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IZA DP No. 7977

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Discussion Paper No. 7977
February 2014

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IZA Discussion Paper No. 7977
February 2014

# ABSTRACT

## The Dynamics of Social Assistance Benefit Receipt in Germany: State Dependence Before and After the 'Hartz Reforms'*

In this article, I study state dependence in social assistance receipt in Germany using annual survey data from the German Socio-Economic Panel (SOEP) for the years 1995-2011. There is considerable observed state dependence, with an average persistence rate in benefits of 68% comparing to an average entry rate of just above 3%. To identify a possible structural component, I estimate a series of dynamic random-effects probit models that control for observed and unobserved heterogeneity and endogeneity of initial conditions. I find evidence of substantial structural state dependence in benefit receipt. Estimates suggest that benefit receipt one year ago is associated with an increase in the likelihood of benefit receipt today by a factor of 3.4. This corresponds to an average partial effect of 13 percentage points. Average predicted entry and persistence rates and the absolute level of structural state dependence are higher in Eastern Germany than in Western Germany. I find only little evidence for time variation in state dependence including for the years around the Hartz reforms.

JEL Classification: I38, J60, J64, C23

Keywords: social assistance, welfare benefits, state dependence, Germany, Hartz reforms

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* This article is part of a series of studies on social assistance dynamics initiated and supported by the OECD. A revised version of this paper has been submitted to a special volume of *Research in Labor Economics* with the title "Safety Nets and Benefit Dependence: Evidence and Policy Implications". An earlier and more comprehensive version of this paper was released as OECD Social, Employment and Migration Working Paper #136. I would like to thank Tony Atkinson, Steve Bond, Stéphane Carcillo, Patricia Gallego-Granados, Herwig Immervoll, Stephen Jenkins, Monika Queisser, Regina Riphahn, Kostas Tatsiramos, Christoph Wunder and two anonymous referees for helpful comments. The paper has moreover benefited from the feedback I received at the joint OECD/IZA/World Bank workshop on Social Safety Nets and Benefit Dependence in Paris in May 2013. Financial support provided through the INET grant INO1200010 by the Institute for New Economic Thinking at the Oxford Martin School is gratefully acknowledged. The usual disclaimer applies. In particular, the views expressed in this paper do not represent the official positions of the OECD or the governments of OECD member countries.

# 1 Introduction

A standard observation in data on social assistance benefit receipt is that current recipients are much more likely than non-recipients to receive benefits also in the next period. For instance, as described below, the average year-to-year persistence rate on benefits for recipients in Germany was 68% over the years 1995-2011 compared to a year-to-year entry rate for non-recipients of only 3%.

Two explanations for this ‘state dependence’ have been proposed (Heckman, 1981a,b). First, there is heterogeneity in personal and socio-economic characteristics. If these characteristics affect the likelihood of benefit receipt, individuals with less ‘favourable’ characteristics – for instance low educational attainment or bad health – will self-select into benefits. The resulting differences in individual characteristics between recipients and non-recipients will induce differences between benefit entry and persistence rates. Any state dependence induced by heterogeneity across individuals will disappear once all relevant characteristics are controlled for, which is why it is referred to as ‘spurious’. Second, the gap between persistence and entry rates might hint at potential pervasive effects of benefit receipt itself. Individuals on benefits might feel less confident or motivated to leave benefits *as a result of benefit receipt*, or they may become accustomed to receiving transfer payments as a ‘way of life’ (Bane & Ellwood, 1994). Potential employers might interpret benefit receipt as a negative signal about a recipient’s unobserved labour productivity when screening job applicants, which would reduce her employment prospects and thus the likelihood of becoming self-sufficient. In these cases, current benefit receipt has a *causal* effect on the probability of future receipt by raising hurdles to self-sufficiency. This effect is referred to as ‘structural’ or ‘genuine’ state dependence.

The two potential drivers of state dependence have very different implications for policy-making. If benefit receipt *as such* increases the probability of future benefit receipt, policies that prevent entry or facilitate early exits from social assistance can induce a lasting reduction in receipt rates. If, by contrast, high benefit persistence is due to recipients’ characteristics, policies that encourage exits from benefits are likely to have little impact unless the factors causing benefit receipt are addressed directly.

This article presents an empirical analysis of state dependence in social assistance receipt in Germany for the years 1995 to 2011 based on annual survey data from the German Socio-Economic Panel (SOEP). The period studied is of particular interest because it covers the far-reaching ‘Hartz reforms’ implemented from 2003 to 2005 that fundamentally changed the system of social assistance benefit provision in Germany. While the type of model estimated is not suited for assessing potential causal reform effects, the analysis presents evidence on both the level of state dependence as well as on potential variations in state dependence over the observation period. Sample selection criteria and the estimation technique used are similar to those in earlier analyses for other countries such that results can be compared across studies. A methodological contribution of the article lies in that it contrasts the results obtained to those reported in an earlier study for Germany by Wunder & Riphahn (2013), who – for a narrower sample and based on a different modelling approach – obtain very different results.

The decomposition of observed ‘raw’ state dependence into its structural and spurious components has been the focus of much recent work on social assistance dynamics. Yet, the number of studies that look for structural state dependence in social assistance receipt remains small to date and the existing work is limited to a few countries.[1] Chay, Hoynes & Hyslop (1999) and Chay & Hyslop (2000) provide evidence for state dependence in the receipt of Assistance to Families with Dependent Children (AFDC) in the United States. Gong (2004) studies benefit transitions of low-income women who receive Income Support or Family Allowance in Australia and finds state dependence in both programmes. Hansen, Lofstrom & Zhang (2006) report strong variations in state dependence across provinces in Canada and suggest that the level of state dependence might be positively related to benefit generosity. Hansen & Lofstrom (2008, 2011) and Andrén & Andrén (2013) study the native-immigrant gap in benefit receipt in Sweden. They find higher structural state dependence for migrants but emphasize the importance of unobserved heterogeneity for explaining differences in receipt rates between natives and migrants. In a study for Britain, Cappellari & Jenkins (2009) estimate stronger structural state dependence for lone parents and for recipients with one non-interrupted spell compared to individuals with a spell of work between interview dates.

In an earlier study of state dependence in social assistance benefit receipt in Germany[2] referred to above, Wunder & Riphahn (2013) compare the benefit dynamics of natives and immigrants in Western Germany for the post-Hartz years 2005-2009. Based on SOEP data, they estimate a dynamic multinomial logit model with three competing states distinguishing between social assistance receipt, employment, and 'inactivity' (which is defined as including unemployment). They find that persistence in social assistance benefit receipt can mostly be accounted for by observable characteristics, with only limited evidence for structural state dependence.

In this article, I follow the approach used in much of the earlier work on social assistance dynamics by estimating a series of dynamic random-effects probit models that permit controlling for individuals' observable characteristics and persistent unobserved heterogeneity. I find that even though individual heterogeneity explains most of the gap between observed benefit persistence and entry rates, there is evidence of substantial structural state dependence in social assistance. On average, benefit receipt at the last interview raises the likelihood of benefit receipt at the current interview by a factor of 3.4. This corresponds to an average partial effect of past benefit receipt on the probability of receipt in the current period of 13 percentage points. By contrast, I do not find evidence of a change in state dependence around the time of the Hartz reforms. While state dependence was lower for the years 1996-2004 than in 2005-2011 this effect seems to be primarily driven by lower state dependence in the late 1990s and a temporary spike in 2010 for Eastern Germany.

A sensitivity check finally illustrates that the estimated level of state dependence is highly sensitive to sample selection and, more importantly, to the method used for defining the benefit variable. Replicating the approach used by Wunder & Riphahn (2013), I show that using an

[1] A few studies look at the related question of duration dependence in social assistance receipt using event-history models, see for instance Dahl & Lorentzen (2003) and Mood (2013) for Sweden or Schels (2013) for Germany.

[2] Other studies use panel data methods to examine the determinants of social assistance receipt in Germany without looking at state dependence (Riphahn, 2004; Riphahn, Sander & Wunder, 2013; Schels, 2013). For cross-sectional analyses of the determinants of social assistance benefit receipt in Germany, see Voges & Rohwer (1992) or Riphahn & Wunder (2012). Mühleisen & Zimmermann (1994) study state dependence in unemployment.

individual- rather than the more standard household-level definition of the social assistance variable leads to a substantial drop in the estimated level of state dependence. Once this issue and differences in sample selection are accounted for, the results presented in this article are consistent with Wunder & Riphahn's findings of only very weak state dependence in Germany.

The remainder of this article is structured as follows: Section 2 gives an overview of the institutional background in Germany during the observation period and defines the benefit variable. The data used in the analysis are described in Section 3. Section 4 presents trends in benefit receipt and transition rates. Sections 5 and 6 introduce the econometric model and present empirical results on state dependence in social assistance receipt. Section 7 concludes.

## 2 Institutional background and definition of the benefit variable

During the 1995-2011 observation period, the German social assistance system underwent far-reaching reforms. The so-called 'Hartz reforms'[3], implemented by the left-of-centre coalition of Social Democrats and Greens from 2003 to 2005, resulted, among other things, in a structural change of the groups entitled to different last-resort minimum-income benefits. This Section describes some key features of the benefit system in the years before and after the reforms and defines the social assistance variable used in the analysis.

### Institutional Background

Until 2005, the German income-support system for working-age individuals had a three-tier structure. As the top layer, Unemployment Insurance benefits (UI, *Arbeitslosengeld*) aimed at replacing an individual's income after job loss for a limited amount of time, with eligibility being conditional on a previous work and contribution record.[4] Individuals whose entitlements to UI had expired could claim Unemployment Assistance benefits (UA, *Arbeitslosenhilfe*). UA was earnings-related but means-tested on family-income and less generous than UI.[5] Unlike UI, UA benefits could in principle be received for an indefinite period of time under the condition that the claimant was looking for and available for work. Finally, Social Assistance[6] (SA, *Sozialhilfe*) served as a benefit of last resort below this primary social safety net. SA was understood as a temporary emergency benefit, and eligibility required from individuals to have exhausted all alternative sources of income in the form of earnings from work, UI or UA benefit payments and financial support from direct family members. While SA had initially been primarily targeted at individuals with special needs and limited employability, a gradual tightening of eligibility

[3]The new legislation was formally labelled 'laws for modern services on the labour market' (*Gesetze für moderne Dienstleistungen am Arbeitsmarkt*) and was subdivided into four packages, which were enacted sequentially in the years 2003 ('Hartz I & II'), 2004 ('Hartz III') and 2005 ('Hartz IV').

[4]The maximum duration of benefit entitlements was 12 to 32 months depending on age and the previous contribution history, with the relevant thresholds changing over the observation period. Benefit levels were determined by a replacement rate of 60% of previous earnings net of taxes and social security contributions (67% for individuals with children) and were independent of individual means.

[5]Until the end of the year 1999, individuals could claim UA without having previously received UI under the condition that they had worked for at least 150 days over the last 12 months. From 2000, receipt of UA benefits was restricted to individuals who had exhausted their claims for UI. Replacement rates were 53% (57% for individuals with children)

[6]Throughout this article, I distinguish between the concept 'social assistance' (non-capitalized) and the benefit programme 'Social Assistance' (SA, *Sozialhilfe*, in capital letters)

criteria for UI and UA over time meant that a growing numbers of individuals were shifted into SA. Due to the lower benefit amounts of UA compared to UI, recipients of UA benefits moreover often qualified for SA payments as a top-up.

The fourth package of the Hartz reforms, which entered into force in January 2005, abolished this three-tier system with the aim of strengthening labour market services and intensifying the activation of unemployed job seekers. The contribution-based UI was replaced by the new Unemployment Benefit I (UBI, *Arbeitslosengeld I*), with an initially unchanged maximum benefit duration and replacement rate.[7] The more relevant change in the context of this study was the merger of UA and SA for employable job seekers into the new means-tested Unemployment Benefit II (UBII, *Arbeitslosengeld II*). The computation of UBII benefit levels follows a similar logic as for the former last-resort SA. Compared to the old UA scheme, the new UBII is typically less generous and it no longer depends on the level of previous earnings. SA continues to exist as a separate programme but is now restricted to individuals incapable for work due to sickness, disability, or care duties. The Hartz reforms thus introduced a clearer distinction between the minimum-income support for employable and non-employable individuals.

