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

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ABSTRACT

In this paper, we study the dynamics of social assistance benefit receipt in Germany using annual survey data from the German Socio-Economic Panel (SOEP) for the years 1995-2011. Rates of benefit receipt were stable in Germany at around 8% in the 1990s but started rising in 2001 to peak at over 12% in 2006. We show that this increase in the receipt rate can be attributed to lower exit rates from benefit receipt since 2001.

In the econometric part of the paper, we study state dependence in social assistance benefit receipt, i.e. the question to what extent benefit receipt today predicts the probability of future benefit receipt. We estimate a series of dynamic random-effects probit models that control for unobserved heterogeneity and endogeneity of the initial conditions and indeed find considerable evidence of state dependence. Our estimates suggest that benefit receipt one year ago raises the likelihood of benefit receipt today by a factor of 3, which corresponds to an average partial effect of about 14 percentage points. The level of state dependence differs between subsamples and is larger in absolute terms for women, Eastern German residents, and migrants, for whom receipt rates are higher.

Studying variations over time, we find a rise in the level of state dependence after the 2005 Hartz reforms in Eastern Germany. We attribute this effect to a drop in average predicted entry rates into benefit receipt without a corresponding fall in predicted benefit persistence rates. We do not identify any comparable change in structural dependence for Germany as a whole or other subsamples we look at. Since a reform that contributes to keeping individuals off benefits while doing little to raise exit rates would increase state dependence but might nonetheless be considered as beneficial, our findings should not be understood as a verdict on the success or failure of the Hartz reforms.

RESUME

Cet article étudie la dynamique des prestations d'assistance sociale reçues en Allemagne, sur la période 1995-2011, à partir des données annuelles du Panel socio-économique allemand (SOEP). Dans les années 90, le taux des prestations d'assistance sociale en Allemagne est resté stable autour de 8 % puis a commencé à augmenter à partir de 2001 pour dépasser 12% en 2006. Cette augmentation s'explique principalement par des taux de sortie plus faible du système de prestations à partir de 2001.

La partie de l'article consacrée à l'analyse économétrique étudie la probabilité qu'une personne bénéficie dans l'avenir de prestations en fonction de recevoir des prestations aujourd'hui. Une série de modèles probit dynamique à effets aléatoires est utilisée en contrôlant l'hétérogénéité non observée et l'endogénéité des conditions initiales. Les résultats étayent l'hypothèse d'une dépendance entre les états . Les estimations suggèrent que le fait d'avoir reçu une prestation sociale un an auparavant augmente par un facteur de 3 la probabilité de bénéficier d'une prestation un an après, ce qui correspond en moyenne à un effet partiel de 14 points de pourcentage. Le niveau de dépendance varie selon les différents sous-échantillons : en termes absolus, la dépendance est plus importante chez les femmes, les résidents de l'Allemagne de l'Est et les immigrants, pour lesquels le taux de réception des prestations est plus élevé.

L'analyse temporelle des variations de la dépendance entre les états montre une augmentation de celle-ci en Allemagne de l'Est après les réformes Hartz de 2005. Ce phénomène peut être attribué à une diminution du taux d'entrée prédit par le modèle, sans qu'elle s'accompagne d'une diminution des taux attendus de persistance des prestations. On ne trouve aucun changement comparable de la structure de la dépendance pour l'ensemble de l'Allemagne ou d'autres sous-échantillons. En principe, toute réforme qui rend moins probable la réception de prestations sociales tout en ne favorisant pas la sortie du système fait augmenter le taux de dépendance mais pourrait cependant être considérée comme bénéfique. Dans ce sens, nos résultats ne sont pas à interpréter comme un jugement positif ou négatif sur les réformes Hartz.

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1. Introduction

1. A good understanding of the driving forces of social assistance benefit dynamics is important for the design of well-functioning social-safety nets. Academic work on the dynamics of minimum-income benefit receipt has since its beginnings been mostly concerned with evaluating the frequency of long-term receipt, and with explaining the drivers of high observed persistence in benefit receipt (for early contributions, see Boskin & Nold (1975), Bane & Ellwood (1983, 1994), and Blank (1989)). While existing research suggests that the majority of benefit spells tends to be relatively short, a standard finding is that benefit receipt today is a very strong predictor of the probability of future benefit receipt.

