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Surprisingly small: effects of “generous” social benefits on re-employment of (quasi-) jobless households

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Abstract

This article analyses whether the extent of public welfare provision influences the speed at which (quasi-) jobless households get (back) into employment. (Quasi-)joblessness is one of the key criteria defining the risk of poverty and social exclusion in the EU. Moreover, the perceived tension between the main functions of social benefits (protecting households from poverty and incentivising job search) is most acute among these households. Based on EU-SILC data, we examine changes of household work intensity during one year after benefit receipt. We observe that “more-generous” social benefits have a slightly negative impact. This can potentially be due to a disincentive effect of social benefits, but it can also mean that the additional financial leeway is used by job seekers to wait for more adequate job offers or engage in further training. Even though statistically significant, the estimated negative effects are very small.

Keywords: Social benefits; social investment; social policy; employment incentives; joblessness; work intensity

Introduction

Criticism of “generous” welfare states has, among other issues like fiscal sustainability, pointed at a possible behavioural change of transfer recipients. The present article seeks to add to the empirical knowledge by analysing intertemporal effects of social benefits on (re-)employment: Looking at (quasi-) jobless households, we test whether “generous” social benefits impair the evolution of household work intensity over time.

We can distinguish three competing paradigms of social protection, two of them being welfare-pessimist: The neo-liberal “making work pay” perspective (OECD, 1994) sees the benefit recipient as a homo-economicus who weighs the value of leisure time against wages earned through work. From this perspective, social security policy is faced with a genuine dilemma: poverty alleviation in the short run, through “generous” benefits, may discourage the take-up of job offers and thus prolong income insecurity. Hence, a balance must be found between “generosity” and efficiency, by ensuring a sufficient gap between benefits and potential net earnings. The second (conservative) paradigm believes that exits from poverty depend mainly on the recipients’ personal responsibility and effort. Therefore, efficient social security systems should include strict conditionality, close monitoring and sanctions to maximize job search efforts. Failing to do so would allow a *dependency culture* to emerge and to be transmitted to the next generation (Heinemann, 2008). Like the neo-liberal paradigm, the conservative view discommends a high extent of welfare provision.

By contrast, the Re-InVEST¹ project's capability- and human rights-oriented view of *social investment* can be situated in the tradition of a *welfare resources* perspective, "concerned with how collective resources and services stimulate and enable social participation" (van der Wel & Halvorsen, 2015, p. 101). It considers that people generally want to leave the state of unemployment or poverty and wish to actively take part in society. It further considers that they are sometimes hindered from doing so, eg. by a lack of education, poor health, family responsibilities, etc. In addition to active labour market policies, this approach prescribes adequate social benefits as a means for the recipients to continue investing in themselves (eg. through further training, health care and mobility) and their family members (eg. through childcare and education). Unemployment insurance is thus considered as a "subsidy" to workers (Gangl, 2004, p. 1336; Gangl, 2006, p. 991), used to promote their employment careers.

Welfare-pessimist and welfare-optimist paradigms thus have diverging interpretations of individuals and their motivations and yield irreconcilable social policy recommendations. Interestingly, however, they do not necessarily lead to different predictions concerning the impact of social benefits on the duration of job seekers' unemployment spells. No matter whether the explanation is sought in "capability maximization," acquired incapacity to work, or job seekers' motivation to find a job which is decent and adequate – "generous" social benefits can be expected by all three paradigms to foster longer phases of unemployment, at least in the short and medium runs.

As we will see in the next section, there are empirical studies on the matter in many different countries. After reviewing the state of empirical knowledge, we will describe our own approach and then present and discuss our results in the light of the existing research.

State of knowledge: social benefits' impact on re-employment

All modern countries have put in place *some* kind of social benefits to support job seekers. It is thus not the consequences of benefits as such, but of their "generosity" which have been scrutinized by empirical studies. "Generosity" can refer to a (high) level of benefits, (long) duration of the benefit period, as well as (broad) coverage and (low) conditionality (cp. Otto, 2017, p. 3).²

A large number of empirical studies have been conducted on the former two aspects, namely level or duration of benefits. Usually, studies are on one specific country. Their reasoning is often based on the idea that monetary incentives support transitions into work ("neo-classical search theory," see Tatsiramos, 2009, p. 1227). Studying the effect of benefit "generosity" ideally requires a measure that varies across individuals. There are three main ways of how empirical variation of social benefits is captured: either in terms of individual net replacement rates (NRR) or in terms of an analysis of what happens when benefit periods end, or on the occasion of a benefit reform. As we will argue, all these approaches have their specific merits but also their difficulties.

The NRR is the net income difference between a scenario of continued (partial) unemployment and a scenario of (additional hours of) employment. Often, it is the difference between 0 and 40 working hours which is analysed (Savage, Callan & Walsh, 2015, p. 126). Based on that method, Guzi (2014, p. 411) "demonstrates the existence of a welfare trap" in the Czech Republic, ie. a situation where "social benefits are accepted as an alternative to low and insecure earnings." (Guzi, 2014, p. 411). Similar findings for the case of Germany are presented by Schneider & Uhlendorff (2005). A problem of working with the NRR concept lies in the necessary assumptions on the potential earnings of job seekers: these cannot be determined with certainty by the researcher, nor are they necessarily known to the job seeker. Another difficulty of the NRR-method lies in determining the loss of other income as soon as (additional)

¹Re-InVEST stands for 'Rebuilding an Inclusive, Value-based Europe of Solidarity and Trust' (see www.re-invest.eu). The research leading to this article was done in the context of this project, funded by the European Union.