Both before and after the reform, an income-tested Housing Benefit (HB, *Wohngeld*) is targeted at low-income households more broadly. Until 2005, this benefit could be claimed by individuals in work and by recipients of UI or UA benefits while SA recipients were not entitled. Since 2005, recipients of UBII and SA receive support for eligible housing expenses as part of their benefit entitlements while HB continues to be available for other low-income groups.

### Definition of the benefit variable

In light of the institutional changes just described, it is not obvious what the best choice is for defining a social assistance benefit variable that allows for a consistent analysis of receipt dynamics over the entire observation period. Existing studies of social assistance dynamics in Germany focus only on relatively short time periods either before (Voges & Rohwer, 1992; Riphahn, 2004) or after the Hartz reforms (Schels, 2013; Wunder & Riphahn, 2013) and look at receipt of either SA or UBII only. In this paper, I choose a slightly different approach by defining a broader benefit variable that takes into account receipt of all means-tested benefits (for an overview, see Table 1).

The classification of pre- and post-reform SA and of UBII as social assistance programmes is probably uncontroversial. A categorization of UA by contrast is more difficult: As a contribution-based and earnings-related benefit it does not correspond to the standard definition of a social assistance programme. The reason why it is included in this analysis nonetheless is that treating UA as a social assistance programme is sensible in terms of the implied benefit dynamics. The typical recipient of UA in December 2004 would go on to receive UBII in January 2005. It is not evident why such a transition should bring about a change in the individual's social assistance benefit receipt status for the purpose of this analysis. As the direct precursor to UBII, UA moreover shared a number of key features of the other social assistance programmes. Unlike UI benefits, UA was means-tested and could be claimed for an infinite period of time. Also,

[7]The maximum period of benefit entitlements remained 12 to 32 month depending on age until a year after the reforms. In 2006, it was lowered to 18 months but raised again to 24 months in 2008. The replacement rate remained at 60% (67% for individuals with children).

it was not paid for by social-security contributions but was tax-funded. Both of these features make it resemble social assistance benefit schemes like SA or UBII.[8] Finally, I also take into account receipt of HB as a means-tested benefit targeted at low-income benefits more broadly. HB receipt rates are however relatively low and excluding HB from the analysis does not affect its main conclusions (see Königs (2013)).

Table 1: Principal eligibility conditions of social assistance benefit programmes for working-age individuals in Germany

| **before the Hartz reforms** | **after the Hartz reforms** |
|---|---|
| **Social Assistance (*Sozialhilfe*)**<br>• lacking or insufficient social insurance contribution history and income and assets below a specified minimum level<br>• possibly available for (part-time) work | **Social Assistance (*Sozialhilfe*)**<br>• lacking or expired claims to contributory Unemployment Benefit I and income and assets below a specified minimum level<br>• incapable of working |
| **Unemployment Assistance (*Arbeitslosengeld*)**<br>• history of work and social insurance contributions but expired (or lacking) entitlements to Unemployment Insurance benefits | **Unemployment Benefits II (*Arbeitslosengeld II*)**<br>• lacking or expired claims to contributory Unemployment Benefit I and income and assets below a specified minimum level<br>• available for at least part-time work |
| **Housing Benefits (*Wohngeld*)**<br>• income below a specified minimum level and not recipient of Social Assistance (but possibly of Unemployment Insurance or Assistance Benefits) | **Housing Benefits (*Wohngeld*)**<br>• income below a specified minimum level and not recipient of Social Assistance or UBII (but possibly of UBI) |

Like most comparable previous studies, I use the individual as the unit of analysis. While eligibility for social assistance benefits is determined at the level of a possibly larger ‘need unit’, frequent changes in household composition imply that it is not obvious how the benefit dynamics of a household could be studied over time. Since for the means test, the financial status not only of the claimant alone but of other household members matters, I however categorize an individual as a benefit recipient if benefit payments are recorded for any individual in the household.[9] Information on benefit payments is taken from both the household questionnaire and the questionnaires completed by each household member.[10] I include partner and house-

[8]Earlier studies of immigrant-native differences in social assistance benefit receipt by Riphahn & Wunder (2012) and Riphahn et al. (2013) also looked at Unemployment Assistance, Unemployment Benefit II, and Social Assistance jointly without however accounting for receipt of Housing Benefits.

[9]In the estimations, I need to assume independence across individuals. Strictly speaking, this assumption is violated if in each period, a household can be represented by several observations that by construction have the same social assistance receipt status. Like earlier authors, I ignore any potential inconsistencies induced by this lack of independence. In an earlier version of this paper (Königs, 2013), I however show that results differ little when the sample is split between women and men, a case in which the independence assumption is arguably more credible.

[10]Questions on the receipt of minimum-income benefits by any of the members of a household are included in the household questionnaire. For UBII, an additional question is included in the personal questionnaire that is completed by each working-age member of the household. For further information on the design of the SOEP see the following Section.

hold characteristics as explanatory variables in the econometric estimations to account for the importance of household composition. It is worth noting however that the household as defined in the survey will not always coincide with the benefit unit used by the social assistance office to assess eligibility for income-support payments.

The time interval of analysis is one year during which benefit receipt is measured only once at the moment of the interview. While respondents in the SOEP are requested to provide information on receipt of income-support payments on a monthly basis, corresponding information on personal and household characteristics is lacking that would be required to estimate the model at the monthly level. Earlier research moreover indicates that the quality of monthly data on benefit receipt derived from annual surveys is often poor. In particular, so-called 'seam bias' is observed in months where survey periods adjoin or overlap as respondents have apparent difficulties to answer questions that relate to early parts of the survey year (Pavetti, 1993; Blank & Ruggles, 1994). The approach of modelling benefit transitions from one interview date to the next therefore appears to be the safer option, and it has been previously used for the same reason by Cappellari & Jenkins (2009, 2013) in their analysis of social assistance receipt dynamics in Britain.[11]

## 3 Data used

The data for the analysis come from the German Socio-Economic Panel (SOEP)[12], a representative longitudinal survey of private households in Germany. The panel was started in West Germany in 1984 and expanded to the territory of the former German Democratic Republic in 1990. The last wave currently available is for 2011. Over time, the sample size increased from an initial 6,000 households to around 12,300 households and 22,000 individuals in 2011.

In a sampled household, all individuals aged above 16 are interviewed personally and one of the household members additionally completes a separate household questionnaire. All members of a sampled household are followed over time even if they leave the original household. Individuals who move into a sampled household become part of the panel and remain in the sample even in case of a split-up of that household. Household interviews are conducted annually, with the majority of interviews taking place early in the year.[13] The SOEP oversamples 'guest workers' and other immigrants, German residents of the former German Democratic Republic, and high-income individuals. For a detailed description of the dataset, see Haisken-DeNew & Frick (2005) and Wagner, Frick & Schupp (2007).

I use the last 17 waves of the SOEP for the years 1995-2011 prior to which no question on the receipt of income-support benefits at the time of the interview was asked. I restrict the sample to working-age individuals (25-59 years) who are not dependent children and without missing information on benefit receipt and a few other important variables. I further drop observations

[11] An alternative approach frequently used is to define a 'benefit year' by setting the binary social assistance variable equal to one if any positive amount of benefit receipt is recorded during the calendar year. This method is convenient if data come from annual administrative records where information on the amount of benefits received is available but the exact timing of payments during the year is unknown (see Hansen et al. (2006), Hansen & Lofstrom (2008, 2011), or Andrén & Andrén (2013)).

[12] Data for the years 1984-2011, Version 28, SOEP, 2012, doi:10.5684/soep.v28

[13] In the years used for the analysis just below 80% of interviews have been conducted in the months January to April.

for individuals with a partner who is not of working-age (*i.e.* below 25 or above 59 years), observations for individuals in a household with a working-age member in full-time education, and all observations after a gap in an individual's interview sequence. Excluding the initial observation in each individual's interview sequence for which no lag is available, the resulting estimation sample consists of 17,733 individuals and 100,434 person-year observations.

I match the sample with annual data on unemployment rates in the individuals' state of residence from the German Federal Statistical Office (Statistisches Bundesamt, 2013). These data are used to control for differences in regional labour market conditions in the econometric analysis.[14]

# 4 Trends in benefit receipt

Germany has seen a slight rise in rates of social assistance benefit receipt over the 17 years of the observation period. As illustrated in the left panel of Figure 1, the frequency of benefit receipt among working-age individuals is initially relatively stable at around 7-8%. After 2001, rates of benefit receipt start rising strongly to peak at 12.7% in 2006. The beginning of this increase coincides with the start of a period of economic stagnation in Germany in the early 2000s. After 2006, the year after the Hartz reforms, the frequency of benefit receipt declines through the years of the Great Recession and drops to below 10% in 2011.

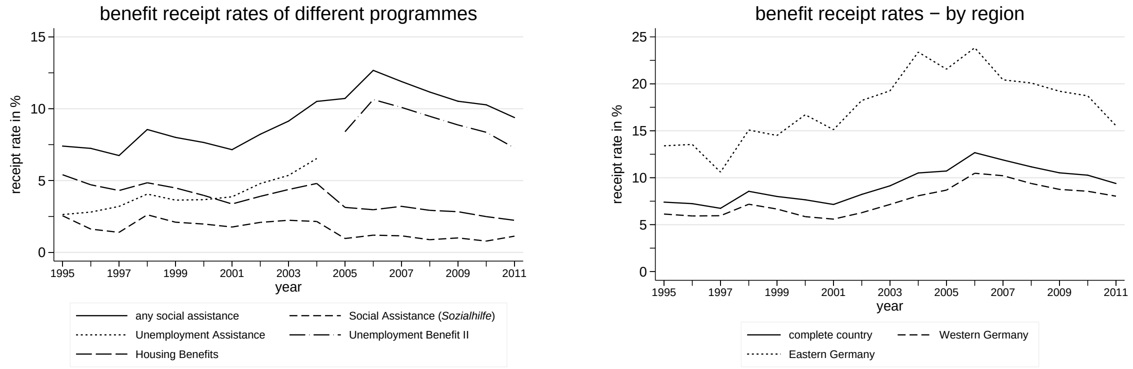
A breakdown of social assistance into the different programmes shows that trends for the different programmes differ. Rates of HB and SA receipt are relatively stable until 2005, but then drop visibly with the implementation of the Hartz reforms. By contrast, rates of UA receipt show an upward trend in the first decade of the panel, which continues for the newly introduced UBII in 2005 and 2006. The drop in SA receipt rates and the simultaneous jump in receipt rates from UA to UBII indicate that a large share of SA recipients were moved into UBII through the Hartz reforms. Similarly, HB receipt rates fall as recipients who are transferred from UA to UBII lose eligibility to HB. The decline in receipt rates after 2006 is primarily due to a reduced number of UBII recipients.

Patterns of benefit receipt still differ considerably between Eastern and Western Germany. As illustrated in the right panel of Figure 1, receipt rates in Eastern Germany are substantially higher averaging 17.6% compared to 7.6% for Western Germany. This difference is broadly comparable to the disparity in unemployment rates in the two parts of the country.[15] Benefit receipt rates in Eastern Germany show a weak upward trend even in the initial years of the panel and already peak in 2004. Receipt rates for Western Germany closely follow those for Germany overall, which reflects the fact that about 80% of observations in the sample are for Western Germany.

[14]The version of the SOEP used for the analysis does not permit for a distinction between the two German states of Saarland and Rhineland-Palatinate in each of the years of the observation period. I therefore allocate a weighted average of the unemployment rates of these two federal states to all individuals living in either of these states.

[15]Over the observation period, the average of the yearly unemployment rates was 8.0% in Western Germany compared to 16.0% in Eastern Germany (Bundesagentur für Arbeit, 2013).