2. Two potential explanations for this 'state dependence' in benefit receipt have been proposed. Social assistance benefit recipients typically differ from non-recipients in terms of their personal and socio-economic characteristics, both observable and unobservable. As these characteristics are likely to affect the probability of benefit receipt, differences between entry rates and the high observed rates of persistence in benefit receipt are certainly driven at least in part by differences in characteristics between recipients and non-recipients. Yet, high persistence rates might also hint at a potential pervasive effect of social assistance. Benefit receipt might lead to a 'culture of dependence' as recipients may become accustomed to receiving transfer payments. Individuals who have been receiving social assistance benefits for a while may also start to feel less able or motivated to escape from social assistance, which might imply that activation measures like job-search requirements or assistance become less effective. Finally, employers could interpret benefit receipt as a negative signal about a recipient's unobserved labour productivity, which would reduce employment prospects. In all of these cases, current transfer receipt has a *causal* effect on the probability of future transfer receipt by raising hurdles to self-sufficiency thus creating a 'social assistance trap'.

3. The implications for policy design of those two scenarios are very different. If benefit receipt *as such* increases the probability of future benefit receipt, policies that prevent entry or facilitate exit from social assistance can induce a lasting reduction in benefit dependency. If, by contrast, high benefit persistence is a consequence of the recipients' individual characteristics, policies that encourage exits from benefits are likely to have little impact unless barriers to self-sufficiency are addressed directly. In terms of the concepts introduced by Heckman (1981a; 1981b), it is thus of great relevance for policy-design to distinguish 'true' or 'structural' state dependence from the part that is 'spurious', i.e. the result of unobserved individual heterogeneity.

4. In this study, we analyse the dynamics of social assistance benefit receipt in Germany using annual data from the German Socio-Economic Panel (SOEP) for the years 1995 to 2011. This period is of particular interest due to the far-reaching 'Hartz reforms' that were implemented in the years 2003 to 2005 and that fundamentally changed the system of social assistance benefit provision in Germany. In a first, descriptive part, this paper analyses rates of benefit receipt and the underlying benefit transition rates for different subsamples. To study state dependence, we then present in a second part the results from a series of dynamic random-effects probit models that allow us to capture persistent unobserved heterogeneity. We control for the endogeneity of initial conditions using the method proposed by Wooldridge (2005). The degree of estimated state dependence is compared across different subsamples and over time. In contrast to

earlier studies, we use a relatively broad definition of social assistance looking beyond receipt of Social Assistance[1] and Unemployment Benefit II to Unemployment Assistance and Housing Benefits.

5. The descriptive part of the paper shows that while the rate of social assistance benefit receipt in Germany was relatively stable at around 8% in the late 1990s, there was a strong rise in the receipt rate from 2001 leading to a peak of around 12.5% in 2006. We find that this increase in the frequency of benefit receipt over time was primarily due to a drop in the exit rate from benefit receipt in 2001. By contrast, differences in the rates of benefit receipt across different groups – notably the higher rates of benefit receipt in Eastern Germany and among migrants – are primarily due to differences in entry rates.

6. Our econometric analysis shows that even though individual characteristics explain the largest share of observed state dependence in social assistance benefit receipt, there is evidence of a substantial structural component in all subsamples we look at. Based on our model estimates, we calculate that an individual is predicted to be 3.3 times more likely to be in receipt of social assistance benefits if she already received benefits at the time of the last interview. This corresponds to an average partial effect of past benefit receipt on receipt in the current period of over 14 percentage points. This effect is comparable in size to the one obtained in earlier studies for other countries, for instance by Cappellari & Jenkins (2008) in their analysis for Britain. Splitting up our data into different subsamples, we find that the level of state dependence varies by recipient group being larger in absolute terms for females, Eastern Germans, and immigrants.