²The term "benefit generosity" can be criticized as inadequate due to its moral connotation. This holds in particular in the face of structural unemployment or if social benefits derive from (formerly earned) social insurance rights. Yet, the term is well established and easily understandable; we thus keep it and use it in quotation marks.

employment income is earned: a smaller gross income of the partner, an increase of taxes imposed on it, expenses for childcare and travel to work (Savage, Callan & Walsh, 2015, p. 126 et seq.).

The second way of observing variance in benefits is to exploit the “natural” variation that exists between the periods before and after expiration of the benefit. The “change in benefit levels” is anticipated by receivers of benefits and studies use this to model the impact of social benefits on their behaviour. As Card, Chetty and Weber (2007) point out, “conventional wisdom” is that job seekers’ hazard rate of leaving the state of unemployment peaks as soon as the benefit period draws to an end. They question, however, the validity of this observation for drawing conclusions on *re-employment*: The effect is much smaller than expected on the basis of empirical research on unemployment spells, as this research often ignores that many job seekers do not switch to employment but just become formally *inactive* when unemployment benefits stop coming in. This was also found earlier by Bratberg and Vaage (2000) for a Norwegian sample, and the authors find no clear evidence “that the hazard into employment increased when the end of benefits approached” (Bratberg and Vaage, 2000, p. 153). Another drawback of analysing transitions at the moment of benefit exhaustion is that the composition of the group of job seekers may not be constant until the moment of benefit exhaustion is reached, which weighs on the generalizability of results.

Observing job seekers’ transitions before and after a policy reform is a third form of capturing variation of welfare state “generosity.” Based on a shortening of the maximum possible duration of benefit receipt in Portugal in the 1990s, Portugal and Addison (2008) identify “strong disincentive effects” (Portugal and Addison, 2008, p. 393) of unemployment benefits on the exit rate from unemployment (but see above). For Slovenia as well, Van Ours and Vodopivec (2006) found that in 1998, a shortening of the entitlement period boosted transition rates into employment (only) for the job seekers concerned by the benefit reform. By contrast, the above cited study of Bratberg and Vaage (2000) does not find a behavioural change of Norwegian job seekers when a reform extended the benefit period at the beginning of the 1990s.

The merit of studying benefit reforms is the advantage of a “natural experiment”: unobserved heterogeneity between the groups of job seekers compared is probably very small. However, there can be unobserved heterogeneity at the institutional level, as benefit reforms are often accompanied by other changes in the system. For example, analysing the different impacts of social benefits in Germany before and after the Hartz reforms would require considering the deep changes in the organization of the labour administration and the new setup of active labour market policies.³ Research has shown that the institutional environment of social benefits matters by pointing to interactions with other aspects of regulation. For example, Fredriksson and Holmlund (2006) found “that a system with monitoring and sanctions restores search incentives most effectively,” concluding that “generous” unemployment insurance is less of a problem where these precautions are in place.

The above overview focused on studies using micro-level data, because so does ours. Two points to take home from our small literature review (there are many more works) is that (1) the majority of studies point to a “disincentive” effect of generous benefits and that (2) various methodologies are being used, but all have their specific weaknesses.

Our study will propose yet a different method, different in more than one way. First, even though we analyse micro-data, we include several countries. This adds to the generalizability of results across countries. Second, rather than the transitions of individuals, we will observe those of households (we justify this below). Third, we focus on the subject of work intensity and thus include the status of economic inactivity, which escapes studies that focus on unemployment. Fourth, while studies using the NRR method measure the “generosity” of social benefits as the short-term monetary difference between working and not working, we assume that most persons strive for employment anyway, and for other reasons than monetary incentives: to receive (self-)recognition, to structure their day, to socialize, etc. (cp. van der Wel & Halvorsen, 2015, pp. 100–101). In line with the welfare resources perspective, we consider social benefits as a resource that permits job seekers to carefully choose their next job. Rather than the difference between

³In Nagl and Weber (2014), the Hartz reforms figure in the model as a dummy, so all of the reform’s features are in principle included. Yet, the particular effect of social benefits can thus not be isolated.

social benefits and potential earnings from employment – the NRR – the crucial issue is thus whether social benefits are *high enough* to permit job seekers not to accept any job offered to them.

The expectation with regard to our empirical results is that high levels of public welfare provision should have a *negative* effect on the speed of employment take-up. This is also what the three – otherwise opposing – paradigms mentioned in the introduction suggest. What would speak against this prediction is that job seekers may have an incentive to not overly prolong their search phase in order to maintain their “employability”: Scarring effects of longer unemployment spells have been amply shown (Nilsen & Reiso, 2011), and job seekers are presumably aware of this issue.

Data and method of analysis

Our analysis relates to the link between the monetary “generosity” of social benefits received by households with low work intensity at a specific point in time and the change of these households’ situations in terms of work intensity during the year following benefit receipt. It is a multilevel regression analysis, where the first level are households and the second level are countries. We use pooled longitudinal data from the European Union Statistics on Income and Living Conditions (EU-SILC), reporting on the (income reference) years between 2006 and 2014. In the following, we will review in detail the phenomenon to be explained by our analysis (change in household work intensity), our main explanatory factor (social benefit “generosity”), as well as the sample and the regression model.

Change in household work intensity (dependent variable)

The concept of household work intensity is at the basis of one of three components of the AROPE indicator (“At risk of poverty or social exclusion”), used by the European Pillar of Social Rights’ social scoreboard.⁴ The definition of “quasi-jobless households” – households with low work intensity – refers to households “where working-aged adults (18–59) worked less than 20 per cent of their total work potential during the past year.”⁵ The indicator is officially calculated with data of the EU-SILC. In the present analysis, work intensity is relevant both for sampling and for constructing our dependent variable (DV): Only households with low work intensity in the first year of observation are included in our sample (see below).