Figure 1: rates of benefit receipt



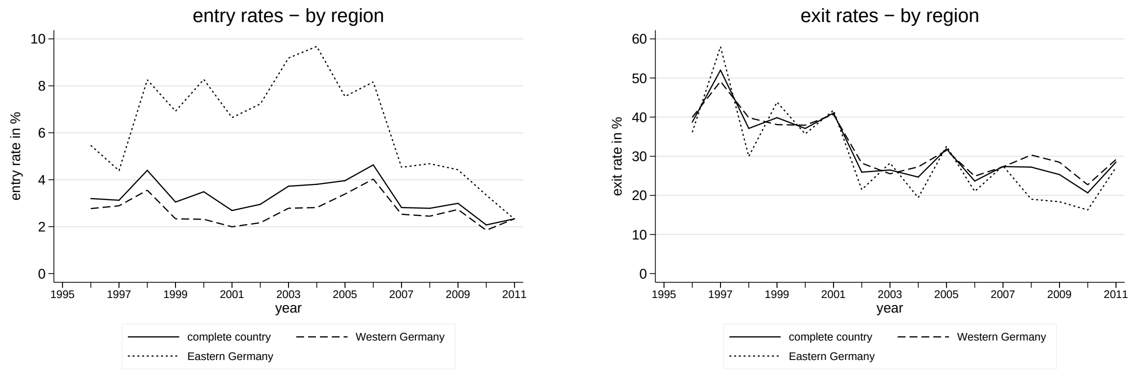

*Note:* Rates of benefit receipt were calculated using cross-sectional individual sampling weights. The frequency of benefit receipt is the share of working-age individuals who live in a benefit-receiving household at the time of the interview. *Source*: SOEP, 2011

Benefit transition rates plotted in Figure 2 show that the rise in the benefit receipt rates observed after 2001 seems to have been primarily due to a permanent drop in exit rates from benefit receipt. The share of individuals who report leaving benefits from one interview to the next falls from around or above 40% until 2001 to below 30% thereafter (right panel). This is remarkable, since earlier comparable work for Canada (Finnie & Irvine, 2008) and Britain (Cappellari & Jenkins, 2008, 2013) shows that declining rates of benefit receipt in these countries where primarily driven by falling entry rates while exit rates remained stable or declined as well. The rise in receipt rates in Eastern Germany during the late 1990s and, more importantly, the decline in receipt rates after 2006 appear to be due to changes in entry rates (left panel).

The breakdown of benefit transition rates by region again shows very different patterns in the two parts of the country. Exit rates are nearly identical with slightly stronger fluctuations for Eastern Germany due to the much smaller sample size. Entry rates into benefit receipt by contrast are up to four times higher in Eastern Germany than in Western Germany. They also show much more variation in Eastern Germany, rising from around 5% to 8% in 1998 and dropping again by the same amount from 2006 to 2007. The gap in social assistance receipt rates between Eastern and Western Germany shown in Figure 1 is thus due to much higher entry rates in Eastern Germany.

An important implication of these benefit transition rates is that there is indeed substantial observed (or 'raw') state dependence in benefit receipt as highlighted in the Introduction. Average exit rates of around 32% over the observation period imply that 68% of benefit recipients in a given year will continue to receive benefits in the following year. Entry rates into benefits by contrast average only around 3% over the same period. Observed state dependence is thus around 65%. At least some of this effect is of course likely to be driven by differences in individual and household characteristics between social assistance recipients and non-recipients. Königs (2013) shows for instance that benefit recipients in Germany are on average substantially more likely than non-recipients to have less than ten years of education or poor self-assessed health. Also, a larger share of benefit recipients are migrants, and the proportion of single parent households is about three times as high among benefit recipients than for non-recipients.

Figure 2: benefit transition rates




*Note:* Entry rates into benefit receipt are defined as the number of individuals in receipt of social assistance benefits at time $t$ but not at time *t-1* divided by the total number of individuals not in social assistance at time *t-1*. Similarly, exit rates from benefit receipt are defined as the number of individuals in receipt at time *t-1* but no longer in receipt at time $t$ divided by the total number of individuals in receipt at time *t-1*. Individuals observed for only one of the two waves have not been used in the calculations. Benefit transition rates were calculated using cross-sectional individual sampling weights for period $t$. *Source*: SOEP, 2011

Based on the descriptive evidence alone it is not obvious whether there might have been a structural change in benefit receipt dynamics around the time of the Hartz reforms. Receipt rates stopped rising in 2006, the year after the Hartz reforms, to decline thereafter. This trend was driven by a strong drop in entry rates from 2006 to 2007 especially in Eastern Germany. The decline in entry rates however mirrors a comparable earlier increase in the late 1990s, and exit rates remained mostly stable during the reform years.

The econometric model presented in the following Section attempts to determine whether there is evidence for 'structural' state dependence in Germany, and if so, whether the level of state dependence differs for the periods before and after the 2005 Hartz reforms.

# 5 Econometric approach

The econometric analysis is based on a dynamic random-effects probit model, the standard model in recent empirical work on the dynamics of social assistance receipt. Let $y_{it}$ be a binary outcome variable such that for $y_{it} = 1$ individual $i$ is in receipt of social assistance in period $t$. A latent variable specification for this outcome can be written as

$$\begin{aligned} y_{it} &= \mathbb{1}\{y_{it}^* > 0\} \\ &= \mathbb{1}\left\{x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + u_{it} > 0\right\} \quad \text{for} \quad i = 1, ..., N; \; t = 1, ..., T_i, \end{aligned} \tag{1}$$

where $y_{it}^*$ depends linearly on a vector of observable characteristics $x_{i(t-1)}$[16], the observed receipt status in the previous period $y_{i(t-1)}$ and an error term $u_{it}$. The latent variable $y_{it}^*$ can be interpreted as the potential utility from receiving social assistance, with the individual choosing benefit receipt for $y_{it}^* > 0$.

[16] An alternative specification of the model uses current values of the observable characteristics $x_{it}$. In using lagged values $x_{i(t-1)}$, I follow Cappellari & Jenkins (2009). The difference in results between the two approaches is however modest.

The error term can be decomposed as $u_{it} = \alpha_i + \varepsilon_{it}$, where $\alpha_i$ is an individual-specific random intercept and $\varepsilon_{it}$ is a transitory shock. The two error components are assumed to be mean zero and uncorrelated with each other. The persistent component $\alpha_i$ is by construction correlated with the lagged dependent variable $y_{i(t-1)}$ but initially assumed to be uncorrelated with the regressors $x_{i(t-1)}$, an assumption that is relaxed below. It is further assumed that the transitory shock $\varepsilon_{it}$ is standard normal and serially uncorrelated, that the benefit receipt dynamics are correctly represented by a first-order Markov process, and that the covariates $x_{i(t-1)}$ are strictly exogenous.[17]

Under these conditions, the probability of benefit receipt is given as

$$P(y_{it}|y_{i0}, ..., y_{iT}, x_i, \alpha_i) \quad = \quad \Phi(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \alpha_i), \tag{2}$$

where $x_i$ is the vector of an individual's characteristics in all time periods and $\Phi(\cdot)$ is the standard normal cumulative distribution function.

Following Heckman (1981a), the coefficient of the lagged dependent variable $\lambda$ in such a model is interpreted as measuring 'structural' state dependence. 'Spurious' state dependence induced by permanent unobserved heterogeneity is captured by the persistent individual-specific error term $\alpha_i$ that might be interpreted as representing differences in unobserved labour market ability or an individual's preference for benefit receipt.

A difficulty for estimation of this model is that the specification suffers from an initial conditions bias. As for linear dynamic panel data models with unobserved heterogeneity, the individual-specific error component $\alpha_i$ induces a correlation between the error term and the lagged dependent variable that leads to inconsistent estimates. Integrating out the individual-specific effect $\alpha_i$ requires specifying its relationship with the outcome in the initial period $y_{i0}$ that typically cannot be treated as exogenous.

The simplest approach for addressing the initial conditions problem has been proposed by Wooldridge (2005).[18] He suggests specifying a density for the individual-specific effect conditional on the outcome in the initial period and the covariates, which permits integrating out $\alpha_i$. More specifically, Wooldridge sets $\alpha_i = \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i$, with $a_i|y_{i0}, x_i \sim \mathcal{N}(0, \sigma_a^2)$. The vector $x_i$ contains here the values of time-varying covariates for all periods not yet already included in $x_{i(t-1)}$ and allows for a correlation of $\alpha_i$ with the covariates as proposed by Chamberlain (1982, 1984). Under this assumption, the joint density of $y_{i1}, ..., y_{iT}|y_{i0}, x_i$ unconditional

[17] Stewart (2006, 2007) estimates a comparable model however allowing for serial correlation in the transitory shock; Biewen (2009) permits feedback effects between the outcome variables and some of the regressors in his model of poverty dynamics in Germany.

[18] The earliest and most widely-used approach is due to Heckman (1981b), who suggests approximating the unknown density of $y_{i0}|x_i, \alpha_i$ to remove the conditioning on $\alpha_i$. A further approach proposed by Orme (2001) is used much less frequently in practice. Comparisons of the Heckman and Wooldridge estimators by Arulampalam & Stewart (2009) and Akay (2012) suggest that neither of them is strictly superior in terms of their finite-sample properties. Cappellari & Jenkins (2008) compare all three approaches in their analysis of social assistance benefit dynamics in Britain and find that they give nearly identical results.

on $\alpha_i$ can be written as

$$\int \prod_{t=1}^{T} \left[\Phi\left(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + x'_i\gamma_2 + a_i\right)\right]^{y_{it}} \times$$
$$\left[1 - \Phi\left(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + x'_i\gamma_2 + a_i\right)\right]^{1-y_{it}} \left(\frac{1}{\sigma_a}\right)\phi\left(\frac{a_i}{\sigma_a}\right) da_i. \quad (3)$$

This expression corresponds to the likelihood of the standard random-effects probit model with the additional explanatory variables $y_{i0}$ and $x_i$ added in each period $t$ and can be used for maximum likelihood estimation. In empirical practice, the vector of lags and leads of all time-varying covariates $x_i$ is typically replaced by an individual's longitudinal averages of these covariates $\bar{x}_i$ à la Mundlak (1978). This is also what I do in this article to reduce the number of regressors and thus computation time.[19] Consistency of this model relies on the assumption that unobserved heterogeneity is uncorrelated with the regressors once between-individual differences in observable characteristics are accounted for.

Due to the non-linearity of the model, the size of the coefficient estimates is little informative about the magnitude of the implied effects on the outcome variable. To evaluate the degree of state dependence, I therefore calculate the average partial effect (APE) of benefit receipt at the previous interview on benefit receipt at the current interview. Under the assumptions just discussed, I consistently estimate an individual's expected probability of social assistance benefit receipt in period $t$ as

$$\frac{1}{N}\sum_{i=1}^{N}\left(\Phi(x'_{i(t-1)}\hat{\beta} + \hat{\lambda} y_{i(t-1)} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2)(1-\hat{\rho})^{\frac{1}{2}}\right), \quad (4)$$

where $\hat{\rho} = \hat{\sigma}_a^2/(1+\hat{\sigma}_a^2)$ is estimated share of the variance of the composite error term that can be attributed to persistent unobserved heterogeneity (Wooldridge, 2005).

Following Stewart (2007), the APE of past benefit receipt is then defined as the difference in average predicted probabilities of social assistance receipt across individuals and time conditional on benefit receipt and non-receipt in the previous period:

$$APE = \frac{1}{NT}\sum_{t=1}^{T}\sum_{i=1}^{N}\left[\hat{P}\left(y_{it}=1|y_{i(t-1)}=1, x_{i(t-1)}, y_{i0}, \bar{x}_i\right) - \right.$$
$$\left. \hat{P}\left(y_{it}=1|y_{i(t-1)}=0, x_{i(t-1)}, y_{i0}, \bar{x}_i\right)\right]. \quad (5)$$

The APE measures structural state dependence in absolute terms by comparing average predicted entry and persistence probabilities across all individuals over time.