7. Studying variations in the degree of state dependence over time, we find a significant rise in structural state dependence for Eastern Germany in 2006, the year after the Hartz reforms. We attribute this effect to a drop in average predicted entry rates into benefit receipt without a corresponding fall in predicted benefit persistence rates. We do not find a comparable effect for Western Germany or for any of the other subsamples we look at. Across the entire sample, there is no significant variation in state dependence over the observation period. This finding is robust to dropping Housing Benefits from the definition of our social assistance benefit variable.

8. The remainder of this paper is structured as follows: Section 2 provides a short introduction to the literature on state dependence and social assistance benefit dynamics in Germany. In Section 3, we give an overview of the relevant institutional background in Germany during the observation period, before defining the social assistance benefit variable in Section 4. Section 5 provides information about the data used for our analysis. In Section 6, we describe trends in the benefit receipt and transition rates over time. Sections 7 and 8 finally discuss the econometric approach and provide results from our estimations. Section 9 concludes.

2. Relevant previous work

9. The decomposition of observed 'raw' state dependence – i.e. the difference between benefit persistence and entry rates – into its structural and spurious components has been at the centre of much of the

[1] Throughout this paper, we distinguish between the concept 'social assistance' (non-capitalized) and the German benefit programme 'Social Assistance' (*Sozialhilfe*, in capital letters). For a discussion of the German income-support system see Section 3.

recent work on social assistance benefit dynamics. While early studies often only inadequately control for unobserved heterogeneity and endogenous initial conditions, dynamic panel data methodology and the approaches developed by Heckman (1981b) and Wooldridge (2005) offer ways of addressing these issues. Still, the number of econometric studies on the dynamics of social assistance benefit receipt is relatively small and existing work is limited to a narrow set of countries. Hansen & Lofstrom (2006; 2008) and Andrén & Andrén (2013) analyse the native-immigrant gap in social assistance receipt in Sweden. They find considerable evidence for structural state dependence especially for migrants and confirm that unobserved heterogeneity plays an important role in explaining differences in benefit receipt between natives and migrants. Mood (2013) finds evidence for weak duration dependence in Social Assistance benefit receipt in Sweden using a hazard rate framework.

10. Hansen, Lofstrom, & Zhang (2006) compare social assistance benefit dynamics across different provinces in Canada. They also find strong evidence for structural state dependence, the degree of which however varies substantially between provinces. They argue that differences in benefit generosity across provinces could be responsible for the variance in state dependence, with higher benefit levels inducing stronger structural state dependence. A few further studies restrict their analysis to smaller target populations: Gong (2004) studies the dynamics of minimum-income benefit receipt in Australia for low-income females. Similarly, Chay & Hyslop (2000) analyse the dynamics of AFDC[2] receipt for females in the United States. Chay, Hoynes, & Hyslop (1999) study the dynamics of AFDC receipt in the state of California.

11. In an earlier study commissioned by the OECD, Cappellari & Jenkins (2008) analyse the dynamics of social assistance benefit receipt in Britain using data from the British Household Panel Survey (BHPS) and discuss the principal methodological issues that arise in such an analysis. They find substantial evidence for state dependence in social assistance benefit receipt, the degree of which is heterogeneous across groups of individuals. State dependence in benefit receipt is found to be higher for lone parents, and it is also stronger for individuals with one non-interrupted spell of benefit receipt compared to those with a spell of self-sufficiency in between interview dates. The strong decline in social assistance caseloads in Britain from 1991 to 2005 is attributed to a fall in entry rates rather than an increase in exit rates. In a follow-up paper, Cappellari & Jenkins (2009) further suggest that improvements in the economic environment measured by a decline in unemployment rates as well as institutional reforms – particularly the introductions of the Jobseeker's Allowance in 1996 and the Working Families Tax Credit in 1999 – might have contributed to the fall in receipt rates.

