522	10522
Number of controls	10632	10632	10632

Robust standard errors in parentheses Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, occupation codes, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

In the first Table we present estimates of the re-employment probability: in line with our previous results, we find that the re-employment rate was higher in 2011 at 6 months' duration, by about 9 percentage points, which is a more than 25 percent increase (compared

[41] In the Appendix, we show measures of the quality of matching.

to a baseline of 32 percent). By 12 months after job loss, when UI benefits have been exhausted in 2012 as well, the re-employment probability gap shrinks to about 3 percentage points, amounting to about a 5% increase.

In the next Table, we present estimates of the number of days employed and labour income. Please note that we use all days of insured employment. These estimates are close to the descriptive evidence, with number of days worked and labour income higher in the months 1-6 and 7-12 after job loss due to the shortened PBD, with about 8 additional days employed in each 6-month period. Looking at longer term results, over a period of two years after job loss, we find that due to the shortened benefit duration, prime-age male jobseekers ended up being employed by 23 days more on average (a 6% increase over the baseline number of days employed). Over the same period, they earned about 130 thousand HUF more, which is an 8 percent increase in labour income, compared to the jobseekers who lost their employment in 2011. It is worth contrasting this to the income from UI benefits, which was lower by about 140 thousand HUF on average in 2012 (compared to the mean UI benefits amounting to 310 thousand HUF).

Table 6 Effect of reform on days employed and labour income, by semester following job loss

	days employed	labour income
month t+1 to t+24	20.67***	111.5***
	(3.639)	(28.19)
month t+1 to t+6	7.708***	30.48***
	(0.890)	(5.448)
month t+7 to t+12	7.578***	33.37***
	(1.174)	(8.043)
month t+13 to t+18	-0.0439	6.236
	(1.193)	(8.803)
month t+19 to t+24	5.433***	41.40***
	(1.174)	(9.124)
Number of treated	10522	10522
Number of controls	10632	10632

Robust standard errors in parentheses Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, occupation codes, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

By the months 19-24 after job loss – when both groups had similar employment rate and number of days worked – there was no difference between the two years (while job losers still hade wage rates about 10 percent lower than their previous wages). It is also important to examine other aspects of job quality. We can see that the number of days worked in jobs where the daily average earnings were higher than 80 percent of the daily fulltime minimum wage is substantially lower than the number of days worked. This means that about one-third of the additional days worked were likely in a part-time job. We can notice however that the higher employment results of 2012 UI benefit recipients was not due to a higher number of days employed on public works contracts.[42]

Table 7 Effect of reform on outcomes aggregated over months 1-24 after job loss

	labour income month	days employed	days worked W>mininum wage*0.8	days worked in a public works contract
ATT	111.5***	20.67***	12.19***	0.223
	(28.19)	(3.639)	(3.839)	(1.517)
N. of treated	10522	10522	10522	10522
N. of controls	10632	10632	10632	10632

Robust standard errors in parentheses Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, occupation codes, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

Turning to the estimates of the re-employment wages, we find that while all jobseekers' wages decreased substantially (by about 15 percent) in the jobs they found in months 7-12 relative to the wages just prior to job loss, this wage decrease was only marginally larger in 2012 compared to 2011.

Table 8 Effect of reform on changes in daily earnings and firm fixed effects

	(1) Daily earnings, months 7-12	(2) Firm FE, months 7-12	(3) Daily earnings, months 19-24	Firm FE, months 19-24
Real daily earnings, months 7-12	-0.0134	-0.0069	0.0058	-0.0039
	(0.0105)	(0.0059)	(0.0101)	(0.0056)
N. of treated	7659	7659	8458	8458
N. of controls	7403	7403	8287	8287

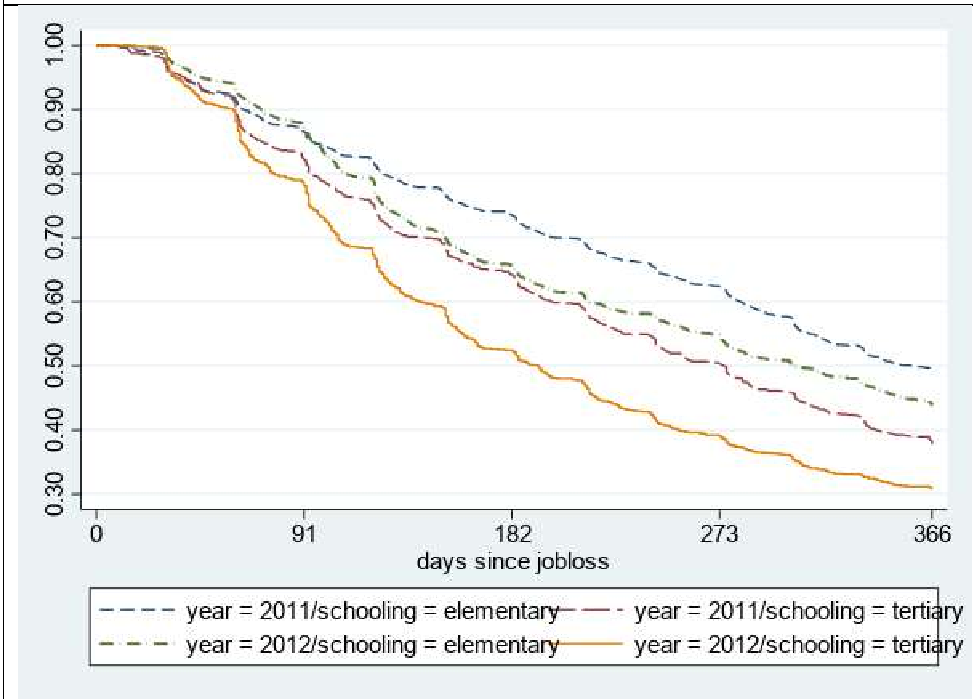
Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Outcome variable is the change in (log) daily earnings between months 1-6 before job loss and the daily earnings in the relevant period. Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

[42] Although public works contracts were widespread, former UI beneficiaries worked about 1 month on PW over a two-ear period.

Heterogeneity checks

Finally, we test the heterogeneity of our findings, across three variables. First, across four education categories: (1) those with primary education; (2) those with vocational education; (3) those with upper secondary education and (4) those with tertiary education.[43] In the Graph below, we show Kaplan-Meier estimates of the survival in non-employment (exit towards stable jobs) for the lowest and the highest education categories, by year. Note that propensity score matching weight have been applied to the sample – hence we already control for changes in composition across the two years. We can clearly see that those with higher education find jobs quicker independent of the year, and this difference is quite pronounced. What is more interesting is that the effect of the UI benefit reform seems to have been larger for the tertiary educated.

Graph 2 Kaplan-Meier estimates of time in non-employment, elementary and tertiary educated, 2011 and 2012



In the Table below we can see that the very short term effect of the UI benefit change at 6 months after job loss is similar across education categories (with improvements in re-employment rates that are up to 30 percent relative to the baseline level). However, by the end of one year, most of the policy effect disappeared, although it is still substantive for those with the lowest levels of education, and particularly for those with tertiary education. In line with this, in the first year after job loss, due to the shortening of the PBD, tertiary educated jobseekers were employed for an additional 22 days (which is a 15 percent increase), while other groups around 15 days. These gains increased slightly in the second year following job loss. An exception were the tertiary educated, whose employment gains were similar in the

[43] The composition of our sample across these categories is roughly 19; 42; 29; and 9 percent, respectively.

second year, and thus, over two years, they worked close to 12 percent more due to the UI benefit reform.

Table 9 Proportion found a stable job in the primary labour market 2012 vs 2011, by level of education

	elementary school	vocational school	upper secondary school	tertiary
within 183days	0.0713***	0.101***	0.0826***	0.0991***
	(0.0159)	(0.0113)	(0.0140)	(0.0250)
within 366days	0.0513***	0.0129	0.0216	0.0658***
	(0.0172)	(0.0116)	(0.0141)	(0.0241)

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

Gains in labour income largely mirror the findings for days employed. For those without tertiary education, total labour income increased by around 6 percent over two years. By contrast, the total labour income of those who have completed at least college increased by more than 12 percent in the first year after job loss, and their gains were even larger over a two-year period. This implies very marked differences across education categories in their overall income, as the drop in UI benefits were very similar for all jobseekers[44]. In fact, for the two lower education categories, the increase in labour income did not compensate for the decrease in UI benefits, hence they experienced a roughly 5 percent decrease in total income.[45] Those who finished upper secondary education barely broke even in two years following job loss, while for tertiary educated, the labour income slightly exceeded decreased UI benefit income already by the end of one year after job loss.[46]

Table 10 Number of days employed and labour income 2012 vs 2011, by level of education

	elementary school	vocational school	upper secondary school	tertiary
days employed; months 1-12	14.93***	16.67***	13.59***	23.03***

[44] This is related to the very low ceiling in UI benefits in Hungary, as well as to the fact that tertiary educated found a job before their UI was exhausted in 2011.
[45] Pls not that we did not calculate the value of minimum income benefits, which would paint a slightly less negative picture for those with the lowest level of education. We cannot include this in our calculations, since those benefits are calculated at the household level (though they are se tat a very low level).
[46] Over two years, taking into account decrease in income from UI benefits, tertiary educated jobseekers total income increased by about 10 percent.

	(4.012)	(2.845)	(3.587)	(6.558)
labour income months 1-12	56.03***	61.59***	53.02**	158.8*
	(16.68)	(13.20)	(24.08)	(81.91)
days employed months 1-24	23.53***	19.42***	16.59**	45.65***
	(7.967)	(5.472)	(7.036)	(12.40)
labour income months 1-24	73.43**	76.33***	87.60*	520.2***
	(36.13)	(28.20)	(52.31)	(179.7)

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

Turning to the results of wages, we find no evidence of any negative effects on wages for the three lower schooling categories. For tertiary educated, we find a large immediate wage effect[47], and a significant downgrading in terms of firm quality. This can partly be due to the fact that there was a small increase in public works participation in this schooling group. However, this negative wage effect disappears one year after job loss.

Table 11 Changes in real daily wages and firm FE (relative to 1-6 months before job loss)

	elementary school	vocational school	upper secondary school	tertiary
Real daily earnings, months 7-12	-0.0045	0.0085	0.0067	-0.1546***
	(0.0223)	(0.0151)	(0.0204)	(0.0391)
Firm FE, months 7-12	-0.0166	0.0086	-0.0048	-0.0644***
	(0.0119)	(0.0087)	(0.0121)	(0.0226)
n_treat	1455	3182	2260	764
n_used_cont	1414	3296	2057	638
Real daily earnings, months 13-18	-0.0128	0.0213	0.0250	-0.0391
	(0.0201)	(0.0151)	(0.0201)	(0.0385)
Firm FE, months 13-18	-0.0282**	0.0091	0.0029	-0.0269
	(0.0112)	(0.0082)	(0.0113)	(0.0221)
N treated	1611	3539	2465	840
N controls	1630	3666	2243	738

[47] It is also worth keeping in mind that part of this wage effect might be due to selection on unobservables that is not fully captured in our matching procedure.