To calculate household work intensity, we do not use the item (HX020) provided in the EU-SILC as it does not offer the 0–20 per cent category needed to identify (quasi-)jobless households. As pointed out by Ward and Ozdemir (2013), the item does not take due account of part-time working hours. We use a method and syntax that was kindly made available to us by the latter authors, except that we apply some small modifications necessary to adapt it to the longitudinal files of the EU-SILC.

As a starting point for understanding the method, it is useful to consider that the total work potential of each household is the amount of labour that would be realized if every adult household member (ie. 18 years or more) worked full-time during all months of the year, except those spent studying (for persons younger than 25) or in retirement. The work intensity of a *household* in a given year is the time worked by all household members, expressed as a share of its total work potential in that year. The challenge is to determine the time that household members worked. Due to some restrictions inherent to the EU-SILC, this is not as straightforward as it may sound, as soon as one seeks to take part-time work into account.

The root of the difficulty is that the information given in the EU-SILC on the actual number of hours worked (PL060 for the main job, PL100 for further jobs) applies to the time of the interview, while the information on how many months a person spent working (PL210 resp. PL211 depending on EU-SILC wave) refers back to the income reference period. This is unproblematic for all months spent in full-time work, here we ignore the actual number of hours worked for simplicity. For months spent working in part-time (cf. PL030/PL031), however, it is an important difference whether it was rather marginal or

⁴<https://ec.europa.eu/eurostat/web/european-pillar-of-social-rights/indicators/social-scoreboard-indicators>, last accessed 3 May 2019.

⁵<http://ec.europa.eu/social/BlobServlet?docId=10421&langId=en>, last accessed 21 February 2019.

rather substantial part-time work. In order to know this, we need to find a way of using the information given in the data to determine how many part-time hours were worked in the income reference period.

Following Ward and Ozdemir, we use the number of part-time working hours at the time of the interview as a proxy of the number of part-time hours worked during the income reference period under the condition that there has been no change of job since last year (PL160) and that information on working hours at the time of the interview is provided (PL060). Otherwise, again following Ward and Ozdemir, we calculate the weighted mean working hours (of persons employed in part-time)⁶ for subgroups of the sample population at the time of the interview, discerning by country, year, sex and age group.⁷ The result is then assumed as the personal working time during the income reference period, according to which group the sample person belongs to.⁸

The reason for taking the observed value at the time of the interview instead of the imputed value, if the mentioned conditions are fulfilled, is the assumption that the former is closer to the true value in the reference period.

The aspect in which our analysis had to deviate from Ward's and Ozdemir's method is that only the primary job of a worker could be considered, as the working time item for further jobs (PL100) is only provided in the cross-sectional EU-SILC files, not in the longitudinal ones used by us. This can lead to underestimating the working time of persons with several part-time jobs. In addition, our measure of household work intensity excludes from the denominator not just student months for persons 18–24 years old, but also months in retirement. This is in order to avoid that household work intensity drops sharply when a household member retires. Excluding student months, as Ward and Ozdemir do as well, avoids counting households of young students among the group of (quasi-)jobless households.

In a final step, the household's work intensity is obtained by dividing the total working months⁹ of all household members by the total work potential of the household. The DV of our regression analysis will be the *change* of work intensity during one year. It results from subtracting the work intensity of year t from the work intensity of year $t + 1$. The difference is expressed in percentage points (p.p.).

Social benefit “generosity” (independent variable)

As our independent variable (IV), we construct a sum measure of social benefits received by households. It includes benefits aiming to fight poverty (family- and children-related allowances, benefits addressing social exclusion and housing allowances) and decommodifying benefits paid to individual household members (unemployment benefits, sickness and disability benefits). Summing up across these categories is useful in comparative research because different countries may treat them as functional equivalents (cp. Erlinghagen & Zink, 2008). We use the *gross* amounts because, unlike net amounts, they are regularly provided in the EU-SILC.

The sum of benefits (in €) is then equalized by household size¹⁰ and deflated by the purchasing power parity (PPP) of the respective country and year. As recommended by Mack and Lange (2015), we use the PPP given for “actual individual consumption,” referring to the EU28.¹¹ This leads us to expressing social benefit amounts in terms of purchasing power standard (PPS), a unit which is comparable across countries with different price levels. As a sensitivity test for our analyses, we

⁶Part-time hours are capped at a maximum of 35: We can find up to almost 100 work hours per week (according to PL060) for persons who are part-time workers according to PL031.

⁷This mean value is calculated based on the data in the EU-SILC cross-sectional files because no common weighting variable can be obtained for the pooled releases of the longitudinal files.

⁸This imputation procedure also helps to deal with the frequent missings in the working time variables.

⁹Months worked in part-time are counted as fractions of months, the fraction depending on the number of hours worked with a maximum of 35 hours. A part-time month with an average of 35 working hours per week would thus be counted as a month worked in full-time.

¹⁰The equalized household size captured by EU-SILC item HX050 is defined as 1.0 for a single household plus 0.5 for each additional household member aged 14 and over, plus 0.3 for each additional household member aged 13 or less.

¹¹Values are taken from the Eurostat database: item `prc_ppp_ind`, specification `PPP_EU28`.

alternatively deflate equivalized social benefits (in €) received by households by the level of the poverty threshold in the respective country and year.