Alternatively, one can express the degree of state dependence in relative terms by calculating the predicted probability ratio (PPR), *i.e.* the ratio of average predicted probabilities with and

[19]Rabe-Hesketh & Skrondal (2013) warn that especially in short panels this simplification can lead to biased estimates. They suggest that also the initial values of all time-varying explanatory variables $x_{i0}$ should be included in the model as regressors when the simplified Wooldridge approach is used. I have tested this alternative specification and found it to give nearly identical results, which is why I only report results for the simplified Wooldridge approach.

without benefit receipt in the previous period:

$$PPR = \frac{\frac{1}{NT}\sum_{t=1}^{T}\sum_{i=1}^{N}\hat{P}\left(y_{it}=1|y_{i(t-1)}=1, x_{i(t-1)}, y_{i0}, \bar{x}_i\right)}{\frac{1}{NT}\sum_{t=1}^{T}\sum_{i=1}^{N}\hat{P}\left(y_{it}=1|y_{i(t-1)}=0, x_{i(t-1)}, y_{i0}, \bar{x}_i\right)}. \tag{6}$$

The results presented in the next Section were obtained using Stata's `xtprobit` command, which employs adaptive quadrature with twelve quadrature points for evaluation of the integrals. As a robustness check, all specifications have been re-estimated using Stata's `gllamm` command that permits robust standard errors and the use of sampling weights (Rabe-Hesketh, Skrondal & Pickles, 2004, 2005), but for which computation time is substantially higher. I find that the use of sampling weights leads to higher standard errors but does otherwise not strongly affect the estimation results. I therefore use weights only for the calculation of APEs and PPRs but not in the estimation process. Results from weighted estimation as well as robustness checks for balanced panels are provided by Königs (2013).

# 6 Estimation results

This Section presents estimation results for the complete sample and separately for Western and Eastern Germany. Covariates used in the estimation consist of personal characteristics (sex, age, years of education, health status, and migrant status), household characteristics (household type, a dummy for the presence of a child aged six years or younger in the household, and household size), and partner characteristics (age, years of education, health status, and migrant status)[20]. In the specification for the complete sample, I control for region of residence using a dummy variable for Eastern Germany. I moreover include a variable measuring the annual state-level unemployment rate in all specifications to capture regional and time differences in the economic environment and, unless noted otherwise, a set of year dummies to control for time trends in benefit receipt.

To address the endogeneity of initial conditions, I include in all specifications as 'Wooldridge controls' the receipt status in the initial period $y_{i0}$ as well as time-averages of the different family-type variables, the dummy for individuals living in a household with a child aged under six years, household size, the respondent's and her partner's health status, and the regional unemployment rate.

The division of the sample into Western and Eastern Germany is based on residence in the initial period in which an individual is observed. This is meant to help avoid possible endogeneities that might arise as sample members move from one part of the country to another although such moves are infrequent.[21]

[20] Partner characteristics are set equal to zero if the individual is single.

[21] The proportion of individuals who move from Eastern to Western Germany is indeed slightly higher among social assistance recipients than among non-recipients. However, only about 0.6% of sample members who live in Eastern Germany move to Western Germany in a given year, and only about 0.1% migrate in the opposite direction. Benefit-induced migration within Germany is thus unlikely to be an important issue for the analysis.

### The evidence for state dependence

I start the econometric analysis by presenting results from the standard version of the dynamic random-effects probit model described in the previous Section. Estimation results reported in Table 2 indicate that there is considerable state dependence in social assistance benefit receipt in Germany. Column I of Panel A, which gives coefficient estimates, shows that the coefficient of the lagged dependent variable is positive and strongly significant for the complete sample. Panel B presents the corresponding average predicted transition rates: I calculate an average predicted entry rate of 5.4% and an average predicted persistence rate of 18.4%. The resulting APE is 13.0 percentage points.

The result implies that even after controlling for observed and persistent unobserved characteristics, an individual in the sample is on average 13 percentage points more likely to report benefit receipt at the current interview if she already received benefit payments at the last interview. This corresponds to an increase in the probability of benefit receipt by a factor of 3.4 as indicated by the PPR. While an APE of 13 percentage points is substantial, the value is considerably lower than the difference between observed persistence and entry rates of about 65 percentage points shown in Figure 2. Most of the 'raw' state dependence is thus due to observed and unobserved heterogeneity across individuals.

Results for Western and Eastern Germany show strong disparities in average predicted transition rates, but relatively similar levels of state dependence in absolute terms. Columns II and III of Panel A again give significantly positive coefficient estimates for the lagged dependent variable in both subsamples. Average predicted entry and persistence rates for Western Germany are very close but lower than for the entire country. For Eastern Germany, both predicted entry and persistence rates are substantially higher as one would expect. State dependence in Western and Eastern Germany is comparable when measured in absolute terms at 13.5 percentage points in Western Germany and 15.2 percentage points in Eastern Germany. In relative terms, the effect of past benefit receipt is however much stronger for Western Germany where receipt rates are much lower: The PPR implies that benefit receipt at the time of the last interview raises the likelihood of benefit receipt at the current interview by a factor of 4.2 for Western Germany compared to 2.2 in Eastern Germany.

A methodological point worth mentioning is that predicted transition rates for Western and Eastern Germany are *within-sample* predictions in the sense that they have been calculated for the respective subsamples used for estimation rather than over all sample members in Germany. A disadvantage of this approach is arguably that results are less comparable, as – due to the non-linearity of the model – they depend on the distributions of observable characteristics in the two subsamples. The reason why I have nonetheless opted for this approach is that results by region can be straightforwardly be interpreted as the decomposition of the results for the entire country. By contrast, I found that *out-of-sample* predictions based on coefficient estimates for Western or Eastern Germany over all individuals in Germany can give very counter-intuitive results, for instance with predicted transition rates in each of the two regions being higher than in Germany overall.

Table 2: baseline specifications

**- Panel A -**

| | complete country | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| $y_{t-1}$ | 1.160*** | (0.029) | 1.259*** | (0.036) | 0.977*** | (0.048) |
| *individual characteristics* | | | | | | |
| female | -0.004 | (0.029) | -0.010 | (0.033) | 0.023 | (0.057) |
| age | -0.078*** | (0.013) | -0.077*** | (0.015) | -0.080*** | (0.025) |
| age$^2$ | 0.088*** | (0.015) | 0.087*** | (0.017) | 0.089*** | (0.029) |
| years of education | -0.293*** | (0.042) | -0.252*** | (0.045) | -0.725*** | (0.127) |
| years of education$^2$ | 0.007*** | (0.002) | 0.006*** | (0.002) | 0.021*** | (0.005) |
| good health | -0.060** | (0.028) | -0.070** | (0.034) | -0.027 | (0.051) |
| poor health | 0.080** | (0.035) | 0.097** | (0.041) | 0.020 | (0.066) |
| migrant | 0.268*** | (0.045) | 0.261*** | (0.046) | 0.263 | (0.186) |
| *household characteristics* | | | | | | |
| single, with children | 0.029 | (0.064) | 0.100 | (0.082) | -0.043 | (0.104) |
| couple, no children | 0.061 | (0.073) | 0.095 | (0.089) | 0.022 | (0.132) |
| couple, with children | -0.076 | (0.075) | -0.061 | (0.092) | -0.047 | (0.131) |
| child $\leq$ 6 years | 0.096** | (0.039) | 0.166*** | (0.045) | -0.083 | (0.076) |
| household size | 0.062*** | (0.022) | 0.047* | (0.026) | 0.108** | (0.043) |
| *partner characteristics* | | | | | | |
| age | -0.018*** | (0.004) | -0.016*** | (0.005) | -0.021*** | (0.008) |
| age$^2$ | 0.022*** | (0.007) | 0.020*** | (0.008) | 0.026* | (0.013) |
| years of education | 0.056*** | (0.012) | 0.046*** | (0.014) | 0.086*** | (0.025) |
| years of education$^2$ | -0.005*** | (0.001) | -0.004*** | (0.001) | -0.006*** | (0.001) |
| good health | 0.003 | (0.031) | 0.002 | (0.038) | 0.018 | (0.057) |
| poor health | 0.155*** | (0.041) | 0.175*** | (0.048) | 0.097 | (0.076) |
| migrant | 0.163*** | (0.048) | 0.163*** | (0.049) | 0.504** | (0.201) |
| *calendar-year effects* | | | | | | |
| 1997 | -0.116** | (0.055) | 0.061 | (0.064) | -0.525*** | (0.108) |
| 1998 | 0.039 | (0.058) | 0.097 | (0.070) | 0.049 | (0.114) |
| 1999 | -0.100* | (0.057) | -0.016 | (0.068) | -0.155 | (0.115) |
| 2000 | -0.076 | (0.058) | -0.100 | (0.070) | 0.057 | (0.111) |
| 2001 | -0.083 | (0.052) | -0.108* | (0.062) | 0.066 | (0.104) |
| 2002 | -0.081 | (0.053) | -0.130** | (0.065) | 0.114 | (0.106) |
| 2003 | 0.097* | (0.053) | 0.068 | (0.063) | 0.278*** | (0.107) |
| 2004 | 0.080 | (0.055) | 0.054 | (0.065) | 0.316*** | (0.115) |
| 2005 | 0.129** | (0.056) | 0.169*** | (0.065) | 0.229** | (0.116) |
| 2006 | 0.190*** | (0.060) | 0.195*** | (0.075) | 0.410*** | (0.118) |
| 2007 | -0.018 | (0.058) | 0.043 | (0.071) | -0.005 | (0.111) |
| 2008 | 0.049 | (0.057) | 0.015 | (0.069) | 0.134 | (0.105) |
| 2009 | 0.136** | (0.060) | 0.158** | (0.072) | 0.019 | (0.111) |
| 2010 | 0.155** | (0.063) | 0.095 | (0.076) | 0.208* | (0.115) |
| 2011 | 0.084 | (0.068) | 0.108 | (0.080) | -0.084 | (0.130) |
| *Wooldridge controls* | | | | | | |
| $y_0$ | 1.268*** | (0.048) | 1.222*** | (0.059) | 1.335*** | (0.086) |
| avg: good health | -0.110** | (0.055) | -0.080 | (0.066) | -0.192* | (0.103) |
| avg: poor health | 0.308*** | (0.071) | 0.332*** | (0.083) | 0.217 | (0.143) |
| avg: single, with children | 0.304*** | (0.093) | 0.245** | (0.113) | 0.392** | (0.173) |
| avg: couple, no children | -0.360*** | (0.090) | -0.447*** | (0.108) | -0.250 | (0.167) |

– continued on next page –

Table 2 – continued from previous page –

| | complete country | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| avg: couple, with children | -0.267*** | (0.098) | -0.358*** | (0.116) | -0.107 | (0.187) |
| avg: child $\leq$ 6 years | 0.267*** | (0.064) | 0.192*** | (0.073) | 0.525*** | (0.133) |
| avg: household size | 0.059** | (0.028) | 0.086*** | (0.032) | -0.030 | (0.058) |
| avg: reg. unemployment rate | 0.024** | (0.010) | 0.042** | (0.018) | 0.005 | (0.017) |
| avg: good health (partner) | -0.096* | (0.057) | -0.024 | (0.068) | -0.227** | (0.107) |
| avg: poor health (partner) | 0.209*** | (0.079) | 0.260*** | (0.092) | 0.115 | (0.161) |
| reg. unemployment rate | 0.037*** | (0.009) | 0.021 | (0.017) | 0.007 | (0.016) |
| Eastern Germany | 0.247*** | (0.056) | | | | |
| constant | 0.923** | (0.375) | 0.549 | (0.423) | 5.096*** | (0.998) |
| $\sigma_a$ | 0.831*** | (0.025) | 0.773*** | (0.030) | 0.959*** | (0.047) |
| $\rho$ | 0.409*** | (0.014) | 0.374*** | (0.018) | 0.479*** | (0.025) |
| log Likelihood | -14,538.474 | | -9,671.444 | | -4,776.992 | |
| # of observations | 100,434 | | 79,790 | | 20,644 | |
| # of individuals | 17,733 | | 14,010 | | 3,723 | |

* p<0.10, ** p<0.05, *** p<0.01

*Note:* Standard errors are given in parentheses. $y_{t-1}$ and $y_0$ are the observed social assistance receipt status in the last period and the initial observed period, respectively. All other covariates with the exception of calendar-year dummies are lagged by one period. Among the 'Wooldridge controls', I use the prefix 'avg:' to refer to an individual's time-average for this variable. Values of age$^2$ have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011

**- Panel B -**

| | complete country | Western Germany | Eastern Germany |
|---|---|---|---|
| average predicted entry rate in % | 5.4 (0.2) | 4.2 (0.1) | 12.3 (0.6) |
| average predicted persistence rate in % | 18.4 (0.7) | 17.7 (0.9) | 27.5 (1.2) |
| Average Partial Effect (in ppts) | 13.0 (0.7) | 13.5 (0.9) | 15.2 (1.3) |
| Predicted Probability Ratio (in ppts) | 3.4 (0.2) | 4.2 (0.3) | 2.2 (0.1) |

*Note:* Calculations are based on the coefficient estimates presented in Panel A. All averages have been calculated using individual cross-sectional sampling weights. Standard errors in parenthesis were obtained by bootstrapping with 100 replications. Results for Western and Eastern Germany are *within-sample* predictions.