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

In our second heterogeneity check, we distributed the sample in terms of relative effect of the UI benefit reform. Thus, we calculated the decrease in the value of UI benefits across the two regimes, and divided it by the daily wages prior to job loss. Using this statistic, we divided the sample into three categories: with the value of UI benefit change less than 45 days' earnings; between 45 and 75 days' earnings, and more than 75 days' earnings.[48] Thus, we divide the sample by the 'intensity of treatment'; expecting that if it is the UI benefit reform which affecting job search behaviour, we should see increasingly large responses. It is important to keep in mind that the 'bite' of the reform is inversely proportional to prior earnings and directly proportional to the stability of previous employment.

Table 12 Proportion found a stable job in the primary labour market 2012 vs 2011, by value of UI benefit loss

	Low loss (1-44 days)	Medium loss (45-74 days)	High loss (75+ days)
within 183days	0.0491***	0.0968***	0.1480***
	(0.0123)	(0.0126)	(0.0134)
within 366days	0.0045	0.0406**	0.0504***
	(0.0120)	(0.0132)	(0.0142)
N treated	3300	3413	3763
N controls	3675	3590	3301

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

Table 13 Days worked and labour income over months 1-24, 2012 vs 2011, by value of UI benefit loss

	Low loss (1-44 days)	Medium loss (45-74 days)	High loss (75+ days)
Days employed	20.46***	18.28***	26.50***
	(6.069)	(6.241)	(6.458)
Labour income	66.08**	73.98**	154.89***
	(29.17)	(37.02)	(65.32)
N treated	3300	3413	3763
N controls	3675	3590	3301

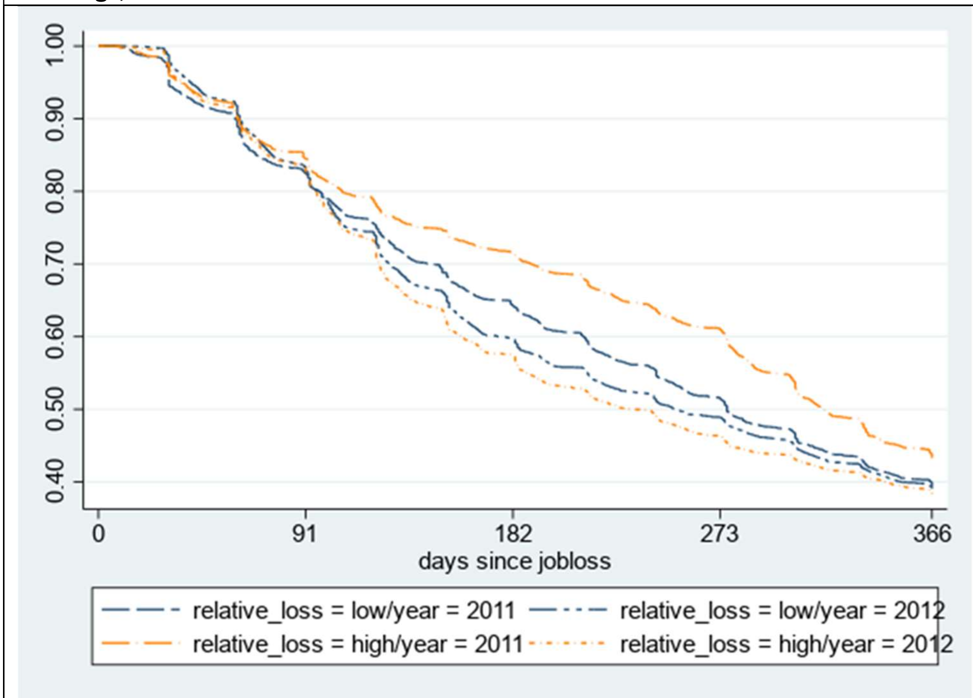
Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based

[48] This distributes the sample into roughly three equal sub-samples.

algorithm (following Huber et al. (2015)).

The main results are evidence from the Graph below. In 2012, all groups have a very similar survival curve, with a large increase in job finding after 3 months of non-employment. In 2011 however, the survival curves are markedly different, with those with longer PBD and lower daily earnings having long non-employment spells. In unreported results, we show that the in the group that experience the largest cut in the value of UI benefits, there was a large immediate effect, they were employed for more than an additional month during the first year after job loss.[49] We also show that only about 60 percent of these days were in a job where they earned more than 80 percent of the daily minimum wage and that about every eight day of this increase was due to public works contracts. While there was a small increase in days worked in the second year after job loss, this was minimal. By contrast, the effect on days employed for those with a small 'bite' of the UI reform was very modest, about 6 additional days spent in employment. Looking at re-employment wages, we find no significant change due to the reform, however.

Graph 3 Kaplan-Meier estimates of time in non-employment, by UI benefit loss relative to previous earnings, 2011 and 2012



Our third heterogeneity check is inspired by the model in Nekoei- Weber (2017). They use a simple job search model with negative duration dependence to derive predictions about the magnitude of wage (and nonemployment) effects of PBD extensions. In line with their model, one can expect that the PBD cut is more effective for those with lower (ex ante) expected probability of benefit exhaustion in the absence of the PBD shortening, but the potential wage

[49] Notice that this group lost almost 6 months' UI benefit eligibility, thus they were far from moving to jobs immediately upon benefit exhaustion.

loss is the smallest for this group of workers. By contrast, those with lower (ex ante) job finding probability will only be able to find a job quicker at the expense of a large wage cut. In other words, there is a correlation between the (positive) effect of the PBD cut on reemployment and the (negative) effect of the PBD cut on reemployment wages.

We implement this idea in the following way. First we use UI benefit claimants from 2010 who were selected along the same lines as our sample. We use a host of characteristics to estimate the probability that they were re-employed within 6 months after job-loss. Then we predict using this model the probability to find a job within 5 months for our sample of UI claimants (in 2011 and 2012). Finally, we divide UI claimants into terciles based on their predicted probability, and estimate the effect of PBD cut for each of these three groups.

Table 14 Proportion found a stable job in the primary labour market 2012 vs 2011, by predicted re-employment probability

	Low probability	Medium probability	High probability
number of days employed 1-24 months	20.464***	18.286***	26.506***
	(6.069)	(6.240)	(6.457)
N treated	3300	3413	3763
N controls	3675	3590	3301
change in wages, months 7-12	0.01738	-0. 0034	-0.0488**
	(0.0188)	(0.0167)	(0. 0179)
N treated	2210	2500	2918
N controls	2382	2541	2430
change in wages, months 19-24	0.0165	0.0109	-0.0042
	(0.0176)	(0.0167)	(0.0174)
N treated	2548	2749	3148
N controls	2734	2841	2698

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is the year of jobloss. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

Our results show that it was those individuals with the highest probability find a job quickly on their own who accumulated the most additional days of work as a result of the PBD cut. This represents a 6 percent increase in days worked. The other groups gained in terms of days employed, but to a slightly lower extent. In the Table, we show that indeed, for those with the best (ex ante) employment prospects, the cut in the PBD had a negative effect on re-employment wages in the short term, between 7-12 months. However, the 19-24 months after job loss, there are no significant effects.

Given that the (predicted) re-employment probability and the 'bite' of the UI benefit reform are negatively correlated, we also experimented with first performing a principal component analysis, then forming four quartiles and running the matching analysis in these four sub-groups. This analysis showed some interesting results: in the short run, the UI benefit had a significant effect on employment for all groups, but the largest effects were present for the two groups with large bite of the UI benefit (and low predicted re-employment probability). In the longer run, over 2 years, these two groups had 32 and 24 additional days employed, with two-thirds of these employment gains coming in the first year after job loss. At the same time, there was no significant effects on daily wages.

We also combined predicted re-employment probability and the 'bite' of the UI benefit reform by formulating groups for above and below medians of each variable, and then interacted these groups. For the group with low re-employment probability, but also low 'bite' of the reform (in other words, with moderate earnings and lower employment stability), the reform led to relatively small gains in employment, but their participation in public works contracts decreased significantly and they switched to (low-wage) jobs on the primary labour market. This led to a 4 percent increase in re-employment wages, primarily thanks to moving away from low-wage firms. For all three other groups, there were important gains in number of days employed, with some of these coming from public works contracts. The two groups for whom the bite of the UI reform was large experienced similar increases in days employed - though this meant a larger relative increase for the group with low predicted re-employment probability (those with lowest wages prior to job loss). For this latter group, the employment gains were accompanied by a 4 percent decrease in wages, with half of this due to having to take up employment at low-wage firms. Looking at overall income changes, the only group which experienced a positive total income change were those with high predicted re-employment probability and low 'bite' of the reform (essentially high wage individuals with moderate stable employment prior to job loss).