Observing social benefits at the institutional level, as it is frequently done, means treating benefits as a feature of the country, not of the individual household. Yet, households have unequal access to social benefits due to their different employment histories and legally acknowledged situations. Also, a “legislative ‘paper reality’ does not necessarily translate into actual ‘benefit reality’ due to issues such as deviating policy implementation, administrative discretion, and non-take-up of benefits.” (Otto, 2017) Using micro-level measures of the extent of welfare provision thus has important advantages. A further drawback of the macro-level approach is statistical: the number of independent cases remains limited, most of the time, to the number of countries in the sample.¹² This limits both the size and reliability of statistical models. The reason why policy evaluations with a comparative (cross-country) design often use institutional data is that the relevant information is frequently not available at the micro-level. By contrast, macro-level databases have been thriving during recent years, provided, eg. by Eurostat, the OECD, the World Bank and many smaller institutions.

Also in the present case, where EU-SILC data are exploited to construct an IV at the micro level, not all the relevant information is contained in our data set. Concretely, all information on social benefits is given as sums for the whole income reference period. We do not know for how many months social benefits were received. This is critical, as we understand social benefit “generosity” as a relationship between the amounts of resources provided to households and their respective needs. One aspect of a household’s neediness is the number of months for which the situation of financial need lasts. To determine the “generosity” of sum of benefits granted to a household during one year, we have to distinguish between a case where unemployment concerned the household during a whole year and a case where there was a transitory unemployment phase of only three months, for example. Beside the issue of *duration*, there is of course also the *extent* of neediness at any given moment. For example, some households may be without any income, while others still have one member in employment.

It has to be added that social benefits are not always granted on the basis of need. Especially with regard to child and family benefits, many countries provide these benefits independently from need to lower the costs related to having a child/children (eg. maternity leave, universal child benefits, etc.). Transfers can also be related to previous earnings (and contributions). In an earnings-related system, household members with high previous levels of earnings may get higher transfers in the case of unemployment or sickness.¹³ Therefore, if we want to measure the impact of social benefit “generosity,” we need to look at it in the light of households’ unequal need for *and* entitlement to social benefits.

We choose two different routes to solve this, a more conventional and a more experimental one: First, we will analyse the effect of the variable capturing social benefits, built in the way described above, while including control variables in the regression model which reflect the household’s socioeconomic situation. Secondly, as an alternative approach, we will transform the independent variable: social benefits received will be deflated by information on the households’ entitlement and need.

Deflating yearly social benefits by household entitlement and need

For taking the second route just mentioned, we first need to quantify households’ entitlement to or need for benefits. In principle, this should be feasible given the ample information on households’ situations provided by the EU-SILC. The problem is that much of it refers to the year of the survey, more precisely the *moment* of the *interview*. By contrast, as mentioned, the relevant data on employment and income refer to the *whole* income reference period, which is usually one year *prior* to the survey year.¹⁴

¹²Even if a longitudinal observation covers several years, these years are still nested in countries and are thus not statistically independent.

¹³Thanks to two anonymous reviewers for pointing this out very clearly.

¹⁴Exceptions are the United Kingdom, where the income reference period is the very year of the survey, and Ireland, where it is the 12 months before the interview.

The available information thus neither belongs to the same period as our main variables of interest, nor does it cover the diversity of situations that may have been experienced by the household during this period. We thus have to make do with the information offered on the income reference period if we want to contextualize the amount of social benefits received by a household. In that situation, we cannot properly distinguish between need and entitlement, we can just assume that it is a mixture of both that triggers transfers granted to households.

We approximate the entitlement and neediness of each household by an index, which we call the entitlement-and-neediness-index (ENI). It combines two indicators: (1) the household work intensity described above and (2) the household's depth of poverty before benefits (expressed as percentage of the poverty threshold, capped at 100 per cent).¹⁵ Both components count into the index with equal weight. The ENI reaches its maximum if the work intensity of a household is zero and the income is also zero, i.e. the pre-transfer poverty gap is at 100 per cent of the poverty threshold. The index reaches its minimum if all members of the household work full time during the income reference period (which does not occur in our sample by definition) and household income equals the amount of the poverty threshold or lies above it.

The sum of benefits received by each household in the first year observed is then deflated by the household's score on our index in that year. We do not simply divide the sum of benefits by the value of the ENI. The formula is determined empirically, with the following guiding idea: households with a lower work intensity and a greater poverty gap before benefits receive higher amounts of social benefits (the ENI correlates with yearly social benefits by approximately $\rho = 0.4$). Considering that for each given situation of households' benefit entitlement/neediness, there are cases with "more-generous" and cases with "less-generous" social benefits in our sample, we modify the measure of benefits received so that no correlation is left with the ENI.¹⁶ For each point on the entitlement-neediness scale, each value of the ENI, we thus assume a similar empirical distribution of "generosity" of benefits. Therefore, if we modify the benefit variable in a way that it is not correlated any more with the ENI, we have transformed it into a variable that approximates the adjusted benefit "generosity."

The approach just described is experimental, without any doubt. To our knowledge, there is no precursor or other example in the literature. It is clear that the ENI or any other way of quantifying household entitlement and neediness are just approximations. Constructing a measure of neediness and entitlement with the available information on the income reference period opens space for uncertainty; also, there can remain unobserved heterogeneity of neediness and entitlement between households. We will get back to this topic when interpreting our results. As mentioned, the ENI-adjusted social benefits are just supplementary to the non-adjusted amounts that we also use (separately) as IV in our analysis.

Sample

The sample for the analysis is drawn from EU-SILC. It consists of households observed over any two consecutive years and which feature low work intensity – between 0 per cent and 20 per cent – in the first year of observation (see above). The reason for this sampling choice is the political priority laid on bringing those households (back) into employment.¹⁷ Employment being today the main avenue to social participation, the impact of social benefits on reaching this aim should be of particular interest for the case of this group of households.