The links between individual-, household-, and partner-level characteristics and the likelihood of benefit receipt tend to have the direction one would assume. Since there are few differences between results for Western and Eastern Germany – except for the lower statistical significance of coefficients estimates for Eastern Germany due to the smaller sample size – I limit the discussion to estimates for the entire country.

Looking first at the impact of individual-level explanatory variables, a surprising finding is that, *ceteris paribus*, the sex of the respondent does not seem to be related to the risk of benefit receipt as indicated by the insignificant coefficient on the female dummy.[22] The effect of age on the outcome variable is u-shaped with a minimum at age 44 implying that young adults and older individuals have a higher probability of benefit receipt. Education is associated negatively with social assistance receipt at a slightly diminishing rate for higher years of education as suggested by the positive coefficient on the quadratic term (the minimum is at about 21 years

[22]For a detailed breakdown of the benefit dynamics by sex, see Königs (2013).

of education). As one would expect, poor health is associated with a higher probability of benefit receipt and healthier individuals are less likely to receive benefits. Even after controlling for personal characteristics, first- or second-generation migrants are significantly more likely to receive social assistance than natives.

A first impression from looking at the controls for household-level characteristics may be that these variables are not strongly associated with benefit receipt status since for instance all family-type variables are insignificant. This result may however primarily reflect insufficient time-variation in those variables over the observation period. The time-averages of these variables among the Wooldridge controls are highly significant: Living in a couple (with or without children) is a associated with a lower probability of benefit receipt and being a single parent is associated with a higher likelihood of benefit receipt (both compared to the base category singles without children). These findings however have to be interpreted with care, because the time-averages were only included in the specification to capture persistent differences in unobserved factors that they might be correlated with. Both household size and the dummy variable for a child aged 6 years and younger in the household enter positively. This might reflect the greater generosity of the means-test for larger households.

Also the partner's characteristics appear highly relevant for determining an individual's social assistance receipt. The variable controlling for the partner's age displays a profile similar to that of the respondent with the size of the effect falling until age 41 and rising thereafter. There is again a negative relationship between the partner's education and the likelihood of benefit receipt, with additional eduction reducing the risk of benefit receipt at an increasing rate. Finally, respondents whose partner suffers from poor health and those with a migrant partner are more likely to receive benefits.

Further down in Table 2, the coefficient estimate for the state-level unemployment rate indicates that living in a region with higher unemployment is associated with a higher likelihood of benefit receipt. The positive coefficient of the dummy for residence in Eastern Germany implies that even once socio-economic characteristics and the state-level unemployment rate are controlled for, the probability of benefit receipt is higher for individuals living in the east.

The model captures time trends in benefit receipt during the observation period through a series of year dummies measured in reference to the year 1996. The large majority of year coefficients in the model is insignificant, which suggests that the model does relatively well at explaining the time trends observed in Section 4. I obtain significantly positive coefficient estimates for the years 2005 and 2006, which indicates *ceteris paribus* a higher probability of social assistance receipt in the years directly after implementation of the Hartz reforms. Yet, this rise in the probability of benefit receipt is transitory as the estimated coefficients for the years 2007 and 2008 are very close to zero again and statistically insignificant. The coefficient estimates for the years 2009 and 2010 are positive and significant suggesting a higher probability of benefit receipt during the economic crisis. While a negative coefficient estimate is observed for the year 1997, there does not appear to be a clear time trend or any systematic difference in the probability of benefit receipt before and after the Hartz reforms.

### Time-variation in state dependence

The results presented thus far suggest that social assistance benefit receipt in Germany is characterised by a substantial degree of state dependence. As outlined in Section 3 the institutional framework for the provision of minimum-income benefits in Germany underwent a major reform during the observation period. Since one explicit aim of these reforms was to strengthen the activating elements in social assistance, an interesting question is whether based on the models I can find any evidence for changes in the degree of state dependence around the time of the Hartz reforms.

I begin with a comparison of the level of state dependence before and after the Hartz reforms by re-writing the standard model as

$$\begin{aligned} y_{it} \quad &= \quad \mathbb{1}\left\{x'_{i(t-1)}(\beta^0 + \beta^1 H_t) + (\lambda^0 + \lambda^1 H_t)y_{i(t-1)} + (\gamma_1^0 + \gamma_1^1 H_t)y_{i0} + \right. \\ & \left. \bar{x}'_i(\gamma_2^0 + \gamma_2^1 H_t) + a_i > 0\right\} \quad \text{for} \quad i = 1, ..., N;\ t = 1, ..., T_i, \end{aligned} \tag{7}$$

where $H_t$ is a dummy variable for the post-Hartz period that takes the value of one in the years 2005-2011 and is zero otherwise.

This specification allows for differences in the processes driving benefit receipt before and after the Hartz reforms by letting the coefficients of the lagged dependence variable $y_{i(t-1)}$, of the covariates $x_{i(t-1)}$ and of the Wooldridge controls $y_{i0}$ and $\bar{x}_i$ differ between those two periods. Note that unlike in the standard specification the covariate vector $x_{i(t-1)}$ now no longer includes a set of year dummies. The time trend is instead captured by an interaction of the post-Hartz dummy with the intercept term included in $x_{i(t-1)}$. Unobserved heterogeneity is assumed not to vary over time.

I evaluate the degree of state dependence implied by the model estimates by calculating again predicted entry and persistence rates and the average partial effect across all individuals in the sample. A comparison of pre- and post-reform state dependence is carried out by making counterfactual assumptions on the period of observation switching the post-Hartz dummy 'on' and 'off' for each individual. The results of this exercise are presented in Table 3. Coefficient estimates are provided in Table A.1 in the Appendix but are not discussed in detail.

Estimation results show an increase in state dependence from the pre-Hartz to the post-Hartz period. Average predicted entry rates into benefits for the complete country increase by 1 percentage point from 1996-2004 to 2005-2011, with the effect being slightly lower in Western Germany (0.8 ppts) and larger in Eastern Germany (1.8 ppts). Average predicted persistence rates rise by 3.6 percentage points (2.5 ppts in Western Germany, 6.1 ppts in Eastern Germany). The implied APE rises by 2.6 percentage points for the entire country, by (insignificant) 1.7 percentage points in Western Germany and by 4.3 percentage points in Eastern Germany. Measured in relative terms, state dependence by contrast remained stable with the change in PPRs being statistically different from zero in none of the three samples.

An aspect worth noting is that the predicted increase in state dependence for the complete country described in Table 3 is entirely due to a change in the coefficient estimates of the covariates, in particular the intercept term. Unlike for Eastern Germany, the coefficient estimate of the lagged dependent variable does not vary over time in the specification for the complete country as illustrated by the insignificant interaction term between the lagged dependent variable and the post-Hartz dummy reported in Table A.1. The increase in state dependence in the data is thus entirely captured by the non-linearity of the model. This non-linearity means that changes in the effect of observable characteristics and the intercept term indirectly affect the impact of the lagged dependent variable and thus the estimated degree of state dependence. When using a non-linear model to evaluate changes in state dependence over time by interacting the lagged dependent variable with a time dummy, it is thus not sufficient to focus alone on the significance of the interaction term if the model also includes a time trend.

Table 3: pre- *vs.* post-Hartz variation in state dependence

**- complete country -**

| | 1996-2004 | 2005-2011 | Δ |
|---|---|---|---|
| average predicted entry rate in % | 5.2 (0.2) | 6.2 (0.2) | 1.0*** (0.2) |
| average predicted persistence rate in % | 16.6 (0.7) | 20.2 (1.0) | 3.6*** (0.9) |
| Average Partial Effect | 11.5 (0.7) | 14.0 (0.9) | 2.6*** (0.9) |
| Predicted Probability Ratio | 3.2 (0.2) | 3.3 (0.2) | 0.0 (0.2) |

**- Western Germany -**

| | 1996-2004 | 2005-2011 | Δ |
|---|---|---|---|
| average predicted entry rate in % | 3.9 (0.2) | 4.7 (0.2) | 0.8*** (0.2) |
| average predicted persistence rate in % | 16.3 (0.9) | 18.8 (1.2) | 2.5** (1.2) |
| Average Partial Effect | 12.4 (0.9) | 14.0 (1.2) | 1.7 (1.2) |
| Predicted Probability Ratio | 4.5 (0.3) | 4.3 (0.3) | -0.2 (0.3) |

**- Eastern Germany -**

| | 1996-2004 | 2005-2011 | Δ |
|---|---|---|---|
| average predicted entry rate in % | 12.0 (0.6) | 13.8 (0.8) | 1.8*** (0.7) |
| average predicted persistence rate in % | 24.8 (1.3) | 30.8 (1.9) | 6.1*** (2.0) |
| Average Partial Effect | 12.7 (1.4) | 17.0 (1.8) | 4.3** (1.9) |
| Predicted Probability Ratio | 2.1 (0.1) | 2.2 (0.2) | 0.2 (0.2) |

* p<0.10, ** p<0.05, *** p<0.01
*Note:* Calculations are based on the coefficient estimates presented in Table A.1 in the Appendix. Δ gives the change between columns 1 and 2. All averages have been calculated using individual cross-sectional sampling weights. Standard errors in parenthesis were obtained by bootstrapping with 100 replications. Results for Western and Eastern Germany are *within-sample* predictions. *Source*: SOEP, 2011

For a better impression of the timing of changes in state dependence, I estimate an alternative specification that allows the effect of the lagged dependent variable on the likelihood of benefit receipt to vary on a year-by-year basis. I again extend the standard specification, this time allowing for interactions between the lagged dependent variable and calendar-year dummies $\mu^{\tau}$ writing

$$y_{it} = \mathbb{1}\left\{x'_{i(t-1)}\beta + (\lambda + \sum_{\tau=1997}^{2011} \mu^{\tau})y_{i(t-1)} + \gamma_1 y_{i0} + \bar{x}'_i\gamma_2 + a_i > 0\right\}$$
$$\text{for } \; i = 1, ..., N; \; t = 1, ..., T_i. \quad (8)$$

As the standard model, this model now again includes a simple time trend in the form of a set of year dummies included in the covariate vector $x_{i(t-1)}$. The effects of all other variables are assumed to be constant over time.[23]

This specification is more flexible than the the previous one in allowing for richer variation in the effect of lagged benefit receipt. It is by contrast more restrictive in imposing that the coefficients of other covariates do not change over time. The results presented in Table A.1 however indicate that most of the time-variation in benefit dynamics in the model described in Equation (7) is driven by a change in the intercept term such that this simplification does not appear too problematic.