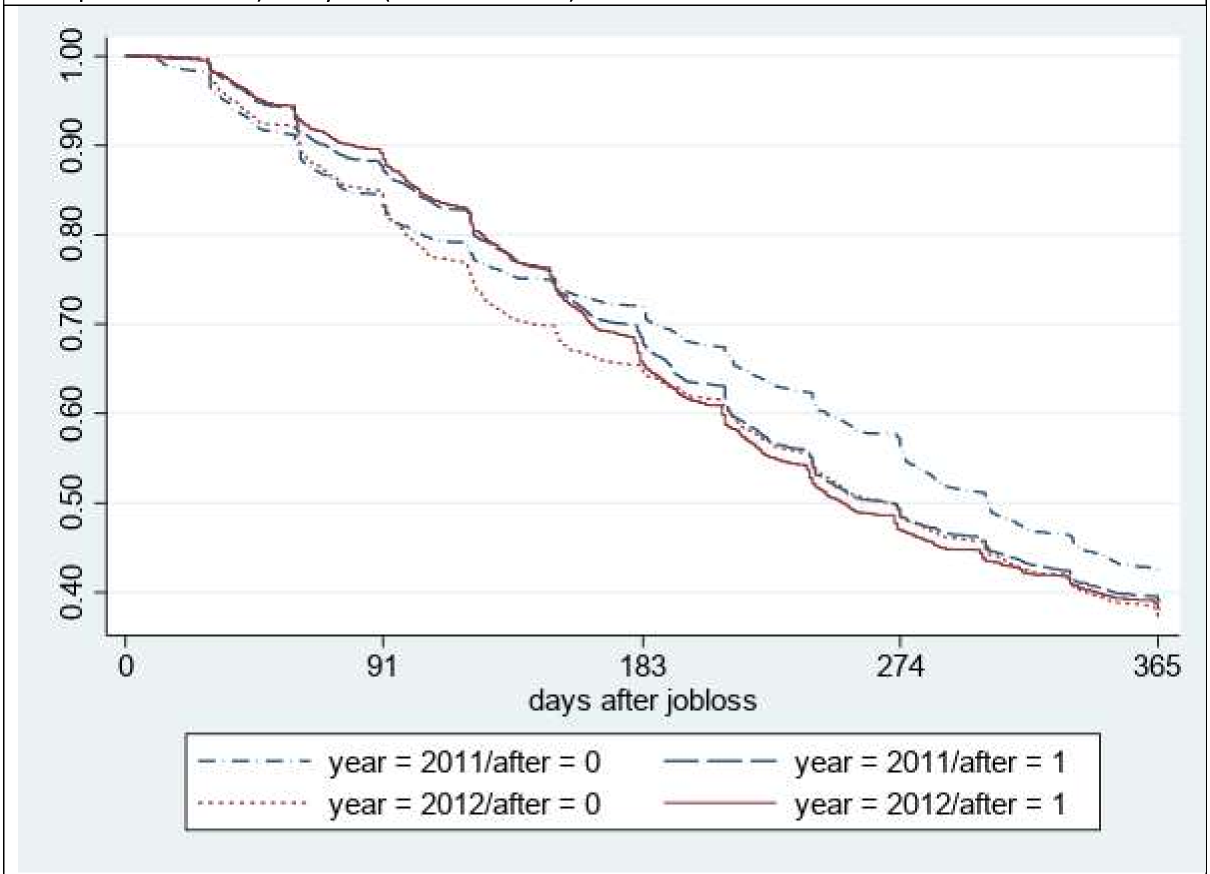
Robustness check: the effect on employment and earnings from around the cutoff date

Descriptive evidence

We first show the time-pattern of job losses (employment spell endings) as well as those of UI claim spells around the cutoff date of 1st of September 2011. It is important to remember that all the main elements of the reform, including the drastic shortening of the PBD were already implemented at this stage. It is also worth noting that while in the intermediate period the 'base period' was expanded to 5 years, we keep our sample constant, and that the individuals in our sample could not claim additional UI benefit days (as they have typically already reached the maximum days in the last 3 years).

We look at the months May-November of 2010-2012, and we divide days of the month into three periods 1-10; 11-20; 21-30 (31); thus we have 24 observations in each year. For each of these periods, we calculated (a) the number of individuals who ended their employment spell; (b) the number of individuals who started their UI benefit claim. In panel (a) we see that there are no marked patterns in job endings in 2011 which would substantially differ from the two neighbouring years. However, in panel (b) we can see that in the last ten days of August 2011 there was a large surge in UI benefit claims; furthermore, we can also see a drop in UI benefit claims in the first ten days of September 2011.[50] Further (not reported) analysis shows that those who claimed UI benefits in the last days of August 2011 were individuals with long employment histories, who had the most to lose from the benefit reform.

Graph 4 Kaplan-Meier estimates of time in non-employment, by inflow cohort (July 1st to Aug 20th vs Sept 11 to Oct 31) and year (2011 and 2012)



In the Figure below, we display Kaplan-Meier estimated survival graphs, separately by year and by UI benefit entry 'cohort'. We show the results for a 'tighter' cutoff (1st of July-20th of August vs 11th of September to 31st of October 2011).[51] There are a number of interesting observations to be made. First, that there is a clear seasonal difference in exits to jobs during the first 3 months after jobloss between the 'after' and 'before' groups. In particular, those who entered UI benefits around July had a significantly higher probability of finding a job

[50] Indeed, the overall number of individuals starting their UI benefit spell in the period between 21st of August and 10th of September hardly changes across the years, with around 730 individuals in our sample. What is remarkable is that slightly more than half of those entries is before the 1st of September in 2011, while less than a quarter of entries fall into those 10 days in the two adjacent years.

[51] Pls note that exits to stable regular employment contracts are considered sucessful job finding (public works exits are considered censored).

quickly (both in 2011 and 2012) as opposed to those who enter UI benefits around October.[52] The second and most significant observation is how the survival graphs of the 'after' group of 2011 very closely mirror those of the 'after' group of 2012, by 9 months after jobloss around 52 percent have been reemployed. By contrast, the time-patterns of the 'before' groups differ markedly across the two years, and this difference start appearing immediately after 3 months of non-employment. Indeed, by 6 months after job-loss, there is around a 7 percentage point difference in the estimated survival probability, which remains stable until 9 months after job-loss, but is slightly reduced to around 5 percentage points after 12 months. This is consistent with a group of individuals being able to find a job immediately upon benefit exhaustion after the UI benefit reform, who likely would not have searched for a job if they had longer potential benefit duration. [53]

Matching results

In what follows, we show the results for the 'tighter' cohort , however the results are qualitatively unchanged for the broader cohort definitions. First, we find that in the short run, there is roughly a 5 percentage point effect on the re-employment both at 6 months and 12 months after jobloss. Second, we find a remarkable seasonal pattern of effect on days employed, with no effect on months 1-6 and 13-18, but a strong (almost 20 percent) positive effect on days employed in months 7-12; and a weak positive effect in months 19-24. This translates, in terms of calendar months to roughly April-May 2012 and 2013.

Table 15 Effect of reform on outcomes aggregated over months 1-24 after job loss, sample around cutoff dates (2011 July1st to August 20th vs 2011 September 11 to October 31st)

	(1) Employment within 183 days	(2) Employment within 366 days	(3) Total days in 2 years	(4) Total earnings2 years	(5) Days W>minW*0.8	(6) Days in PW contracts
ATT	0.0517^{***}	0.0525^{***}	22.08^{**}	83.99	12.25^{*}	7.136^{**}
	(0.0135)	(0.0151)	(7.06)	(53.45)	(7.37)	(2.605)
Observations	15045	15045	15045	15045	15045	15045

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Table shows estimates of average treatment effect on the treated where the treatment is after UI claiming which started 1st of September 2011. The sample is composed of those starting claiming UI benefits between 1st of June and 19th of August 2011 versus those starting UI benefits claims between 11th of September and 31st of October 2011. The underlying matching algorithm propensity score matching combined with exact matching on county, and quarter of jobloss. The bandwidth for the Epanechnikov kernel is calculated with a pair-matching based algorithm (following Huber et al. (2015)).

In the next Table, we investigate the quality of employment and labour income. We find that while the increase in labour income was non-negligible (about 5 percent) it was not statistically significant. We find that this is due to the fact that roughly third of the increase in

[52] This consistent with having a job offer arrival rate which is lower in the months of November-March (the winter saeson) than in the rest of the year.
[53] However, given that the gap between the survival curves between 6-9 months is fairly stable also shows that there seems to be a group of UI beneficiaries who are not able to find a job quickly (even if their UI benefits have been exhausted).

days employed was in public works contracts (which is a large difference in relative terms). At the same time, the number of days worked in regular employment contract paying more than 80 percent of the minimum wage was much more moderate. In line with the above, there was a significant negative effect, about 6 percent on daily wages in the short run, in months 7-12 after job loss. Broadly speaking, this means that is difficult to separate the effect of the UI benefit cut from seasonal employment patterns, and hence this identification strategy needs further refinement.

Conclusions

We evaluated a drastic cut in the potential duration of benefits enacted in the aftermath of the Great Recession in Hungary. We used a sample of prime aged males with relatively stable employment history who lost 142 days' of UI benefits, on average. We find that the cut lead to a sizeable increase in outflows to employment after UI benefits have been exhausted, which meant however only 17 additional days spent in employment in the first year following job loss. However, the effect estimated here is relatively low compared to those found for benefit expansions in the earlier literature. Over a two-year period, the reform resulted in 23 more days spend in employment, and 8 percent increase in total labour income. This latter implies that on average, jobseekers lost income due to a decrease in the value of UI benefits was barely compensated for by the additional earnings. We find very small immediate negative effect of re-employment wages due to the reform, but this temporary decrease does not lead to a longer-lasting effect on wage trajectories.

The moderate overall effects hide large heterogeneities though. We find that those with tertiary education had large employment gains, but temporarily had to contend with lower wages. We also find that the employment effects of the reform was largest for those who suffered the largest loss in terms of the relative value of UI benefits. Thus, as expected, the 'bite' of the UI reform is positively correlated with employment gains. It also not surprising that we find relatively large employment gains among those who were closest to the labour market (had a high ex ante probability to be re-employed quickly). Looking at further heterogeneities, we find that those low ex-ante re-employment probability, but from who the UI benefit cut was particularly acute (essentially those with low earnings but very stable employment prior to job loss) worked much more after the reform, but they had to move to low-wage jobs at low-wage employers.

In terms of overall income effects, we find a large decrease in income over a 2-year period for those with education levels lower than upper secondary (representing more than 60 percent of our sample). By contrast, for those with tertiary education (less than 10 percent of UI beneficiaries) the additional earnings already in the first year after job loss more than compensated for the reduced UI benefits. Looking at this from a slightly different angle, those gained in terms of overall income, who were the closest to the labour market (had relatively high earning prior to job loss) but did not have a very stable employment history.

The investigation of heterogeneities also reveals that public works contracts had a non-negligible role even for our sample composed of individuals with a relatively stable employment history on the primary labour market. In particular, a group of individuals who had moderately stable employment paths and moderately earnings, the cut in the PBD lead to a move away from the public works contracts and to employment in the primary labour market resulting in wage increases. This does raise an issue of seasonality, the additional five months (potentially) spent on UI benefits prior to the reform meant that they would exhaust their UI benefits in November-December of a given calendar year, just 2-3 months before the typical timing of inflows into public works. By contrast, the group after the reform would typically exhaust their UI benefits in late Summer, when there were typically no more public works contract available. This does imply a further avenue for investigation: to explore heterogeneity in the effect of the PBD cut by in micro-regional public works intensity.

The fact non-negligible proportion of UI beneficiaries were able to find work relatively quickly without having to revert to low-wage work implies an important role for negative duration dependence of wage offers (a phenomenon also found for high-wage individuals with stable employment in Lindner- Reizer (2019)). General equilibrium effect will need to be investigated further. If UI beneficiaries started to search for jobs earlier this could potentially affect two groups. First, those who had similar characteristics, but chose not to take up UI benefits. Some of these individual could potentially be more productive, and hence still find jobs easily, and earlier than when registered jobseekers exhaust their UI benefits. Some of them might however be overly optimistic, and for these individuals the increased competition in job search from registered jobseekers can have negative effects. Second, it is also possible that the increased job search activity of UI beneficiaries affects non-employed not eligible for UI benefits, if these individuals are substitutes in the labour market.