¹⁵To calculate the depth of poverty, we first measure the distance between each household's EDHI before social benefits and the poverty threshold of the respective country and year (set at 60% of (weighted) median EDHI, which we calculate using the cross-sectional files of the EU-SILC). We then divide this distance by the value of the poverty threshold to make it comparable across countries. Households with an EDHI above the poverty threshold have a depth of poverty of zero.

¹⁶After trying out different formulas, we use: adjusted social benefits (PPS) = unadjusted social benefits (PPS)/(1 + ENI × 0.0625).

¹⁷A priority reflected in (quasi-)joblessness being one of the three components of the EU's official indicator for measuring poverty risks and social exclusion (AROPE).

The analysis refers to the historical years between 2006 and 2014. In terms of EU-SILC releases, we use those between 2008 and 2015.¹⁸ The EU-SILC have a rotating panel structure, and most of the participating countries keep households in the sample for a maximum of four years. In each longitudinal wave, thus, there are data on households that reach back several years into the past before the most recent year observed. We keep from each release only those households that were observed during the two most recent years, as one single transition is sufficient for our purpose and as this grants the largest possible sample size. Pooling across different releases requires filtering out duplicates: Duplicates can be difficult to spot, as the same household ID can refer to different households in different EU-SILC releases, and the same household can be attributed to different IDs. To filter, we use some information (contained in the item DB110) whether the household is new to the EU-SILC in the former wave observed.

Only households with at least one working-age member (18–64 years old) during both years are observed. In addition, the number of working-age members in each sample household is required not to change from one year to the next. What is more, only households with a constant composition are considered; all members are part of the household in both years observed.

Drawing on EU-SILC files on the historical years between 2006 and 2014, these criteria lead to the number of 44,604 observations (ie. households). Yet, only 34,081 will be included in our regression analysis, as not all have non-missing data for all the relevant variables. Table S3 informs about the distributions of these households across countries and years. Their average share of working-age members younger than 30 is 9.3 per cent, the share of members above the age of 55 is 55.2 per cent among sample households. In 49.1 per cent of households, all working-age persons are aged above 55. Persons with low education are also over-represented in the sample: The average share of household members with low education is of 40.4 per cent, the share with high education of 15.8 per cent.¹⁹ Yet, there are more households without working-age members of low education (52.6 per cent) than households with exclusively members of low education (33.4 per cent). The average share of working-age members limited in their activities due to health problems is as high as 28.5 per cent in our sample of (quasi-)jobless households. 44.6 per cent of households are partner households, the majority are thus single-person households. There are dependent children (below 18 years) in 15.9 per cent of households. There is at least one child below 3 years in 4.2 per cent of households, between 3 and 7 years in 5.1 per cent and between 8 and 17 years in 11.8 per cent of our sample households.

There are several reasons to choose households as the unit of observation instead of individual persons. Firstly, social benefits are our IV, but some benefits are received by the household, not by its individual members (see above). Disaggregating the total benefits received by a household and allocating them precisely to each individual member is hardly feasible without a lot of context knowledge on each household. Secondly, while it is the individual who is employed or not, the decision on employment participation often is a collective decision: When someone who does not live alone engages in paid employment, this tends to require exchange relationships internal to the household (Brose, Diwald & Goedicke, 2004, p. 287). This also means that successfully activating one member of a household can reduce the availability of other members (usually the partner) for the labour market. Considering the household – and not the individual – as the economic unit which offers labour suggests analysing the impact of social benefits on the work intensity of the household as a whole, thus looking at the *net* change of work intensity over time.

Data structure and modelling

The data set created for analysis consists of one observation per household. Observations belong to different countries and historical years (see above). As explained, each sample household appears in the

¹⁸The earliest EU-SILC-year of which we use data is thus the year 2007, contained in the data release of 2008. Considering that for most countries (except the UK and Ireland), the income reference period is the year prior to the survey, the earlier of the two years (2007), taken from our earliest release (2008), thus draws most of its data from the historical year 2006.

¹⁹Low education is defined here as no more than (pre-)primary education, high education as tertiary or other post-secondary education.

EU-SILC during two consecutive years, but most of the information we need stems from the first year observed (t).²⁰ The calculation of the DV, which is a change score between t and $t + 1$, also requires information from year $t + 1$. All variables vary across households, years and countries, except the variables “year” and “country” themselves, plus macro-level control variables which only vary across years and countries.

We use a multilevel regression model with two levels to account for the fact that households are nested in countries. The country is thus the second-level unit. Descriptive analysis shows that there are important differences between the sample countries concerning the mean change in work intensity of (quasi-)jobless households during one year. The mean increase ranges between 1.9 p.p. in Malta and 17.0 p.p. in Norway (Figure S2 in the supplementary material). Most countries with low mean changes in work intensity scores are Eastern European, while the highest changes in scores are found in Scandinavian countries. Despite the apparent importance of countries, the intra-class correlation of our DV is only of 3.7 per cent, the largest part of the variation is thus explained at the household level (keeping in mind that welfare state activity is measured at the micro level in our model).

In principle, time could be modelled as an additional level. The reason why time is not used as an additional upper level is that changes in mean social benefits over time are empirically not relevant enough to justify the additional level, ie. there is high correlation from one year to the next within countries.²¹

As explained, we use two different versions of the IV. The first version consists in the amount of social benefits received by the household during one year, equivalized by household size and transformed into PPS. The second version is additionally deflated by a metric that is supposed to capture the household’s socioeconomic situation (ENI). IVs enter the model one at a time so that their effects can be compared.