To assess variations in the degree of state dependence, I again use the coefficient estimates obtained from the model to predict entry and persistence rates and the implied average partial effect. As for the specification described by Equation (7), time-variation in state dependence is evaluated by making counterfactual assumptions on the period of observation. For each individual in the sample, I now predict entry and persistence rates in each of the 15 years in the observation period by 'switching on' the respective year dummy and its interaction with the lagged dependent variable. Figure 3 plots the predicted average entry and persistence rates in each year (in the panels on the left) and the year-by-year time variation in the APEs compared to the initial year 1996 (on the right). Coefficient estimates for the specification are reported in Table A.2 in the Appendix.

The upper-two panels show changes in predicted transition rates and state dependence for the complete sample. I find that entry rates in benefit receipt are very stable over time at around 5% per year with rises to 6% in 2005-6 and 2010-11. The average predicted persistence rate drops from 19% in 1996 to below 13% in 1997, but then starts rising to reach 20% in 2003 and to peak at 26% in 2010.

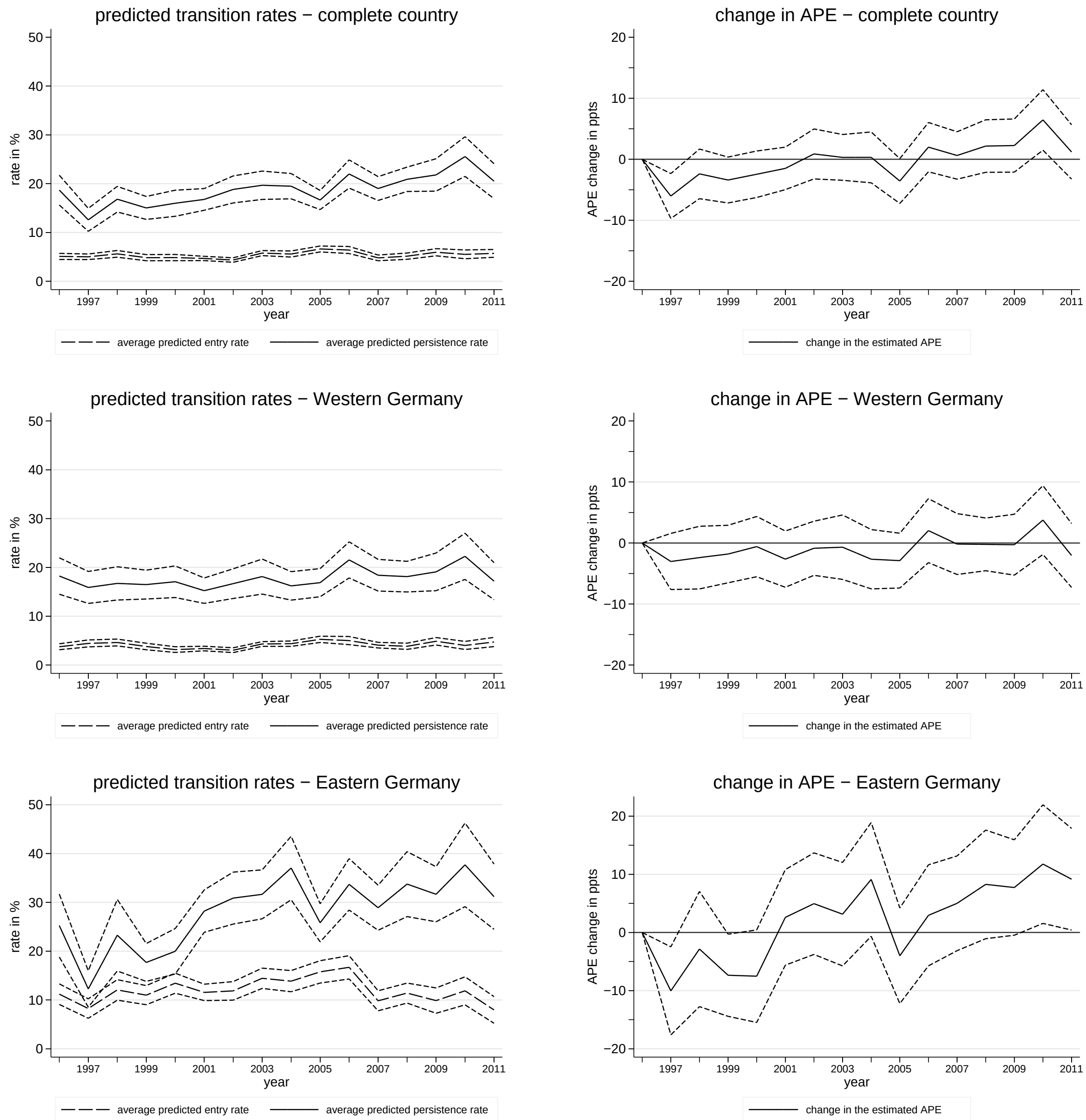
The top-right panel plots the variation in the average partial effect with respect to the reference year 1996, *i.e.* the difference in the vertical distance between average predicted persistence and entry rates in the given year compared to that in the initial year of the observation period. As one would expect from looking at the top-left panel, the level of estimated state dependence is lowest in 1997 and then slowly starts to rise eventually peaking in 2010. The annual APE is however statistically different from the one measured in the initial year 1996 only in 1997 (when it is significantly lower) and in 2010 (when it is significantly greater). A negative blip in the change in APEs for the year 2005 is just on the border of being statistically significant.

The corresponding graphs for Western Germany – shown in the middle-two panels – offer few additional insights. As observed for other specifications, average predicted persistence and entry rates are very similar to those for the entire country. The same is consequently true also for the year-to-year variations in the average partial effects, now however with none of the annual APEs calculated being significantly different from the one for the initial period. Time variations in predicted transition rates appear to be stronger for Eastern Germany (bottom-two panels). Results are however less easy to interpret due to the much smaller sample size and thus much wider confidence bands. Predicted benefit persistence rates are initially only slightly higher than for Western Germany however rise strongly in the early 2000s. They remain relatively stable

[23] The same type of specification has previously been estimated by Cappellari & Jenkins (2008) in their analysis of social assistance dynamics in Britain.

over the second part of the observation period at around 30-35% with a one-year drop in 2005. Predicted entry rates are about twice as high as for Western Germany, appear to rise through the early- and mid-2000s, but then drop substantially after the Hartz reforms from around 17% in 2006 to 10% in 2007.

Figure 3: year-by-year variation in state dependence




*Note:* Calculations are based on the coefficient estimates presented in Table A.2 in the Appendix. The left panels show year-by-year averages of predicted benefit persistence and entry rates. The right panels show the change in the annual average partial effect compared to the one in the initial year 1996. The dotted lines give the 95% confidence intervals. All averages have been calculated using individual cross-sectional sampling weights. Standard errors were obtained by bootstrapping with 100 replications. Results for Western and Eastern Germany are *within-sample* predictions. *Source*: SOEP, 2011

Estimated year-by-year changes in APEs are much more pronounced in Eastern Germany. The APE appears to be around 5 percentage points lower than in the initial period in the late 1990s and around 5 percentage points higher than in the initial period from 2001. I moreover again find a strong drop in the APE for the year 2005 and a rise up to around 10% for the final years of the observation period. Because of very wide confidence intervals, however, only the drops in the APE for the years 1997 and 1999 and the increase for the years 2010 and 2011 are statistically significant at the 5% level.

Overall, I conclude based on the model estimates that there is no evidence for a change in state dependence around the time of the Hartz reforms possibly except for a one-time drop in 2005. A specification that interacts the lagged dependent variable and all regressors with a dummy for the post-Hartz years shows that state dependence over the years 2005-2011 was 2.6 percentage points higher compared to the years 1996-2004. Allowing for year-by-year changes in state dependence however suggests that this effect is due to lower state dependence in the late 1990s and stronger state dependence during the crisis years in Eastern Germany. State dependence in Western Germany shows not significant time variation. More generally, the magnitude of estimated state dependence appears to be positively associated with benefit receipt rates being relatively low in the 1990s and increasing along with rising receipt rates from 2001 onwards. Most of these variations however do not reach statistical significance.

### The sensitivity of results to changes in the methodology

The finding of positive state dependence in social assistance receipt confirms similar results from comparable earlier studies for other countries. The APE of 13 percentage points reported in Table 2 is relatively close for instance to the value of 14.4 percentage points estimated by Cappellari & Jenkins (2008) in their analysis for Britain with the same methodology. The findings just reported by contrast differ surprisingly from those presented by Wunder & Riphahn (2013) in their study on state dependence in social assistance receipt for Germany.

Using SOEP data for the years 2005-2009, Wunder & Riphahn estimate a dynamic multinomial logit model of transitions between receipt of 'welfare' benefits (Unemployment Benefit II), employment and 'inactivity' (which they define as including unemployment).[24] They find evidence of structural state dependence in all three states modelled, the effect size however is much smaller than the one reported in this article. For natives, the authors calculate a predicted persistence rate in welfare of 3.1% and predicted entry rates into welfare of 1.6% from inactivity and of 0.5% from employment. For immigrants, the predicted persistence rate is 9.0%, compared to an entry rate of 3.8% from inactivity and of 1.8% from employment. The partial effect of past welfare receipt on welfare receipt in the current period lies thus between 1.5 and 2.6 percentage points for natives and between 5.2 and 7.2 percentage points for immigrants (Wunder & Riphahn, 2013, Table 5).[25] Since moreover exit rates to employment are higher from welfare than from inactivity for both natives and immigrants, they conclude that there is no evidence of a 'welfare trap'.

One difference between my study and the one by Wunder & Riphahn is methodological. While the analysis presented in this article is based on a binary dynamic probit model, Wun-

[24] For individuals who receive welfare benefits while being employed or unemployed welfare receipt is defined as the overriding state.

[25] They argue that the higher welfare entry rates from inactivity rather than those from employment should be used for assessing state dependence, because transitions from employment to welfare might be influenced more by employment protection legislation than by features of the welfare system. All their predicted transition rates have been calculated for a randomly selected individual, which the authors however show to yield a result similar to the average transition rate across all individuals.

der & Riphahn estimate a more complex dynamic multinomial logit model that distinguishes between inactivity and employment among non-recipients of welfare benefits. A second important difference lies in sample selection and in the way the benefit variable is defined. Wunder & Riphahn limit their attention to receipt of post-Hartz UBII and the years 2005-2009. They further restrict their analysis to Western Germany and drop individuals with a disability from the sample. Finally, Wunder & Riphahn define benefit receipt at the *individual* level using only information from the respondent's personal questionnaire. This approach is different from the one taken in this study (and in most of the related literature) of defining social assistance receipt at the *household* level.

To assess the reason for the difference in findings between the two studies, I use my simpler model to study the UBII receipt dynamics for the years 2005-2009 only. I then vary the way of defining the benefit variable (household- *vs.* individual-level definition) and the estimation sample (standard sample *vs.* restricted sample of natives in Western Germany without disability).[26] The results of this exercise are presented in Table 4.