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Appendix

Table A1: Main features of the unemployment insurance benefits in Hungary, 2011

Benefit, time period	*Entitlement conditions*	*Length of entitlement*	*Minimum and maximum duration*	*Replacement rate*	*Minimum benefit*	*Maximum benefit*
2011 August						
Jobseekers Allowance, Phase 1	A minimum of 365 days of contribution payment in the previous four years	5 days of contribution payment = 1 day benefit entitlement; half of total entitlement length	Minimum 36,5 days; maximum 90 days	60% of taxable wage in previous 4 quarters	60% of taxable wage in previous 4 quarters	120% of the minimum wage applicable on the first day of benefit period
Jobseekers Allowance, Phase 2	A minimum of 365 days of contribution payment in the previous four years	5 days of contribution payment = 1 day benefit entitlement; half of total entitlement length	Minimum 36,5 days; maximum 270 days	Flat rate: 60% of the minimum wage applicable on the first day of benefit period	-	-
2011 September						
Jobseekers Allowance	A minimum of 365 days of contribution payment in the previous five years	10 days of contribution payment = 1 day benefit entitlement;	Minimum 36,5 days; maximum 90 days	60% of taxable wage in previous 4 quarters	60% of taxable wage in previous 4 quarters	100% of the minimum wage applicable on the first day of benefit period

Figure A1: Benefit entitlement length in days as a function of previous work histories, 2011

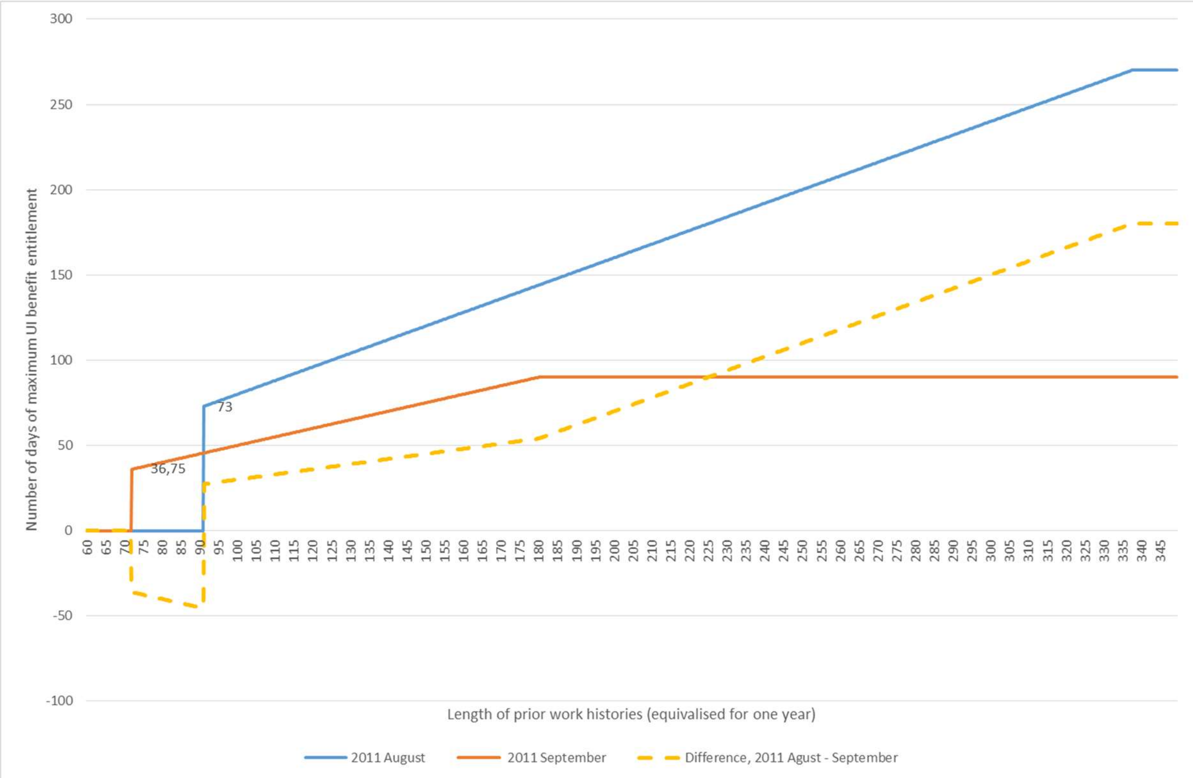
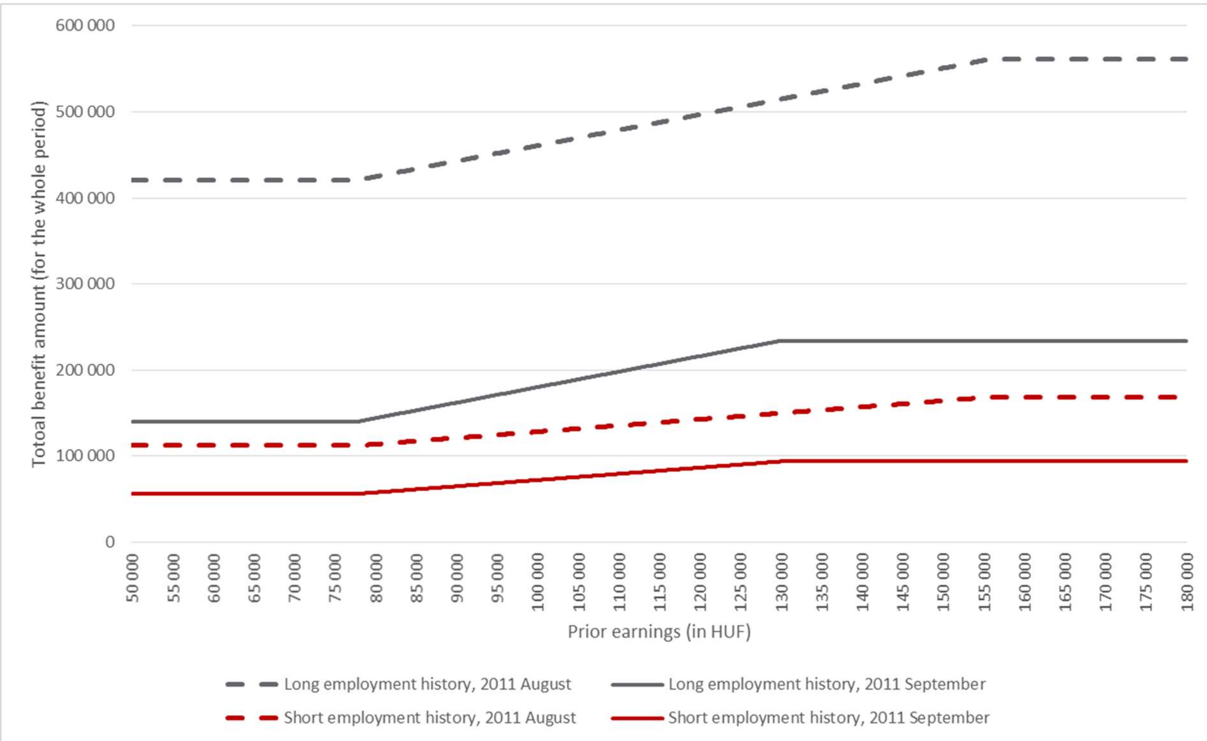


Figure A2: Maximum potential UI benefits, 2011, as a function of base earnings, by length of previous work history



Does cutting the value of unemployment insurance benefits affect take-up? Evidence from Hungary.

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Abstract

We evaluate the effect of a drastic cut in potential benefit duration, reducing the maximum length of UI benefits from 9 to 3 months in Hungary at the end of 2011. We rely on rich longitudinal matched administrative data, which allows us to obtain information on a large sample of job losers eligible for UI benefits. We show that the number of UI benefit claims fell only slightly, but this effect was more pronounced for those with the largest potential losses in UI value.

Introduction

The fact that many of those eligible for social benefits do not claim them is a widely known and studied phenomenon, however, much less is known UI benefits. The Great Recession and the rise of long-term unemployment in its wake has once again directed attention on the design of unemployment insurance benefit systems. An often neglected phenomenon when analysing the incentive effect of UI benefits is the propensity to claim benefits – which is related to the generosity of benefits. This however is important from a scientific point of view since if one does not take into account that decreased generosity can substantially alter the number and pool of UI claimants, one is likely to underestimate the incentive effect of results due to this selection. The effect of a drastic UI cut on non-employed welfare might also be exacerbated if for a large portion of eligible persons, the costs associated with claiming exceed the value of UI benefits. However, claiming costs might not be the right tool to screen those most in need of UI benefits, as those who do not face such high costs might also be those who find jobs relatively easily.

We evaluate the effect of the value of UI benefits on take-up, using a drastic cut in the unemployment insurance benefits which happened in 2011 in Hungary, when the maximum length of entitlement was slashed from 9 to 3 months. There are two important aspects that is worth noting. First, prior to the cut in potential duration of benefits, the UI scheme was composed of first tier benefit which was proportional to previous earnings (which lasted at most three months) and a second tier which was a fixed (low) rate. The benefit reform effectively slashed the second tier benefits. Second, that the reform not only meant that an individual had to accumulate 10 days of employment to qualify for 1 day of UI benefits; but introduced the 3 month maximum rule. This has the following implications. First, that the subjective value of UI benefits for those who expected to be re-employed quickly did not fall by as much as for those who expected a long spell of non-employment. Second, the fall in the value of UI benefits was particularly large for those with very stable employment. Third, the fall in value of UI benefits relative to prior earnings was less severe for high wage individuals. We leverage these last two results to estimate the effect of the drop in the value of UI benefits on UI benefit take-up.

The reform was discussed for a considerable time before its introduction, and it was voted in parliament seven weeks pior to it taking effect. An important point of the Hungarian system is that the UI benefit scheme which applies to a given UI spell is related to the day on which the UI benefit claim is made, and not to the last day of employment. Thus, a number of persons the value of UI benefits could drop if they waited too long to file their benefit claim. This could be related to high frictions in claiming benefits or lack of information on the severity of the reform. We also use this timing to shed light on factors determining claiming frictions.