At the micro level, we include a wide range of controls documenting the household situation (couple status, disposable income²² and a variable capturing work intensity as described above) and composition (household members, their age, education and health) (see Table S4). This helps to neutralize structural differences of households both between and within countries, which might influence the probability of households taking on employment.

As for macro-level controls, the number of possible items in the model is extremely limited due to the small number of second level units (29) and the corresponding lack of degrees of freedom. We include the change of the unemployment rate from t to $t + 1$. It is meant to proxy the general change of absorptive capacity of countries’ labour markets. We presume that if unemployment decreases in general, it should also be easier for (quasi-)jobless households to return to employment. We also include the Gini coefficient of equivalized disposable household income (EDHI) at time t , as the degree of inequality in a country might influence the pace of returning to employment to generate income, even though in the present paper we stress rather the non-financial aspects of employment.²³

Historical time enters as a dummy variable. The model includes country random intercepts, but no other random terms.²⁴ Computation is done using Stata 14.2 with its estimation command “mixed”. In the results section, we will go beyond presenting statistically significant coefficients by calculating partial effects for a hypothetical change of our IV. We choose a 100 per cent change of social benefits; even though this is politically not realistic in terms of a benefit reform, it will be an illustrative way of emphasizing our main message.

²⁰Note that t refers to the income reference year, not the EU-SILC wave. Apart from the DV (work intensity change), the partner household variable is the only variable which uses information from the moment of the interview. It is thus just an approximation of the household’s partner status in the income reference period.

²¹In turn, not including time as an additional *lower* level follows from the fact that we use only two points in time, more precisely, our DV is a change score between them.

²²Using EU-SILC’s HY022, equivalized to household size and deflated by the national poverty threshold (which is functionally equivalent to deflating by median income) to allow for comparison across countries.

²³The latter variable is drawn from Eurostat (tessi190) and derives from EU-SILC data. The former is equally from Eurostat (une_rt_a), we use the percentage of the population which is unemployed and calculate the percentage change between years.

²⁴Table S4 in the supplementary material provides an overview on the variables used in the regression model.

All results in this analysis are calculated without weights for a precise reason: The pooling of different releases of the EU-SILC is technically feasible, but not supported (yet) by the data provider. Whereas weights are provided for each individual release, there is no single weight item that can be applied to all releases simultaneously. This is of course a drawback, but it should not impact too much on our results due to the way the sample is constructed.²⁵

Results

Based on the data and methods described above, this section sheds light on the link between the “generosity” of social benefits received by (quasi-)jobless households and the subsequent change of these households’ work intensities.

The majority of households in our sample do not experience a change of employment participation from one year to the next. Among the 44,604 households in the full sample,²⁶ there are 36,783 or 82.5 per cent that remain with the same work intensity. The most frequent case in our sample is the household that features a work intensity of zero in year t and maintains this zero work intensity in year $t + 1$. By contrast, 6,540 households, thus 14.7 per cent of the sample, increase their work intensity from the first to the second year observed, while 1,281 households or 2.87 per cent decrease it.

From a bivariate perspective, it seems that social benefits connect in a slightly negative way to increases in work intensity: there is a correlation of $\rho = -1.87$ per cent for unadjusted amounts, respectively -1.23 per cent for ENI-adjusted amounts of social benefits. Multivariate regression with the model presented above confirms that higher social benefits are connected to a slower return to employment, see the coefficients in Table 1.

Statistical significance does not imply, however, that effects are of a relevant size. Figure 1 shows the effect of a hypothetical 100 per cent increase in welfare generosity on the shift in work intensity (in p.p.) between one year and the next; it is compared to a business-as-usual scenario. The doubling of benefits is chosen just for illustrative reasons. Taking the amounts of social benefits as observed in the EU-SILC for the first year of observation (and further deflated), households tend to increase their work intensity by about 6.3 p.p. on average in the second year of observation, compared to the first. This increase would be less than 1 p.p. smaller if households received double the amount of social benefits in the first year. We get very similar results if we express our IV, social benefits, not as PPS but in percentage of the poverty threshold. The regression output can be found in the supplementary material (Table S5). If adjusted to the household situation (thus deflated by the ENI), 100 per cent more social benefits would reduce the improvement of work intensity (at a given overall level of social spending) by no more than half a percentage point. In other words, the effect is extremely small, considering that a doubling of social benefits is not politically realistic; the potential “damage” done by a benefit reform in the realm of the possible would be even smaller.

A possible explanation why the estimated effect is smaller for adjusted than for unadjusted social benefits is that part of the “effect” predicted for the latter is in reality due to households in more difficult situations receiving greater amounts of social benefits, for reasons that may *also* prevent them from significantly increasing their work intensity within one year (see above). The model contains control variables which may capture some – but maybe not all – of these reasons: low education or health problems of working-aged household members, and the presence of children in the household. These variables turn out highly significant in the expected sense. We should consider the different estimations as a range of possible values.

The relationship between social benefits and work intensity changes can be analysed for sub-groups of our sample (see Table 2). Classifying by household poverty status reveals that the relationship shown

²⁵Low work intensity can be assumed to correlate with a lower selection probability of the household in the EU-SILC. As explained, our sample consists *only* of households with low work intensity. Furthermore, our approach is not descriptive but multivariate regression; the purpose of weighting in this case remains a contested topic.

²⁶For the 34,081 households without missing data on all variables, see above, we get similar shares.