Table 4: The impact of sample selection and the definition of the benefit variable on estimated state dependence

| | standard sample, 2005-2009 | | | restricted sample, 2005-2009 | | |
|---|---|---|---|---|---|---|
| | entry rate in % | persistence rate in % | APE in ppts | entry rate in % | persistence rate in % | APE in ppts |
| *Benefit variable defined at the* | | | | | | |
| household level | 6.0 (0.4) | 14.2 (1.4) | 8.3 (1.5) | 3.5 (0.5) | 8.7 (1.5) | 5.2 (1.7) |
| individual level | 4.5 (0.4) | 9.0 (1.1) | 4.6 (1.2) | 2.7 (0.5) | 5.6 (1.6) | 2.9 (1.8) |
| # of individuals | | 6,769 | | | 4,083 | |
| # of observations | | 22,289 | | | 13,249 | |

*Note:* The standard sample is defined using the sample selection criteria outlined in Section 3. The 'restricted' sample excludes individuals who live in Eastern Germany, disabled individuals, and first- or second-generation migrants. The benefit variable measures receipt of Unemployment Benefit II. For the household-level definition, I categorize an individual as a recipient if benefit payments are recorded for any member of the household; for the individual-level definition, I only use information on benefit receipt reported in the personal questionnaire. Standard errors in parentheses have been obtained from bootstrapping with 100 replications. Coefficient estimates are not reported but available from the author upon request. *Source*: SOEP, 2011

The level of estimated state dependence in benefit receipt is highly sensitive to both the approach used for defining the benefit variable and the sample selection criteria. For the standard sample and the household-level definition of the benefit variable, I calculate an average partial effect of 8.3 percentage points. The disparity between this value and the much higher 13 percentage points reported for the standard specification in Table 2 reflects the lower rate of benefit receipt once Social Assistance and Housing Benefit are no longer considered. Switching from the household-level to the individual-level definition of the benefit variable then reduces this APE by nearly half to 4.6 percentage points. The restriction of the sample to natives without disability in Western Germany leads to a further reduction in estimated state dependence. For the household-level definition, the estimated APE declines from 8.3 to 5.2 percentage points; for

[26] As in Wunder & Riphahn (2013) 'natives' are defined as sample members who are not first- or second-generation immigrants irrespective of citizenship. I do not replicate Wunder & Riphahn's sample selection criteria exactly in that the upper age threshold of 59 years I use is slightly more restrictive than theirs of 65 years.

the specification that uses the individual-level benefit variable, the APE drops from 4.6 to 2.9 percentage points. This APE of 2.9 percentage points is very close to the 1.5 to 2.6 percentage points reported by Wunder & Riphahn for a similar sample and definition of the benefit variable. The APE is moreover no longer significantly different from zero even at the 10% level ($t$-value of 1.62).

A quick robustness check thus suggests that the results presented in this paper and in the study by Wunder & Riphahn (2013) can easily be reconciled. As outlined in Section 3, I believe that there are good reasons to define the benefit variable at the household level as it is done in this article. The differences observed in Table 4 might thus be an indication that a benefit variable defined at the individual level correctly captures the benefit receipt status of the claimant, but not necessarily also that of other family members in a larger benefit unit. To the extent that this is the case, defining the benefit variable at the individual level is likely to lead to an underestimate of receipt rates and the estimated level of state dependence.

# 7 Conclusion

In this paper, I have studied state dependence in social assistance benefit receipt in Germany using annual data from the German Socio-Economic Panel (SOEP) for the years 1995 to 2011. Estimating a series of dynamic random-effects probit models that control for observed and persistent unobserved heterogeneity, I found evidence of substantial structural state dependence: Benefit receipt at the last interview is on average associated with a rise in the likelihood of benefit receipt at the current interview by 13 percentage points. This corresponds to an increase by a factor of 3.4. Predicted benefit entry and persistence rates and the resulting absolute level of state dependence are higher in Eastern Germany than in Western Germany. Large disparities between observed and estimated structural state dependence however illustrate that individual characteristics are the most important determinant of benefit transitions.

An analysis of variations in state dependence over the observation period does not provide evidence of a difference in patterns before and after the Hartz reforms. A simple comparison of the periods 1996-2004 and 2005-2011 suggests that estimated state dependence was 2.6 percentage points higher in the years after the Hartz reforms than in the years before. However, looking at year-to-year changes in state dependence, I found that this effect is likely driven by a period of lower state dependence in the late 1990s and a spike in state dependence in Eastern Germany in 2010. For none of the other years I found significant variations in state dependence compared to the initial year 1996.

A methodological insight from this article is finally that the magnitude of estimated state dependence can be highly sensitive to the method used for constructing the benefit variable. For the means-tested Unemployment Benefits II that was introduced by the Hartz reforms, I showed that defining the benefit variable at the individual rather than at the household level can lead to substantially lower estimated state dependence. An implication of this finding is that the results presented in this article are consistent with those reported by Wunder & Riphahn (2013), who – based on a slightly different methodological approach – find little evidence for state dependence in UBII receipt in post-Hartz Western Germany.

The policy implications of these findings depend much on what assumptions one makes about likely sources of structural state dependence in social assistance. The results presented in this paper are consistent with 'scarring' effects in social assistance that might arise if benefit receipt affects an individual's job-search behaviour or reservation wages. In this case, it is somewhat surprising that the analysis does not show a decline in the level of state dependence following the Hartz reforms. While clearly, the model estimated in this article is not suited to identify the causal effects of a policy change, one might nonetheless have expected the level of state dependence to fall with the introduction of stronger job search requirements and increased incentives to take up work.

A potential explanation for the lack of measurable time variation in state dependence might be that the effect of the Hartz reforms varies by recipient group. For instance, state dependence might have declined for employable benefit recipients for whom the benefit system got less generous as they were moved from UA to UBII; no such effect might in contrast exist for those judged unable to work who received SA after the reforms. The number of recipient households in the SOEP however is too low to permit splitting the analysis by benefit programme. Separate analyses for women and men and for natives and migrants gave no evidence of time variations in state dependence for any of these groups (Königs, 2013). A point worth noting is moreover that the partial effects reported in this article are (sub)population averages and might well differ from the effect on the 'treated' group of benefit recipients.

A different reason for why even a successful policy reform might not translate into a decline in state dependence is that it might lower both benefit entry and persistence rates, for instance as benefit generosity is reduced. Since state dependence is calculated as the difference between predicted persistence and entry rates, such a reform might not at all affect the level of state dependence. In Eastern Germany, average predicted entry rates into social assistance seem to have fallen after the Hartz reforms at relatively stable average predicted persistence rates such that state dependence increased (though most of these changes were not statistically significant). A policy reform that works by keeping individuals off benefits rather than by promoting exits might thus even induce greater state dependence.

Finally, it remains unclear whether the measured state dependence in benefit receipt indeed reflects the properties of the benefit system as opposed to labour market and income dynamics more broadly. Contini & Negri (2007) for instance highlight that measured state dependence in social assistance might in reality be driven by persistence in unemployment or the detrimental effects of living in poverty. Up to date, there exists unfortunately little empirical evidence on the drivers of state dependence in social assistance, and this study is not well-suited to provide any insights on this question. In the absence of (quasi-)experimental evidence, more sophisticated models of labour market transitions, for instance of the type used by Wunder & Riphahn, might allow to shed further light on this issue.

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# Appendix

Table A.1: specifications with post-Hartz interactions

| | complete sample | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| $y_{t-1}$ | 1.105*** | (0.036) | 1.231*** | (0.045) | 0.863*** | (0.061) |
| $H_t \times y_{t-1}$ | 0.070 | (0.049) | 0.005 | (0.060) | 0.191** | (0.083) |
| *individual characteristics* | | | | | | |
| female | -0.004 | (0.034) | -0.020 | (0.040) | 0.041 | (0.065) |
| age | -0.067*** | (0.016) | -0.074*** | (0.019) | -0.035 | (0.031) |
| age$^2$ | 0.078*** | (0.019) | 0.085*** | (0.022) | 0.044 | (0.037) |
| years of education | -0.236*** | (0.048) | -0.192*** | (0.053) | -0.613*** | (0.144) |
| years of education$^2$ | 0.005*** | (0.002) | 0.004* | (0.002) | 0.018*** | (0.005) |
| good health | -0.038 | (0.038) | 0.003 | (0.045) | -0.122* | (0.068) |
| poor health | 0.112** | (0.047) | 0.140** | (0.056) | 0.058 | (0.086) |
| migrant | 0.278*** | (0.055) | 0.264*** | (0.057) | 0.471** | (0.222) |
| *household characteristics* | | | | | | |
| single, with children | 0.042 | (0.095) | 0.139 | (0.122) | -0.025 | (0.153) |
| couple, no children | 0.187* | (0.102) | 0.302** | (0.123) | 0.016 | (0.186) |
| couple, with children | 0.001 | (0.106) | 0.021 | (0.130) | 0.059 | (0.190) |
| child $\leq$ 6 years | 0.035 | (0.052) | 0.127** | (0.061) | -0.180* | (0.098) |
| household size | 0.057* | (0.030) | 0.069** | (0.035) | 0.013 | (0.059) |
| *partner characteristics* | | | | | | |
| age | -0.019*** | (0.005) | -0.018*** | (0.006) | -0.024** | (0.011) |
| age$^2$ | 0.026*** | (0.009) | 0.026*** | (0.010) | 0.032* | (0.017) |
| years of education | 0.057*** | (0.015) | 0.040** | (0.018) | 0.111*** | (0.033) |
| years of education$^2$ | -0.004*** | (0.001) | -0.004*** | (0.001) | -0.007*** | (0.002) |
| good health | 0.013 | (0.041) | 0.030 | (0.050) | -0.036 | (0.074) |
| poor health | 0.208*** | (0.053) | 0.235*** | (0.064) | 0.158* | (0.095) |
| migrant | 0.186*** | (0.058) | 0.198*** | (0.060) | 0.602** | (0.239) |
| *Wooldridge controls* | | | | | | |
| $y_0$ | 1.310*** | (0.056) | 1.261*** | (0.069) | 1.374*** | (0.098) |
| avg: good health | -0.153** | (0.066) | -0.171** | (0.081) | -0.113 | (0.120) |
| avg: poor health | 0.269*** | (0.084) | 0.291*** | (0.099) | 0.174 | (0.160) |
| avg: single, with children | 0.421*** | (0.125) | 0.315** | (0.155) | 0.465** | (0.223) |
| avg: couple, no children | -0.481*** | (0.117) | -0.680*** | (0.144) | -0.280 | (0.212) |
| avg: couple, with children | -0.235* | (0.130) | -0.389** | (0.157) | -0.088 | (0.243) |
| avg: child $\leq$ 6 years | 0.352*** | (0.080) | 0.188** | (0.094) | 0.872*** | (0.161) |
| avg: household size | 0.065* | (0.035) | 0.069* | (0.040) | 0.053 | (0.073) |
| avg: reg. unemployment rate | 0.017 | (0.012) | 0.014 | (0.018) | -0.015 | (0.021) |
| avg: good health (partner) | -0.144** | (0.070) | -0.050 | (0.085) | -0.246** | (0.125) |
| avg: poor health (partner) | 0.145 | (0.093) | 0.211* | (0.110) | 0.050 | (0.179) |
| *individual characteristics (post-Hartz)* | | | | | | |
| female | -0.003 | (0.045) | 0.016 | (0.054) | -0.020 | (0.082) |
| age | -0.025 | (0.024) | -0.023 | (0.029) | -0.026 | (0.047) |
| age$^2$ | 0.023 | (0.029) | 0.022 | (0.034) | 0.027 | (0.055) |
| years of education | -0.202*** | (0.071) | -0.221*** | (0.078) | -0.310 | (0.198) |
| years of education$^2$ | 0.006** | (0.003) | 0.008** | (0.003) | 0.009 | (0.007) |
| good health | -0.060 | (0.058) | -0.180** | (0.070) | 0.196* | (0.107) |
| poor health | -0.071 | (0.073) | -0.087 | (0.086) | -0.059 | (0.142) |