Changes in the unemployment benefit policy in Hungary

In Hungary, the unemployment benefit scheme is traditionally not very generous. In 2010, the net replacement rate of unemployment benefit (defined as the ratio of an average production worker's net benefit during the first month of unemployment to their previous net monthly wage) was around 41% according to Esser et al. (2013). This number was the 6th

lowest in the European Union where two-thirds of members states had 50% net replacement rates or above, and close to half of them had 60% or above. This was primarily due to a low benefit cap in Hungary: daily benefit was calculated as 60% of mean daily earnings from the last four quarters before job loss with a maximum amount of 1.2 times the minimum wage. [54]

During the period between 2005 June 1st and 2011 August 31st, the UI benefit system worked as follows. First, the number of eligible base days (working days) were converted with a 5:1 ratio (thus 5 eligibility base days[11] counted for 1 UI benefit day); with the maximum benefit duration being 270 days. The base eligibility period was four years, and the minimum number of insured days to qualify for UI benefit was 365. Second, there were two periods of UI benefits, a first one, proportional to pervious earnings, and a second one, with flat-rate benefits – the unemployment assistance. The first period was equal to half potential duration of benefits, with a maximum of 91 days, and the daily benefit amounted to 60% of earnings from the previous year. The second period ('unemployment assistance' for the remainder of the potential benefit duration) paid a flat-rate set at 60% of the minimum wage in 2011.[55]

In 2012, the Hungarian government decided to cut the unemployment benefit even further. It was a complicated reform which, at its core, introduced four changes to the regulation, with the most significant changes affecting the length of the potential duration of UI benefits. First, for each day of benefit, the number of required eligibility base days[56] were doubled, thus 10 working days make the beneficiary eligible for 1 day of benefit. Second, the maximum number of benefit days dropped to 90 from 270 during the reform, and the flat-rate benefit period (the unemployment assistance) was abolished.[57] Third, the base period for eligibility shrank to 3 years from 4 years before, with a 360 minimum insured days for qualification. This meant that after the reform people who worked consistently years ago but started working more erratically in the recent past had a lower chance of eligibility for unemployment benefit.

In contrast to the changes to the potential benefit duration, the daily benefit amount was modified only slightly. Specifically, the daily benefit cap changed from 1.2 times the minimum wage to the amount of the minimum wage. Note however, that the nominal maximum daily benefit did not change significantly from 2011 to 2012, as the reform was accompanied by a substantial, 119.2% increase to the minimum wage. All other rules regarding the calculation

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[54] This has been the case ever since the UI benefit system has been instated with the fall of communism. See Micklewright – Nagy (1994).

[55] There were a few other features of the benefit system which are worth noting. (1) The reference date for calculating UI benefits was the day when the jobless individual registered as unemployed. (2) Voluntary quits entailed a waiting period of 90 days. (3) If a person was on UI benefits during the base eligibility period, these days were not directly subtracted from the potential benefit duration, rather they were subtracted from the eligible base days (with 1 day of UI = 5 insured days). (4) There was a re-employment bonus scheme was in place with a bonus amount equal to 50% of the remaining total first-tier benefits, if the individual found a job on her own. However, this meant that if the bonus was claimed, all remaining benefit days were annulled.

[56] Essentially, these are days when the individual was insured. There are some complications, however. First, days when was on long-term sick leave (a) do not count as base days, but (b) the extend the base period for calculating eligibility. Second, days when the individual did not receive pay (due to missing work, for workplace temporary shutdown, for unpaid leave) do not count towards base days.

[57] Note that a means-tested minimum income benefit still existed, eligibility however was set at a very low treshold.

of daily benefits were unchanged. It is based on daily earnings during the last four calendar quarters (prior to the initiation of the UI benefit claim), where total monthly earnings were divided by the number of days employed. The daily UI benefit is equal to 60% of the daily earnings in this base period, with no (daily) minimum, but a very low daily maximum (as highlighted above).

Another channel through which the reform impacted people is the reduction in the base period. The new regulation looks back only on 3 years of job history (instead of 4) to determine the number of benefit days the jobseeker is eligible for. 7.7% of our sample lost at least two days of benefit due to this change in 2012 (30% of those who were eligible for less than the maximum days). [58]

This already complex reform was further complicated by a regulatory mistake. Most of the new rules were implemented in September 2011 except for the reduced base period, which was instead increased to 5 years in September, only to be reduced to 3 years four months later in January. This lead to an intermittent period in the last four months of 2011 where most of the reform was implemented (cutting the benefits of most people). Besides the straightforward consequence that more people were eligible to UI benefits due to the extended base period (during September-December 2011), the modification of the law gave an opportunity to game the UI benefit system. This was specifically possible for those with long stable unemployment histories, due to the fact that past receipt of UI benefits is not subtracted from current UI benefit entitlement days, rather it is subtracted from eligible base days. More specifically, it was possible to receive UI benefits in the Fall of 2011 based on working days from year t-5 and t-4; de-register and re-register (and claim UI benefits) in the beginning of 2012. In that period, the individual could use eligible days from years t-3 to t-1. For this reason, we decided to only include people in our sample if they spent their last year working.

Literature review

The take-up of UI benefits has been rarely studied until recently. It is however relevant for our paper for a number of reasons.[59] First of all, changes in benefit generosity can lead to shifts in number and composition of recipients. Second, changes in composition can be due to both observable and unobservable characteristics, which in turn can bias estimates of the effect of UI generosity on job finding rates. Finally, studying UI take-up is relevant if one is interested in the inequality-reducing effect of UI benefits: in principle, it is those who need the benefit the least who will choose to not apply for them; however, this is not necessarily always the case.

The early literature on the take-up of UI benefits used survey data (see Wandner-Stettner (2000)) to point out two important phenomena in the US. First, that slightly over half of those

[58] On average, they lost 13 days of benefit due to this change in regulation alone.

[59] Economic literature on benefit take-up originates back to the classic paper of Moffitt (1983) on the (means-tested) AFDC. He models the take-up decision as a function of ’welfare stigma,’ and potential welfare benefits, calling attention to the fact that welfare participants are a selected sample: either with low ’tastes for work, or low levels of ’stigma’. He estimates welfare take-up, and labour supply simultaneously, and finds that the ’stigma’ effect is flat (not proportional to the amount of benefit, that is) and the elasticity of take-up to benefit size is about 0.6.

who do not apply for UI (erroneously) believe that they are not eligible for UI. Second, that a non-negligible proportion of non-employed give the expectation to find a job quickly (possibly at the previous employer) as a reason for not filing. In fact this proportion is higher than those who give a response related to the (psychological) cost of filing for UI benefits. Using the introduction of phone-based and internet-based UI filing, Ebenstein-Stange (2010) do not find evidence which support the notion that it is time costs which limit the take-up of UI benefits.

Anderson-Meyer (2007) use the change in the taxation of UI benefits in 1982, as the threshold for taxable income became much lower, and the after-tax value of UI benefits decreased substantially. Their main results imply that a 10 percent increase in benefits imply a 2-2.5 percentage point higher take-up rate, while a 10 percent increase in PBD imply a 0.5-1 percentage point higher one. In a related paper, Meyer-Mok (2007) build a simple model to study the effect of different parameters of UI benefit design on the take-up decision. They show that UI benefits as well as potential benefits duration increases take-up rate; however, the latter ought to have a smaller effect that then latter, unless everybody believes that they will unemployed for at least as long as the PBD. Changes in different parameters also change the composition of those on benefit in terms of expected unemployment duration; and decreased benefit generosity does not necessarily mean that „new” non-claimants will have shorter (expected) duration than claimants (on average).

Blasco-Fontaine (2021) simultaneously estimate take-up and job finding using administrative data from France. Not only do they model ’fixed costs’ but they account for ’frictions’ (transaction costs) as well to model temporary non take-up[60]. In their simple model the unemployed person chooses the level of job search effort, and similarly has to make effort to claim benefits.[61] They also show that the pool of UI claimants depends on the correlation between UI claiming costs and job search effort costs. They estimate a structural model of UI take-up and non-employment duration (allowing for unobserved heterogeneity in both UI claim and job search effort costs), and calculate various elasticities (based on model simulations). They estimate a 1% higher replacement rate leads to: for claimants, total unemployment duration increases by 0.6% ; the rake-up rate increases by 1.3%; the total elasticity is 1.3% for the unemployment duration.

Most recently, a couple of papers call attention to the role that employers might play in UI benefits. Lachowska et al (2023) show that, given that UI benefits include an experience rating for employers, they point out that employers have a very important role to play in UI take-up. They show evidence that relatively “low quality’ employers tend to deter laid off

[60] They focus on men aged 30-50, with long PBD [30 months, the max.], to ensure that everybody is eligible, excluding only spells of 1 week or shorter. In France, only about 31% of those eligible claim UI benefits (eventually). Claiming can be delayed, about half of claimants apply within 1 week, but about 20% will claim only after 3 months (avg. duration of claiming is 6 weeks).

[61] Their model shows that more generous UI benefit leads to higher reservation wages, and hence longer non-employment durations for both claimants and (current) non-claimants, as the latter also have the option to claim benefits before finding a job.

workers from claiming UI benefits. In a European context, Khoury (2023) shows that at least some employers tend to lay off workers later, in response to a discontinuity in replacement rates of UI benefits at a job tenure threshold in France.

Data

Our empirical analysis is based on an individual-level administrative panel database from Hungary, owned by the Databank of the Centre for Economic and Regional Studies (see Sebők (2021) for a detailed description). The data cover half of the country's population aged 0-74 in 2003, who were randomly selected and followed-up until 2017.[62] The database consists of linked data sets of the pension, tax, and health care authorities and the public employment services (hereafter PES) and contains detailed individual-level information on employment and earnings history, use of the health care system, pension, and other social benefits. The PES dataset (Jobseekers' registers) contains information on all registered jobseekers, including UI benefits, and the employment histories required to calculate these. Linking the PES database to the databases of the pension and health care authorities enables us to observe individuals' background characteristics and employment histories of job losers (not only those registered as jobseekers at the PES) , which allow us to calculate precisely both their UI benefit eligibility, their potential benefit duration and UI benefits.[63]

Sample selection and characteristics

During sample selection, we needed to account for the effects of policy design flaws and the imperfections of the data generating process, while ensuring that the sample comprised of genuine jobseekers.

First, we took data on people aged 25-54 who lost their jobs in the first half of 2011 or 2012; thus, we removed all those who could have ended their contract strategically, since the regulatory changes to UI benefits became public knowledge around the end of June 2011. We also filtered out those who were not seeking jobs for one of two reasons. First, those who probably already found a job before the end of their current work contract, and started their new job at most one week after job-loss (similarly to Blasco – Fontaine (2021)). Second, we excluded those who were likely witing for a recall, thus those who returned to their prior employer within a three-month timeframe (see Köllő (2003) for a more detailed analysis of this phenomenon). Additionally, self-employed individuals were excluded because of the difficulty in determining whether their unemployment was due to job loss, a pause between contracts, or working off the books.