Table 1. Regression output with work intensity changes as the dependent variable, for non-adjusted and for adjusted amounts of social benefits.

| Category | Variable | Model 1: non-adjusted amounts | Model 2: ENI-adjusted amounts |
|-----------------------|---|-------------------------------|-------------------------------|
| Social benefits | Non-adjusted, measured as PPS | -0.000*** | |
| | ENI-adjusted, measured as PPS | | -0.000*** |
| Household situation | EDHI as percentage of poverty threshold | 0.004*** | 0.005*** |
| | Work intensity, part-time adjusted | 0.167*** | 0.175*** |
| Household composition | Number of working-aged members below 30 | 0.001*** | 0.001*** |
| | Number of working-aged members above 55 | -0.001*** | -0.001*** |
| | Number of members with low education | -0.013*** | -0.013*** |
| | Number of members with high education | 0.030*** | 0.030*** |
| | Number of members with limited activity due to health | -0.052*** | -0.054*** |
| | Partner household | -0.009*** | -0.009*** |
| | Number of children aged 0–2 | 0.009* | 0.009 |
| | Number of children aged 3–6 | 0.009* | 0.009* |
| | Number of children aged 7–17 | 0.001 | 0.001 |
| Historical time | Historical year (base 2006) | | |
| | 2007 | -0.018*** | -0.018*** |
| | 2008 | -0.026*** | -0.026*** |
| | 2009 | -0.019*** | -0.019*** |
| | 2010 | -0.020*** | -0.021*** |
| | 2011 | -0.003 | -0.003 |
| | 2012 | -0.030*** | -0.031*** |
| | 2013 | -0.031*** | -0.031*** |
| Macro-level controls | Change of unemployment rate as percentage of population | -0.006*** | -0.006*** |
| | Gini coefficient | -0.004*** | -0.004*** |
| | Intercept | 0.272*** | 0.263*** |
| Level 2 estimates | Random intercept variance | -3.322*** | -3.365*** |
| | Within-group error variance | -1.681*** | -1.680*** |
| | N | 34,081 | 34,081 |

Note: Source EU-SILC (Eurostat), own calculation.
 Abbreviations: EDHI, equivalized disposable household income; ENI, entitlement-and-neediness-index.
 * $p < 0.05$; *** $p < 0.001$.

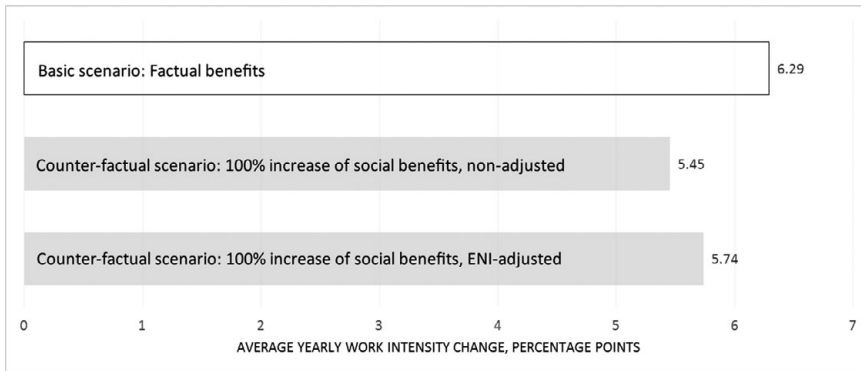


Figure 1. Estimated effect of a 100 per cent increase in social benefits on change in work intensity (p.p.), for non-adjusted and for adjusted amounts of social benefits.

Table 2. Net reduction of work intensity for a hypothetical 100 per cent increase of social benefits, by initial poverty gap, in percentage points.

| Social benefits | Whole sample | Subsamples by size of poverty gap | | |
|--|--------------|-----------------------------------|--------------------|-----------------|
| | | >50% of threshold | 50–1% of threshold | No poverty gap |
| Non-adjusted benefits, measured as PPS | −0.8*** | −2.4*** | Not significant | Not significant |
| ENI-adjusted benefits, measured as PPS | −0.6*** | −2.3*** | Not significant | Not significant |
| <i>N</i> | 34,081 | 14,390 | 6,108 | 13,581 |

Source: EU-SILC (Eurostat), own calculation. Average partial effects. ****p* < 0.001.

above is driven by the sub-sample of households at high risk of poverty. This finding confirms Chetty, (2008, p. 173) who finds that “increases in benefits have much larger effects on [unemployment] durations for liquidity-constrained households.” For households with an income above the poverty threshold already before social benefits (rightmost column), the amount of social benefits received is not significantly connected to subsequent changes of work intensity. The same holds for households with a pre-benefit poverty gap not bigger than 50 per cent of the poverty threshold, thus with an income (before social benefits) of at least 30 per cent the median EDHI. For households with a poverty gap of 50 per cent or more, however, we get highly significant coefficients. The sensitivity analysis (using the IV of social benefits measured as percentage of poverty threshold) yields some negative effect also for households above the poverty threshold, but it is much weaker than the effect for households at high risk of poverty (see Table S6). We will discuss these findings in the last section of the paper.

An alternative decomposition of the sample by age (see Table S7) shows that in households where all working-age members are above the age of 55, the “generosity” of benefits has no significant effect on our DV, while it does for younger households. By contrast, whether or not there are working-age members with limited activities due to health problems in the household does not make a difference for the impact of social benefits’ “generosity.” Distinguishing by level of education shows that effects are (negatively) significant both for low- and high-educated households; effect sizes are a little stronger for the latter group.