12. Few econometric studies exist that examine the dynamics of social assistance benefit receipt in Germany. Voges & Rohwer (1992) use a logit model based on pooled data from the first six waves of the SOEP. While their model attempts to capture state dependence by controlling for lagged receipt status they ignore any endogeneities that arise from unobserved heterogeneity. A series of studies look at the immigrant-native gap in benefit receipt, yet without focusing on the issue of state dependence: Riphahn (1999) uses a panel data approach to account for unobserved heterogeneity and explicitly addresses panel attrition and endogenous labour force status. Riphahn & Wunder (2012) estimate a semi-parametric model for social assistance receipt over the life-cycle of first-generation migrants and natives before and after the Hartz

[2] Assistance to Families with Dependent Children (AFDC) is the precursor of Temporary Assistance for Needy Families (TANF), which was introduced through the Personal Responsibility and Work Opportunity Act (PRWORA) in the U.S. in 1996.

reforms. Riphahn, Sander & Wunder (2013) study correlates of social assistance benefit receipt of Turkish immigrants and German natives using SOEP data for the years 2003-2007. All three studies are again based on data from the SOEP. Aldashev & Fitzenberger (2009) and Schels (2013) exclusively focus on entries and exits to Unemployment Benefit II (UBII), respectively and restrict their analysis to very short observation periods: Aldashev & Fitzenberger study a cohort of receipints of Unemployment Benefit II in the third quarter of 2006 and examine their entry dynamics over the years 2000 to 2005. Schels looks at exits from social assistance receipt for a cohort of young benefit recipients in January 2005 over the period from until 2007

13. In a study which is closest to ours in terms of methodology, Wunder & Riphahn (2013) evaluate state dependence in social assistance in Germany for natives and immigrants using a dynamic multinomial logit model. The selection of states modelled includes receipt of UBII, part- or full-time employment, and 'inactivity', which is defined as including receipt of unemployment insurance benefits. Wunder & Riphahn find that persistence in social assistance benefit receipt can mostly be accounted for by observable characteristics, with only limited evidence for 'structural' state dependence. Since exits from social assistance to work are more frequent than exits from 'inactivity' to work, they conclude that there is no evidence for insufficient incentives for exits from social assistance. Similarly, they find no notable difference in entry rates from 'inactivity' to social assistance compared to persistence rates in social assistance, a result which they interpret as a lack of evidence for a social assistance trap. They argue that as past social assistance recipients are no more likely than 'inactive' individuals to receive social assistance benefits in the current period, there is no evidence for a causal effect of past social assistance receipt on receipt in the current period beyond the potential state dependence induced by being out of work. However, Wunder & Riphahn focus on receipt of UBII in Western Germany only and thus restrict their analysis to the years 2005-2009.

3. Institutional framework

14. During our 1995-2011 observation period, the German social assistance system underwent far-reaching reforms. In particular, 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 last-resort minimum-income benefits. This section briefly describes some key features of the benefit system in the years before and after these reforms. The description serves as a basis for our definition of the term social assistance in the following section.[4]

15. Until 2005, the German income support system for working-age individuals had a three-tier structure. As the top layer, Unemployment Insurance benefits (UB*, 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. The maximum duration of benefit entitlements was 12 to 32 months depending on age and the previous contribution history and with the relevant thresholds changing over our 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.

16. Individuals whose entitlements to UB had expired could claim Unemployment Assistance (UA*, Arbeitslosenhilfe*) benefits.[5] UA was earnings-related but means-tested on family-income. With replacement rates of 53% (57% for individuals with children) it was less generous than UB. As a result of the lower benefit amounts, claimants of UA benefits often qualified for additional top-up payments from Social Assistance (SA, *Sozialhilfe*). Other than for Unemployment Insurance, 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.