The sample was further restricted to individuals for whom benefit eligibility could be accurately predicted. This necessitated the exclusion of women, as they constitute the majority of parental leave recipients in Hungary and their benefit entitlements are therefore difficult to estimate. Additionally, individuals who were not employees in the primary labour

[62] For details, see Sebők (2021).
[63] In fact, having access to UI benefit records allowed us to fine-tune our calculations, and we are able to estimate these quantities with a margin of error or less than 5 percent.

market during their previous employment were excluded, as their work histories are often more complex and may lead to errors in the data generation process, resulting in less precise estimations of benefit eligibility. This exclusion was implemented not only because the focus of the study is primarily on employees, but also to ensure the accuracy and reliability of our estimates.

Practical considerations led to further adjustments of the sample. We excluded individuals who took the benefit more than 61 days after job loss, comprising approximately 10% of benefit recipients.[64] This was necessary to avoid incorporating the effects of the interim period between September and December 2011 during which most reform changes were implemented but the eligibility base period was extended to five years. To account for this interim period and eliminate the possibility of rent seeking[65], the sample was restricted to individuals with a relatively stable employment history, defined as those who worked at least 360 days in the past 12 months and received wages or salary for at least half of those days (a similar adjustment to what Schmieder, von Wachter and Bender (2016) made to drop those with fractured labour market histories). Additionally, outliers in terms of earnings, health variables, and potential available maximum benefit were excluded, as were jobseekers with very low estimated benefits or no entitlement period.

Evaluation strategy

Benefit take-up

In the first step of our analysis, we estimate UI benefit take-up equations, where our key parameters of interest are the changes in the potential benefit duration and/or the changes in the total value of potential UI benefits (which is calculated as the daily UI benefits*PBD). [66]

In our simplest specification, we estimate equations of UI take-up, by pooling years, of the following form:

$$UI_i^t = \alpha + \beta year_i^t + \gamma w_i^t + X'\delta + month_i^t + \varepsilon_i^t$$

Thus, we estimate the effect of the reforms as a simple year effect, while controlling for a host of background characteristics. One of our key variables is prior earnings, which is an indicator of the individual's productivity.[67] We also use a rich set of background characteristics: age, occupation of prior job, variables describing the place of residence and

[64] Note that this also amounts to leaving persons who quit their job out of the analysis, as they were subject to a 2 month 'waiting period' befoer becoming eligible for UI benefits. We did not see any spikes in UI benefit uptake at this point in time, so we can likely conclude that not many workers chose this option. It is worth mentioning that HUngarian Labour Law allows for the employment relationship to end by 'mutual consent' – not leading to the 2-month waiting period.

[65] Rent seeking was possible due to a regulatory mistake, allowing jobseekers to take the benefit twice: first in the fall of 2011 and then again in the first quarter of 2012.

[66] As we have discussed, daily UI benefits did not change substantially.

[67] This is included as piecewise constant, and we allow it to have a differential effect below/above the level equal to the threshold value of daily UI benefits. In different specifications, we allow this variable to enter the take-up equation in a quadratic form.

two indices describing health care spending in the previous year. It is worth noting that we specify the effect of background variables to be constant over the two years. In further specifications, as robustness checks, we also include individual fixed effects and firm fixed effects, which we estimate from an Abowd-Kramarz-Margolis two-way fixed effects regression (on data from 2003-2011)[68].

In the next set of regressions, we measure the intensity of the policy change, by calculating, for each individual, the total value of potential benefits based on the 2011, as well as the difference between the 2012 and 2011 rules. We include an interaction between the (absolute value) of this difference and the year 2012, so in other words, this variable takes the value zero in year 2011.

$$UI_i^t = \alpha + \beta^1 PBD_{2011_i} + \beta^2 (PBD_{2011_i} - PBD_{2012_i}) * year_i^t +$$
$$+\gamma w_i^t + X'\delta + month_i^t + \varepsilon_i^t$$

In this specification, we expect that $\beta^1 > 0$ and $\beta^2 < 0$, and we hypothesize that take-up is inversely proportional with the loss in PBD (or total benefit value). We assume that the error term follows Normal distribution (hence estimated probit equations), and estimate several specifications (with varying background characteristics).

Then, we estimate similar regressions, where the key explanatory variable is the total value of UI benefits (the daily UI benefit amount multiplied by the PBD). One further issue is the inclusion of daily UI benefits in this equation, as it is difficult to separately identify its effect from that of previous earnings. In fact, it can only be estimated relying on (a) functional form assumptions and (b) using the 'kink' in the benefit schedule in the neighbourhood of the benefit cap. Thus, we will not include daily UI benefits in most of our specifications.[69]

It is worth briefly discussing two aspects of our estimation methods. First, a central feature of our longitudinal matched employer-employee dataset is that it enables us to estimate Abowd-Kramarz-Margolis (1999) wage equations. These allow us to estimate individual and firm fixed effects (pertaining to the most recent employer); where the individual fixed effect contains all (time-invariant) determinants of earnings, including 'unobserved ability'; while the firm fixed effect pertains to the firm's wage setting strategy (controlling for the composition of workers). In essence, these can be used as (imperfect) measures of worker and firm 'quality', and we use these as controls in a robustness check.[70] Second, as similar

[68] Please note that the firm fixed effects were estimated on the whole sample period (2003-2017), in order to maximise the possible number of observations. However, it is likely that the sample for which it was possible to estimate firm fixed effects does not include micro firms.

[69] As a robustness check, we estimated a job finding equation simultaneously with the UI take-up equation, by allowing the error term to be correlated across the two equations. In this specification the UI claiming costs can be related to unobserved job search effectiveness terms. We estimated this specification using a bivariate probit model (hence assuming that error terms are distributed as bivariate normal). Results differed only slightly to those of the probit models estiamted, despite the fact that there is a negative correltation between the error terms of the UI take-up equation and the job-finding equation.

[70] We only use data from before the job loss, to exclude the possibility of endogeneity contaminating our estimates. More precisely, for individual fixed effects, we only use data inclusive of 2011, while for firm fixed effects, we use data for the whole sample period.

papers in the related literature, we assume that the effect of control variables does not change through time, or in other words, the reform effect can be captured by potential benefit duration or the monetary value of UI benefits.

Empirical results on UI benefit take-up

Descriptive evidence

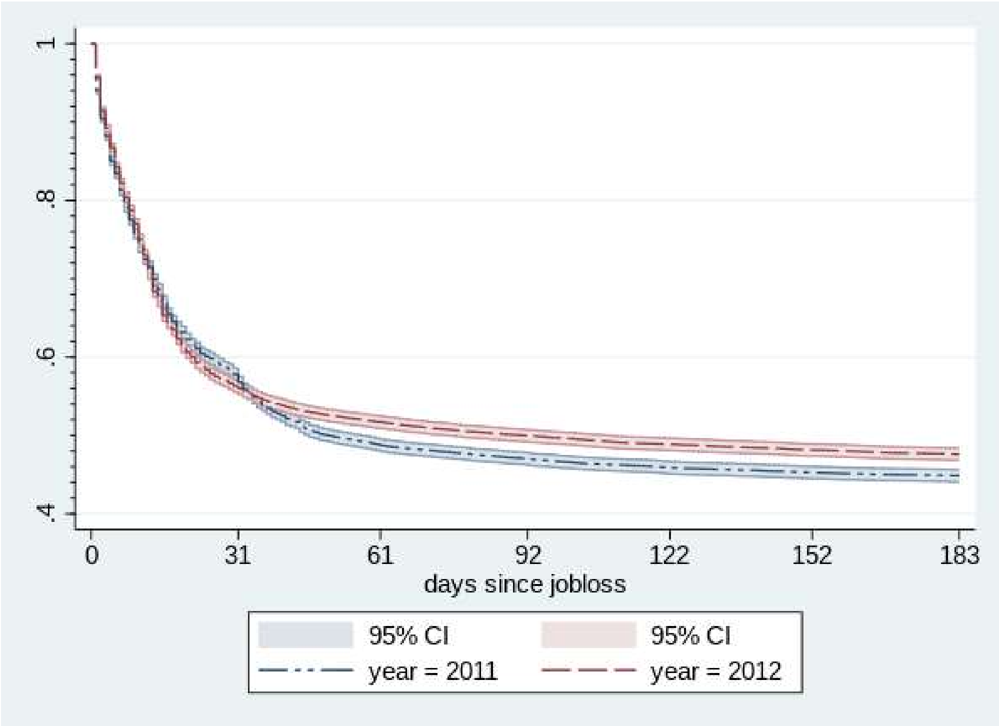
We first show the time-pattern of receiving UI benefits, where our initial interest is whether non-employed workers register as unemployed (and apply for benefits) very quickly upon job endings. Indeed, in our sample, both the proportion claiming is relatively high and registering as jobseeker is prevalent at the beginning of the non-employment spell. We show the survival in unclaimed unemployment for the initial 6 months period, separately by year in the Graph below: these show that in 2012, claimants tend to register slightly quicker. Overall, we can see that at the end of two months, close to 55 percent of UI eligible individuals in our sample registered as jobseeker in 2011, while this proportion was about 2.5 percentage points lower in 2012. It is also clear that this small difference remained stable in the months thereafter.

The graph also reveals that in that there were significantly more individuals registering very quickly after job loss in 2012, with 84 percent of those who will eventually contact the PES office doing so within one month (which is 6 percentage point higher than in 2011). However, after 45 days, there is no significant difference in timing, and by the end of two months after job loss, 93 percent of all those who decide to claim UI benefits already did so. [71] This is in stark contrast to Blasco and Fontaine (2021) where the show that for French prime-age males, roughly 20 percent of non-employed claim benefit only after 3 months of non-unemployment. Given this evidence, we decided to treat claiming benefit as a static decision, and we censor claiming at two months of non-employment (meaning that we do not use individuals who claim UI benefits after 62 days of non-employment). [72]

[71] Thus, the median duration to claiming is 10 (9) days in 2011 (2011), while the average is 20 (19) days.