Beside avoiding distortions by structural effects (caused by different household structures across countries), the micro-level control variables in the model also convey insights into the determinants of households’ propensity of changing their degree of work intensity (please refer back to Table 1). It turns

out that work intensity increases more from one year to the next in households with a *higher* initial work intensity and with higher initial household income. (The *link* between social benefits and increases in work intensity is not affected by initial work intensity, as an analysis of the interaction shows.) Increases are also positively (negatively) affected by a higher (lower) education and a lower (higher) average age of working-age household members. Limitations of household members' activities because of health problems have a negative effect on the further evolution of the household's work intensity. Being a partner household negatively influences the increase of work intensity; it seems that (quasi-)jobless households of persons without partners make a bigger effort to gain ground again in the working world. Surprisingly, the overall number of children in the household is not significantly connected to the evolution of work intensity of quasi-jobless households. Only the number of children between three and six years of age has a weakly significant effect, yet a positive one.²⁷

Looking at the macro-level control variables in our model, increases in household work intensity are higher if unemployment decreases in the labour market, and they are also higher in a context of lower initial income inequality between households. The more inequality there is in a country, the more difficult it seems to get back into employment for (quasi-)jobless households. Potentially, equality thus helps (quasi-)jobless households to be re-integrated; alternatively, the statistical link may be caused by an inverse connection between labour-market tension and income inequality. In turn, the connection between decreasing overall unemployment and transitions of (quasi-)jobless households (back) into employment is on the one hand partly tautological, and on the other hand essentially plausible.²⁸

Summary and discussion

Our analysis sought to explain shifts in (quasi-)jobless households' work intensity by the "generosity" of social benefits. It used information on households in different European countries between 2006 and 2014, drawing on harmonized micro-data from the EU-SILC. We found with a multivariate model that the "generosity" of social benefits is negatively connected to the speed at which households increase their work intensity: (quasi-)jobless households that receive more or higher benefits are slower to return to employment. The surprising aspect about these findings – given corresponding predictions based on theory and the state of empirical knowledge from other studies – was that the estimated negative effect is extremely small. According to our model, even a hypothetical doubling of social benefits would result in a less-than-one-percentage-point lower increase in work intensity between two consecutive years. It seems that most households take on employment or stay unemployed/inactive rather independently of benefit "generosity." This may be due to the fact that getting a job always requires not just the desire to work, but also a job offer, as well as favourable circumstances like adequate childcare arrangements, good health, adequate skills, etc.

We further found that the negative relationship between social benefits and employment is driven by households at high risk of poverty. For households with lower poverty risks, we hardly get any indication that "generous" social benefits slow down transitions into employment. This suggests that only households that find their income situation significantly improved by social benefits use the additional financial leeway to stay at distance from employment. In our view, this underscores the welfare resources hypothesis: the relevant question seems to be not the monetary value of social benefits as such, but whether transfers make a real difference for the household's "capability set" (Lehwiss-Litzmann, 2014).

The remainder of this paper will address two important issues to consider when trying to draw policy conclusions from our empirical results. The first one concerns the validity of our findings in the face of

²⁷The number of elderly persons above 80 years is not significantly connected to changes of household work intensity. The latter variable has been eliminated from our model. Without any doubt, effects of household composition on *levels* of work intensity would be more significant than on the *changes* of work intensity which are in the present focus.

²⁸Other possible macro controls that have been tested are not significantly connected to our DV, eg. the unemployment rate or GDP and GDP growth. The choice of macro-level control variables in the model does not affect the negative significance of the IVs' coefficients.

methodological pitfalls, namely the threat of unobserved heterogeneity between households. As pointed out by Røed and Zhang (2003, p. 190): “The overriding problem in the whole literature is a lack of independent variation in benefit payments or replacement ratios. Variation in benefit entitlements is typically correlated with previous income, which again is likely to be correlated with unobserved characteristics that affect unemployment duration in their own right.” It is thus possible that households receiving “more-generous” benefits do not randomly receive these benefits, but rather based on a different employment history as compared to households with “less-generous” benefits. In particular, they may have experienced less recent unemployment and higher earnings, which would both increase their transfer incomes (as social insurance systems in most countries are earnings-related) and function as a positive signal to potential employers. A higher entitlement, erroneously interpreted as higher “generosity” of benefits, could thus go together with a swifter transition into employment. Note, however, that our results point in the opposite direction, we find a *smaller* increase of work intensity for households with higher social benefits.

A related kind of unobserved heterogeneity could have to do with the total duration of joblessness: If the possibility to take up work increased with the duration of unemployment, eg. because job seekers become more open to compromise on job quality, and social benefits simultaneously decreased over time, then there could be a confounding effect. However, the first assumption does not necessarily hold: job seekers’ chance of finding work may also decrease with the duration of unemployment (Alaouze, 1984). This being said, we cannot exclude the possibility of unobserved heterogeneity playing a role in our analysis.

The second issue on which policy conclusions should crucially depend is how to interpret spells of non-employment. As was discussed at the beginning of the paper, a link between the “generosity” of social benefits and the duration of spells of unemployment or inactivity need not be a bad thing: It can be read as a confirmation of social policy as a public resource that grants troubled households additional time to “sort things out.” Waiting longer for a job – or retraining in the meantime – can be beneficial because of a potential trade-off between a swift transition into a job and the quality, qualification-adequacy and productivity of employment: There is evidence that welfare state “generosity” improves post-unemployment earnings and job stability, eg. Gangl (2004, 2006), Tatsiramos (2009) and Wulfgramm and Fervers (2013), even though the effect may be small (Belzil, 2001) and findings are sometimes mixed (Addison & Blackburn, 2000). Policy implications should thus depend on the reasons and effects of prolonged job search. In that sense, seeking explanations by quantitative and qualitative empirical research – guided by the hypotheses of the neo-liberal, the conservative, and the welfare resources views introduced above – would be the next step after measurement.

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