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Table A.1 – continued from previous page –

| | complete sample | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| migrant | -0.022 | (0.070) | 0.002 | (0.072) | -0.493 | (0.301) |
| *household characteristics (post-Hartz)* | | | | | | |
| single, with children | -0.047 | (0.143) | -0.095 | (0.183) | -0.027 | (0.236) |
| couple, no children | -0.263 | (0.161) | -0.445** | (0.197) | 0.136 | (0.287) |
| couple, with children | -0.219 | (0.163) | -0.208 | (0.202) | -0.267 | (0.285) |
| child $\leq$ 6 years | 0.075 | (0.089) | 0.051 | (0.103) | 0.114 | (0.180) |
| household size | 0.042 | (0.049) | -0.047 | (0.059) | 0.260*** | (0.093) |
| *partner characteristics (post-Hartz)* | | | | | | |
| age | 0.000 | (0.007) | 0.003 | (0.009) | -0.005 | (0.015) |
| age$^2$ | 0.000 | (0.012) | -0.008 | (0.014) | 0.017 | (0.024) |
| years of education | -0.004 | (0.022) | 0.017 | (0.025) | -0.064 | (0.044) |
| years of education$^2$ | -0.001 | (0.001) | -0.002 | (0.001) | 0.002 | (0.002) |
| good health | -0.039 | (0.066) | -0.089 | (0.079) | 0.079 | (0.120) |
| poor health | -0.136 | (0.086) | -0.142 | (0.101) | -0.155 | (0.163) |
| migrant | -0.064 | (0.078) | -0.101 | (0.082) | -0.220 | (0.331) |
| *Wooldridge controls (post-Hartz)* | | | | | | |
| $y_0$ | -0.078 | (0.055) | -0.083 | (0.069) | -0.089 | (0.095) |
| avg: good health | 0.127 | (0.098) | 0.224* | (0.120) | -0.066 | (0.174) |
| avg: poor health | 0.095 | (0.128) | 0.099 | (0.151) | 0.053 | (0.245) |
| avg: single, with children | -0.216 | (0.176) | -0.147 | (0.220) | -0.010 | (0.314) |
| avg: couple, no children | 0.242 | (0.182) | 0.505** | (0.222) | -0.210 | (0.330) |
| avg: couple, with children | -0.043 | (0.190) | 0.079 | (0.231) | -0.014 | (0.349) |
| avg: child $\leq$ 6 years | -0.041 | (0.126) | 0.081 | (0.147) | -0.413 | (0.257) |
| avg: household size | -0.058 | (0.055) | 0.038 | (0.064) | -0.356*** | (0.110) |
| avg: reg. unemployment rate | 0.009 | (0.017) | 0.015 | (0.024) | 0.001 | (0.028) |
| avg: good health (partner) | 0.187* | (0.104) | 0.089 | (0.127) | 0.326* | (0.186) |
| avg: poor health (partner) | 0.174 | (0.145) | 0.103 | (0.171) | 0.182 | (0.281) |
| reg. unemployment rate | 0.041*** | (0.011) | 0.051*** | (0.017) | 0.035** | (0.015) |
| $H_t\times$ reg. unemployment rate | -0.003 | (0.014) | -0.016 | (0.022) | 0.017 | (0.019) |
| Eastern Germany | 0.267*** | (0.069) | | | | |
| $H_t\times$ Eastern Germany | -0.068 | (0.089) | | | | |
| constant | 0.111 | (0.453) | -0.045 | (0.517) | 2.930** | (1.168) |
| $H_t\times$ constant | 2.324*** | (0.687) | 2.274*** | (0.797) | 3.252** | (1.610) |
| $\sigma_a$ | 0.839*** | (0.026) | 0.784*** | (0.030) | 0.953*** | (0.048) |
| $\rho$ | 0.413*** | (0.015) | 0.381*** | (0.018) | 0.476*** | (0.025) |
| log Likelihood | -14,528.497 | | -9,664.339 | | -4,780.015 | |
| # of observations | 100,434 | | 79,790 | | 20,644 | |
| # of individuals | 17,733 | | 14,010 | | 3,723 | |

* p<0.10, ** p<0.05, *** p<0.01

*Note:* Standard errors are given in parentheses. $y_{t-1}$ and $y_0$ are the observed social assistance receipt status in the last period and the initial observed period, respectively, $H_t$ is a dummy variable for the post-Hartz years 2005-2011. All other covariates are lagged by one period. Blocks of variables labelled '(post-Hartz)' have been interacted with the post-Hartz dummy. Among the 'Wooldridge controls', I use the prefix 'avg:' to refer to an individual's time-average for this variable. Values of age$^2$ have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011

Table A.2: specifications with survey-year interactions

| | complete sample | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| $y_{t-1}$ | 1.199*** | (0.095) | 1.359*** | (0.119) | 0.932*** | (0.163) |
| *calendar-year interactions* | | | | | | |
| $y_{t-1}\times1997$ | -0.377*** | (0.127) | -0.254 | (0.158) | -0.522** | (0.228) |
| $y_{t-1}\times1998$ | -0.177 | (0.132) | -0.246 | (0.159) | -0.181 | (0.249) |
| $y_{t-1}\times1999$ | -0.181 | (0.126) | -0.119 | (0.155) | -0.425* | (0.222) |
| $y_{t-1}\times2000$ | -0.114 | (0.132) | 0.050 | (0.164) | -0.495** | (0.230) |
| $y_{t-1}\times2001$ | -0.022 | (0.118) | -0.107 | (0.147) | 0.100 | (0.205) |
| $y_{t-1}\times2002$ | 0.156 | (0.124) | 0.077 | (0.154) | 0.220 | (0.212) |
| $y_{t-1}\times2003$ | -0.025 | (0.125) | -0.103 | (0.156) | 0.044 | (0.211) |
| $y_{t-1}\times2004$ | -0.001 | (0.122) | -0.220 | (0.152) | 0.316 | (0.212) |
| $y_{t-1}\times2005$ | -0.329*** | (0.122) | -0.333** | (0.153) | -0.349* | (0.207) |
| $y_{t-1}\times2006$ | 0.000 | (0.124) | -0.023 | (0.155) | -0.013 | (0.212) |
| $y_{t-1}\times2007$ | 0.081 | (0.121) | -0.038 | (0.151) | 0.320 | (0.212) |
| $y_{t-1}\times2008$ | 0.133 | (0.126) | -0.027 | (0.156) | 0.351 | (0.221) |
| $y_{t-1}\times2009$ | 0.063 | (0.131) | -0.135 | (0.165) | 0.431* | (0.224) |
| $y_{t-1}\times2010$ | 0.321** | (0.139) | 0.153 | (0.174) | 0.542** | (0.237) |
| $y_{t-1}\times2011$ | 0.028 | (0.143) | -0.254 | (0.179) | 0.593** | (0.254) |
| *individual characteristics* | | | | | | |
| female | -0.004 | (0.029) | -0.010 | (0.033) | 0.023 | (0.056) |
| age | -0.079*** | (0.013) | -0.078*** | (0.015) | -0.076*** | (0.024) |
| age$^2$ | 0.088*** | (0.015) | 0.089*** | (0.017) | 0.085*** | (0.029) |
| years of education | -0.289*** | (0.041) | -0.252*** | (0.045) | -0.696*** | (0.126) |
| years of education$^2$ | 0.007*** | (0.002) | 0.006*** | (0.002) | 0.020*** | (0.005) |
| good health | -0.058** | (0.028) | -0.069** | (0.034) | -0.020 | (0.051) |
| poor health | 0.076** | (0.035) | 0.095** | (0.041) | 0.022 | (0.067) |
| migrant | 0.266*** | (0.045) | 0.263*** | (0.046) | 0.268 | (0.184) |
| *household characteristics* | | | | | | |
| single, with children | 0.030 | (0.064) | 0.098 | (0.082) | -0.046 | (0.105) |
| couple, no children | 0.072 | (0.074) | 0.100 | (0.090) | 0.056 | (0.133) |
| couple, with children | -0.073 | (0.075) | -0.061 | (0.092) | -0.034 | (0.132) |
| child $\leq$ 6 years | 0.101*** | (0.039) | 0.166*** | (0.046) | -0.062 | (0.077) |
| household size | 0.063*** | (0.022) | 0.051* | (0.026) | 0.102** | (0.043) |
| *partner characteristics* | | | | | | |
| age | -0.018*** | (0.004) | -0.016*** | (0.005) | -0.021*** | (0.008) |
| age$^2$ | 0.023*** | (0.007) | 0.020*** | (0.008) | 0.027** | (0.013) |
| years of education | 0.056*** | (0.012) | 0.047*** | (0.014) | 0.087*** | (0.024) |
| years of education$^2$ | -0.005*** | (0.001) | -0.004*** | (0.001) | -0.006*** | (0.001) |
| good health | 0.005 | (0.031) | 0.002 | (0.038) | 0.019 | (0.057) |
| poor health | 0.152*** | (0.041) | 0.174*** | (0.048) | 0.100 | (0.076) |
| migrant | 0.164*** | (0.048) | 0.163*** | (0.049) | 0.502** | (0.200) |
| *calendar-year effects* | | | | | | |
| 1997 | -0.011 | (0.064) | 0.120 | (0.074) | -0.321** | (0.129) |
| 1998 | 0.079 | (0.065) | 0.157** | (0.079) | 0.074 | (0.127) |
| 1999 | -0.045 | (0.067) | 0.016 | (0.080) | -0.022 | (0.130) |
| 2000 | -0.043 | (0.067) | -0.122 | (0.084) | 0.177 | (0.124) |
| 2001 | -0.072 | (0.061) | -0.080 | (0.072) | 0.042 | (0.120) |
| 2002 | -0.125** | (0.063) | -0.159** | (0.077) | 0.051 | (0.123) |

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Table A.2 – continued from previous page –

| | complete sample | | Western Germany | | Eastern Germany | |
|---|---|---|---|---|---|---|
| 2003 | 0.107* | (0.060) | 0.094 | (0.072) | 0.268** | (0.121) |
| 2004 | 0.080 | (0.063) | 0.109 | (0.074) | 0.217* | (0.131) |
| 2005 | 0.221*** | (0.063) | 0.249*** | (0.073) | 0.360*** | (0.130) |
| 2006 | 0.191*** | (0.067) | 0.203** | (0.083) | 0.417*** | (0.132) |
| 2007 | -0.048 | (0.069) | 0.049 | (0.082) | -0.142 | (0.139) |
| 2008 | 0.010 | (0.068) | 0.018 | (0.082) | 0.023 | (0.128) |
| 2009 | 0.130* | (0.070) | 0.196** | (0.082) | -0.132 | (0.137) |
| 2010 | 0.070 | (0.074) | 0.050 | (0.089) | 0.045 | (0.139) |
| 2011 | 0.085 | (0.079) | 0.176* | (0.090) | -0.340** | (0.172) |
| *Wooldridge controls* | | | | | | |
| $y_0$ | 1.256*** | (0.048) | 1.221*** | (0.059) | 1.307*** | (0.085) |
| avg: good health | -0.111** | (0.055) | -0.084 | (0.066) | -0.190* | (0.102) |
| avg: poor health | 0.309*** | (0.071) | 0.334*** | (0.083) | 0.230 | (0.141) |
| avg: single, with children | 0.299*** | (0.093) | 0.250** | (0.114) | 0.357** | (0.173) |
| avg: couple, no children | -0.370*** | (0.089) | -0.452*** | (0.108) | -0.295* | (0.166) |
| avg: couple, with children | -0.265*** | (0.097) | -0.355*** | (0.116) | -0.129 | (0.186) |
| avg: child $\leq$ 6 years | 0.256*** | (0.064) | 0.191*** | (0.074) | 0.520*** | (0.132) |
| avg: household size | 0.059** | (0.028) | 0.084*** | (0.032) | -0.018 | (0.058) |
| avg: good health (partner) | -0.099* | (0.057) | -0.026 | (0.069) | -0.221** | (0.106) |
| avg: poor health (partner) | 0.205*** | (0.079) | 0.260*** | (0.092) | 0.116 | (0.159) |
| avg: reg. unemployment rate | 0.022** | (0.010) | 0.042** | (0.018) | 0.004 | (0.017) |
| reg. unemployment rate | 0.039*** | (0.010) | 0.021 | (0.017) | 0.007 | (0.016) |
| Eastern Germany | 0.242*** | (0.056) | | | | |
| constant | 0.890** | (0.374) | 0.538 | (0.425) | 4.834*** | (0.987) |
| $\sigma_a$ | 0.823*** | (0.026) | 0.778*** | (0.031) | 0.934*** | (0.048) |
| $\rho$ | 0.404*** | (0.015) | 0.377*** | (0.019) | 0.466*** | (0.026) |
| log Likelihood | -14,507.179 | | -9,659.002 | | -4,737.816 | |
| # of observations | 100,434 | | 79,790 | | 20,644 | |
| # of individuals | 17,733 | | 14,010 | | 3,723 | |

* p<0.10, ** p<0.05, *** p<0.01
*Note:* Standard errors are given in parentheses. $y_{t-1}$ and $y_0$ are the observed social assistance receipt status in the last period, and the initial observed period, respectively. All other covariates are lagged by one period. Among the 'Wooldridge controls', I use the prefix 'avg:' to refer to an individual's time-average for this variable. Values of age$^2$ have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011