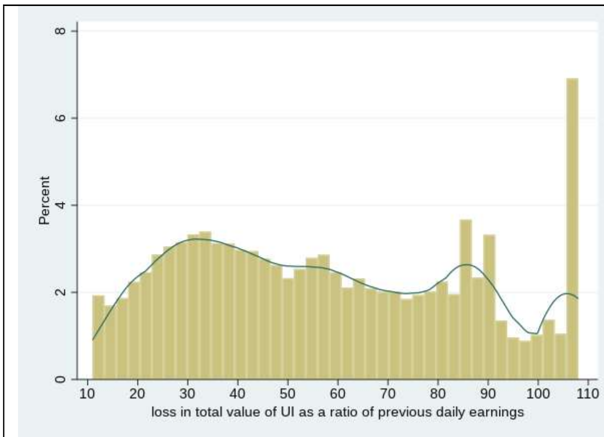
[72] Note that this is also done for a practical reason: we can avoid that individuals losing their jobs in June 2011 end up claiming after the initial reforms have been implemented.

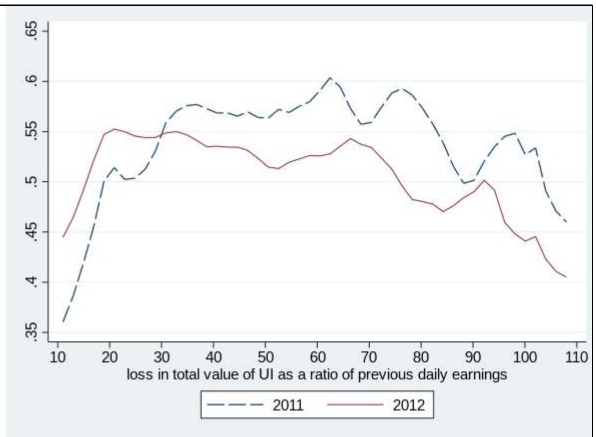
Graph 1: Survival in unclaimed unemployment



We now turn to some descriptive evidence on changes in claiming behaviour. In particular, we will use the value of UI benefit lost due to the policy change expressed as a ratio of daily earnings (during the last year before job-loss). We use this same measure for claimants in 2011, to show what would have happened to them had the reforms been implemented earlier (assuming no behavioural effect for the moment). In the Graph below, where we show local polynomial smoothing of claiming as a function of UI benefits lost, an interesting pattern can be seen. It is indeed those who were negatively affected where claiming dropped more, but the drop in claiming is not linearly related to the value of the UI lost. In the next Graph we show the distribution of potential claimants in 2012 as a function of days lost: we can see that a large proportion had to contend with 90-110 days' worth less UI benefits in 2012.

Graph2: Value of lost potential benefits UI benefits (function of daily earnings)





(a) distribution of sample by UI value lost	(b) Ui claiming as a function of lost value

In the next Table, we divide our sample of non-employed into five quintiles based on the value of UI benefits lost, and show some of their key characteristics. In the first column we show the value of UI benefits lost, and see that it varies quite of bit, with those in the top quintile losing almost 100 days' earnings as a result of the regulatory change, with the individuals 'losing' the least only lost the . The next column is of no surprise: it was low earners who lost the most due to the regulatory change (in relative terms). Please notice that those who lost the most due to the reform had earnings which were below the daily minimum wage, implying that they likely worked part time. It is worth a reminder about a prominent feature of the Hungarian UI benefit system: due to the very low UI benefit ceiling, the replacement rate among high earners is well below the nominal 60%. For instance, among those who lost relatively little, the replacement rate is on average around 40 percent only. [73]

Table 1 Mean values for 2012 jobseekers (except for last column), by quartiles of the ratio of maximum cumulated benefits to previous earnings

	Loss in UI (as % of earnings)	Previous earnings	N. of insured days over last 4 years	PBD 2011 rule	PBD 2012 rule	UI takeup rate in 2012	UI takeup rate in 2011
1	20,4	8972	885	177	71	0,533	0,566
2	36,2	4858	967	194	78	0,620	0,601
3	52,7	4354	1178	236	87	0,627	0,589
4	73,8	3407	1268	254	89	0,611	0,569
5	98,3	2581	1334	267	90	0,540	0,487

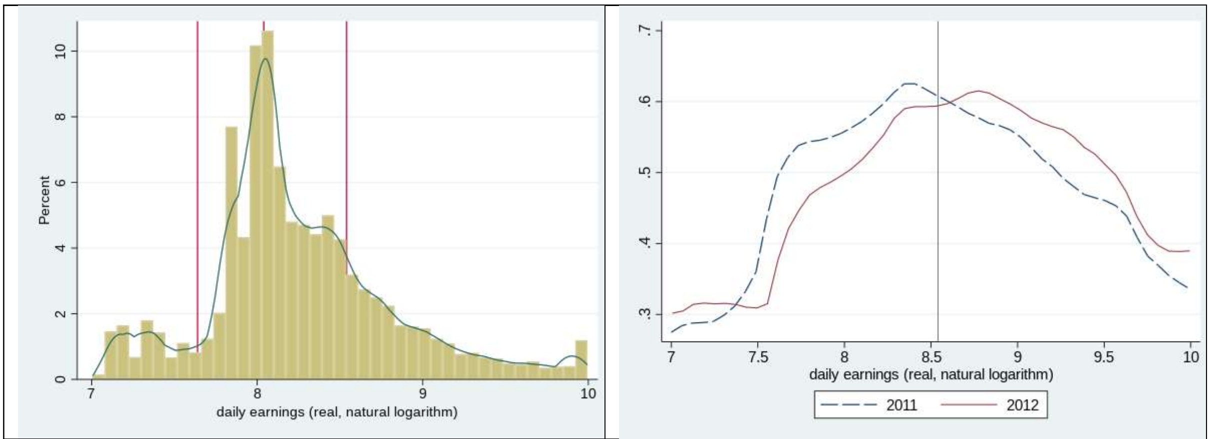
The next column shows the other factor driving the UI value losses: differences in the stability of employment histories. Those who lost relatively little, worked about 60 percent of the previous four years, while those who lost the most worked more than 90 percent of the days. The next two columns document just how drastic the cut in the potential benefit duration was. 60 percent of job losers would have been eligible for 8 months' (or more) of UI benefits in the 2011 regime, and lost at least 150 days on average. Even those who lost the least saw their PBD cut from 6 months to around 10 weeks. In the final two columns we display UI take-up rates for both years. This shows that there was no linear relationship between the loss in the value of UI and take-up rates. In the top two quintiles, take-up rates decreased by about 5 percentage points (or around 8%). In the second and third quintile, UI claiming rates decreased by about 2.5 percentage point (or 4%); while in the bottom quintile (eg. among high earners), UI take-up rates actually increased.

In the next graphs we show the distribution of (previous) earnings and its relation to claiming behaviour, where we display the natural logarithm of daily earnings (as used in the calculation of daily UI benefit amounts). In the first Graph, we can see two interesting facts. First, about one-fourth of the sample had earnings which put them above the UI benefit cap. Second, a

[73] Also note that the benefits lost were from the second, flat-rate portion of UI benefits.

fairly large portion of the sample (around 11 percent) earned lower than 80% of the minimum wage – thus they work in irregular jobs (public works), did not work full-time, or were on long-term sickness benefit (or did not receive pay for some other reason). In the second Graph, we show the UI take-up rate as a function of daily earnings, with four significant phenomena. One, that the UI claiming rate of low earners is only around one-third. Two, that UI claiming rises sharply with earnings, with 60% of individuals around the UI benefit cap registering as unemployed. Three, that take-up rate of UI benefits decreases for higher earners: among those earning more than 3 times the minimum wage (the top 12% of our sample) the UI claiming rate is around 42 percent. Four, that claiming decreased in 2012 (relative to 2011) for those below the UI benefit maximum threshold, while it increased for those above it.[74]

Graph3 : Value of lost potential benefits UI benefits (function of daily earnings)



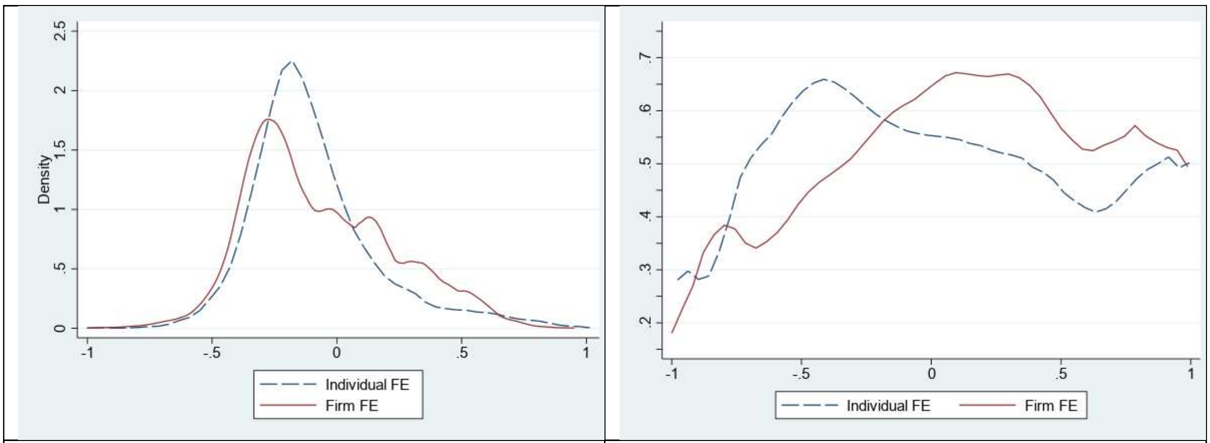
(a) distribution of earnings

(b) UI claiming by earnings

Finally, we also show the distribution of estimated individual and firm fixed effects from the AKM models, as well as how they relate to UI benefit take-up. In the left panel, we can see that – as expected – job losers' come from the lower part of the distribution of individual fixed effects, and they also tend to come from low paying firms (although with a somewhat larger dispersion). What is more remarkable (as shown in the left panel) is that the patterns of the take-up of UI differs markedly across individual and firm fixed effects. In terms of individual fixed effects, it seems that those who claim UI tend to be of "lower productivity" than the typical job loser, with the above-median job loser having a significantly lower probability of claiming. By ,contrast, it is those who lost their job at an above median paying (prodictivity) firm who take up UI benefits. Individuals who worked at a low-wage firm tend to apply for UI benefits much less by almost 20 percentage points.

[74] The median earnings of UI claimants increased by 5% in 2012 (relative to 2011). The difference in median earnings in 2012 between UI claimants and UI non-claimants was 16%, while it was only 8% in 2011.

Graph4 : Estimated individual and firm fixed effect and relationship to UI take-up



(a) density of individual and firm fixed effects	(b) UI take-up as a function of fixed effects

The determinants of UI take-up

In this section, we present a series of estimates of UI benefit take-up, formulated as a static probit model. We focus on two issues: (a) to present the main determinants of take-up, and (b) to estimate to what extent the decrease in PBD is associated with lower take-up rates in 2012. On average, the take-up of UI benefits was 2.77 percentage points lower in 2012, which represents a small, 5% decrease over 2011.

First, we discuss results where we allow for a simple additive effect of the year 2012, and we show a few alternative specifications (we display marginal effects). In the first specification we allow for a flexible (quartic) function of previous earnings and include a total value of UI benefits, as calculated based on the 2011 regulations. In the second specification, we add the total value of UI benefits in two parts: the daily UI benefits and the potential benefit duration (PBD, based on 2011 regime). In both specifications, the take-up rate is estimated to be lower by around 3.15 percentage points. Interestingly, the total value of UI benefits has a small positive effect on take-up. When decomposing the total value of UI benefits into PBD and daily UI, we estimate a very small positive effect for PBD, but a significant negative effect for UI benefits. However, this latter is estimated off of the ‘kink’ in the benefit schedule at the maximum benefit threshold, and hence is sensitive to functional form assumptions.

As for our control variables, we find large effects. There are very pronounced differences across occupations, with managers, professionals, former armed forces type occupations claiming UI benefits with 7-10 percentage points lower probability, while agricultural workers and unskilled workers claiming with about 10 percentage points higher probability. We also found large regional differences (up to 15 percentage points), and pronounced differences across micro-regions with lower and higher Roma minority.[75] We estimated a negative effect

[75] Moving from a region with the median proportion Roma (2 percent in our data) to one at the 9th decile (with a 7.3 percent Roma population) increases the UI take-up by about 4.4 percentage points.

for those with higher health care spending (and more GP visits) – which might imply that these individuals are more likely to seek alternative benefits.

Table 2 Take-up probit with 2011 eligibility days rule: year effect, marginal effects at the mean

	(1)	(2)	(3)	(4)
Year: 2012	-0.03* (0.006)	-0.03* (0.006)	-0.02* (0.006)	-0.51* (0.077)
Log mean past earnings, daily (4 calendar years)			0.09* (0.011)	0.03^{+} (0.012)
Interaction				0.06* (0.009)
Base controls	x	x	x	x
Other controls	Max. cum. ben	Avg. earnings + eligible days + estimated daily benefit	Log eligible days + log daily benefit	Log max. cum. ben
Observations	30115	30115	30115	30115

Robust standard errors in parentheses
$^{+}$ $p < 0.1$, * $p < 0.01$
The outcome variable is a dummy with value 1 if the jobseeker took up unemployment benefit. Control variables are suppressed for brevity.

In the next few specifications, we varied the functional form of a few key variables. First, we entered PBD in a logarithmic form, as well as prior earnings in a logarithmic form. We also added a spline in prior earnings, allowing it to have different effect below the minimum wage, and above the maximum benefit threshold. In this specification, the coefficient on the year 2012 is only about 2 percentage points, but we estimated a strong positive effect for PBD and we estimate a weaker association between earnings and take-up below the minimum wage and above the cap than in the intermediate region. In a slightly different specification, we added the logarithm of the UI value and allowed for the logarithm of earnings to have a differential effect across the two years. In this specification we find a slightly higher reform effect (2.8 percentage points), but also find that earnings have a much higher association with take-up in 2012.

Table 3 Takeup probit with 2011 eligibility days rule: intensity of treatment, marginal effects at the mean

	(1)	(2) Log displayed controls	(3)	(4) Log displayed controls
Estimated eligible days based on 2011 legislation	0.00^{*} (0.001)	0.03^{*} (0.009)		
Difference in eligibility days (2011-2012 rules)	-0.00^{*} (0.000)	-0.02^{*} (0.006)		
Estimated maximum cumulated benefit (2011-rule)			0.01^{*} (0.003)	0.09^{*} (0.009)
Change in max. cum. ben. defined for 2012			-0.01^{*} (0.002)	-0.05^{*} (0.007)
Base controls	x	x	x	x
+ Estimated daily benefit	x			
Observations	30115	30115	30115	30115

Robust standard errors in parentheses $^{+}$ $p < 0.1$, * $p < 0.01$

The outcome variable is a dummy with value 1 if the jobseeker took up unemployment benefit. The natural logarithm of main control variables (presented in the table) are included in models (2) and (4) while coefficients of the non-transformed scale are presented in models (1) and (3). Other controls are suppressed for brevity.

Now we turn to estimating the effect of the intensity of the reform. First, entering the differences in PBD, we find that indeed, those who lost more days were less likely to claim benefits. However, this effect is relatively small - moving from a small value of 100 days lost (P25) to the typical 180 days lost leads to an around 1.6 percentage point decrease in claiming probability. Second, logarithmic specifications of PBD imply slightly larger effects. Third, using the total value UI benefits lost leads to slightly higher estimated effects of the reform intensity. Fourth, estimations using the logarithm of the total value of UI benefits also yield similar results.

Thus, our results on UI benefit take-up lead to two over-arching results. First, that differences in background characteristics of non-employed across the two year do not explain why we only get a very small ‘reform effect’ on UI claiming behaviour. Second, that while the loss in PBD (or the total value of UI benefits) - in other words, the intensity of the ‘treatment’ – is associated with lower UI claiming, but this effect is relatively weak.

Conclusions

In this paper, we shed light on the determinants of unemployment insurance benefit take-up in Hungary. We are the first to calculate UI benefit non take-up rates in Hungary, using a large sample of matched administrative datasets, where we can reliably estimate UI benefit eligibility. We show that even for a sample of prime-age male workers with stable employment about 45 percent of those eligible do not apply. At the same time, we find that individuals do not tend to postpone (potentially hoping for quick re-employment) to apply for UI benefits, 93 percent of those who eventually apply for UI benefits do so within two months of job loss.

We used a drastic cut in the potential duration of benefits to evaluate to what extent monetary incentives affect UI benefits take-up. We only find moderate effects. On the one hand, take-up rates of UI benefits decreased by about 5 percent. On the other hand, the composition of UI beneficiaries changed, with those for whom the value of UI benefits relative to their prior earnings decreased the most foregoing UI benefits the most. As this meant that low-wage individuals with stable employment did not take up benefits, a group who is very unlikely to have savings. We also found a role for firms in UI benefit take-up, with those losing their jobs at high paying firms applying UI benefits in a higher proportion. This is a phenomenon which needs to be investigated further.

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Appendix

Table A1: Main features of the unemployment insurance benefits in Hungary, 2011

Benefit, time period	*Entitlement conditions*	*Length of entitlement*	*Minimum and maximum duration*	*Replacement rate*	*Minimum benefit*	*Maximum benefit*
2011 August						
Jobseekers Allowance, Phase 1	A minimum of 365 days of contribution payment in the previous four years	5 days of contribution payment = 1 day benefit entitlement; half of total entitlement length	Minimum 36,5 days; maximum 90 days	60% of taxable wage in previous 4 quarters	60% of taxable wage in previous 4 quarters	120% of the minimum wage applicable on the first day of benefit period
Jobseekers Allowance, Phase 2	A minimum of 365 days of contribution payment in the previous four years	5 days of contribution payment = 1 day benefit entitlement; half of total entitlement length	Minimum 36,5 days; maximum 270 days	Flat rate: 60% of the minimum wage applicable on the first day of benefit period	-	-
2011 September						
Jobseekers Allowance	A minimum of 365 days of contribution payment in the previous five years	10 days of contribution payment = 1 day benefit entitlement;	Minimum 36,5 days; maximum 90 days	60% of taxable wage in previous 4 quarters	60% of taxable wage in previous 4 quarters	100% of the minimum wage applicable on the first day of benefit period

Figure A1: Benefit entitlement length in days as a function of previous work histories, 2011

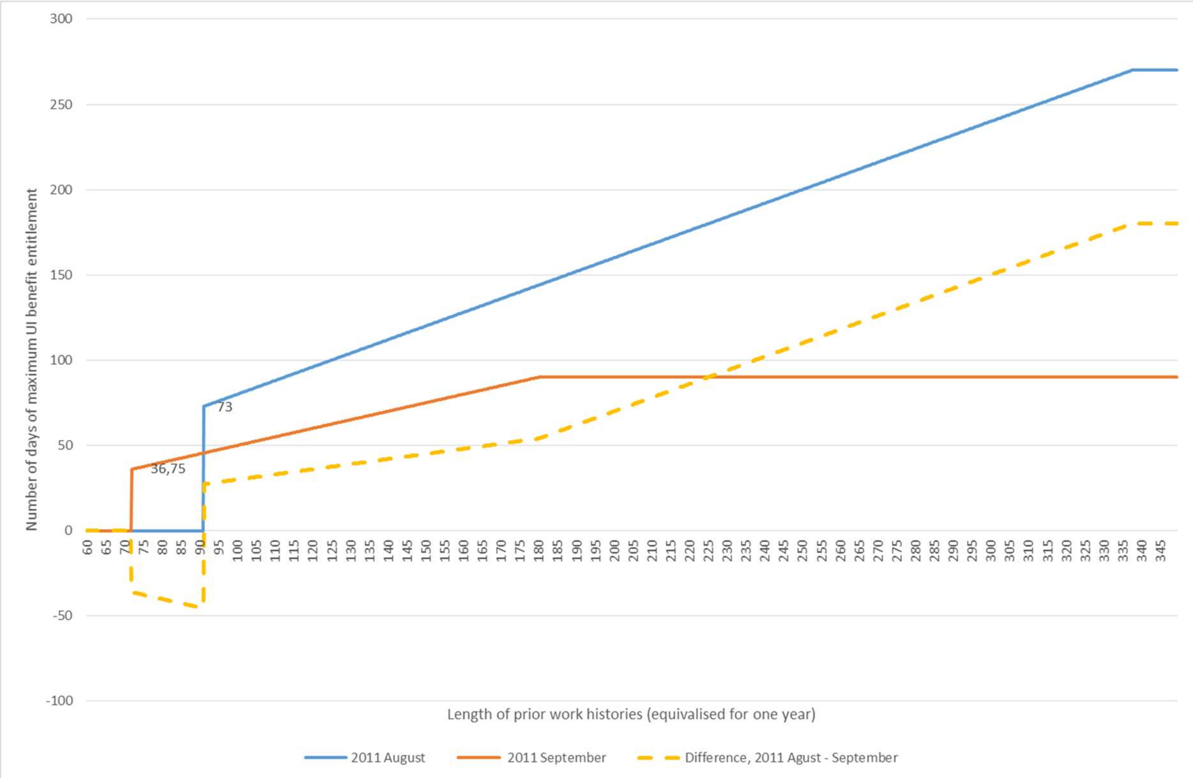

Figure A2: Maximum potential UI benefits, 2011, as a function of base earnings, by length of previous work history