17. Introduced in 1962 through the Federal Social Assistance Act (*Bundessozialhilfegesetz*), Social Assistance was designed as a benefit of last resort below this primary social safety net. It was understood as a temporary emergency benefit based on the principle of 'helping people help themselves' ("*Hilfe zur Selbsthilfe*"). Eligibility required individuals to have exhausted all alternative sources of income in the form of earnings from work, Unemployment Insurance or Assistance benefit payments, and financial support from direct family members. While SA had initially been primarily targeted at individuals with special

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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 For a more detailed discussion of the German minimum-income benefit system at the beginning of our observation period and shortly before the Hartz reforms, see Eardley, Bradshaw, Ditch, Gough, & Whiteford (1996) and Adema, Gray, & Kahl (2003), respectively. Jacobi & Kluve (2006), Eichhorst, Grienberger-Zingerle, & Konle-Seidl (2008) and Caliendo (2009) discuss the key changes introduced through the Hartz reforms. A recent comparison of the minimum-income benefit systems operated in OECD is given by Immervoll (2010).

5 Until the end of the year 1999, individuals could claim UA without having previously received UB under the condition that they had worked 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 UB.

needs and limited employability, a gradual tightening of eligibility criteria for UB and UA over time meant that a growing proportion of individuals were shifted into Social Assistance.[6]

18. Social Assistance consisted of two main components. The 'livelihood assistance' (*Hilfe zum Lebensunterhalt*) was available for an unlimited period of time and aimed at covering all basic household expenditures, including food and clothing. Separate applications for assistance could moreover be filed for the purchase of more expensive items of clothing or durable goods like furniture or electrical appliances. A housing allowance was paid to cover the costs of accommodation including water and heating and possibly interest payments for mortgages for owner-occupiers. Next to this livelihood assistance, 'assistance for individuals in special circumstances' (*Hilfe in besonderen Lebenslagen*) was available to cover expenses arising from special needs for instance due to major health problems or disabilities.

19. The fourth package of the Hartz reforms, which entered into force in January 2005, abolished this three-tier system with the aim of strengthening the labour market services and activating principles for unemployed jobseekers. The contribution-based Unemployment Benefit was replaced by the new Unemployment Benefit I (UBI, *Arbeitslosengeld I*) with an unchanged replacement rate of 60% (67% for individuals with children). The maximum period of benefit entitlements remained at 12 to 32 month depending on age until one year after reforms. In 2006, the maximum period of benefit payments for older individuals was lowered to 18 months, however raised again to 24 months in 2008.

20. In the context of the present study, the most relevant change was the merger of Unemployment Assistance and Social Assistance for *employable* jobseekers into a 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 Social Assistance. Compared to the old UA scheme, the new UBII is typically less generous and no longer depends on the level of previous earnings. Social Assistance continues to exist as a separate benefit scheme but is now restricted to individuals *incapable of work* due to sickness, disability, or care duties. The Hartz reforms thus introduced a clearer distinction between the different social assistance benefit schemes for employable and non-employable individuals.

21. Both before and after the crisis, 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 or recipients of UB or UA benefits. Social Assistance recipients were not entitled to claim HB. Since 2005, UBII and Social Assistance recipients receive support for eligible housing expenses as part of their social assistance entitlements, while a separate Housing Benefit continues to be available for other low-income groups.

4. Definition of the social assistance variable

22. In light of the profound institutional changes in the German income-support system just described, an analysis of the last-resort Social Assistance benefit alone would clearly not allow us to consistently study the dynamics of benefit receipt over a longer time horizon. Indeed, while the few existing studies on social assistance dynamics in Germany typically look at the receipt of Social Assistance benefits or Unemployment Benefit II only, they focus on relatively short time periods either before or after the Hartz reforms. Since in this study, however, we are interested in comparing the benefit dynamics before and after

[6] For an overview of the early history of Social Assistance in Germany, see Seeleib-Kaiser (1995).

the reforms, a relatively broad definition of the social assistance variable is needed to allow for a consistent analysis.

23. In their study of social assistance dynamics in Britain, Cappellari & Jenkins (2008) characterize the term 'social assistance' by contrasting the purpose of income maintenance with that of income replacement. Based on this distinction, they define a benefit programme as a social assistance scheme if it does not require previous social security contributions and if it is means- or income-tested. Moreover, they exclude in-work benefits in the form of tax credits from their analysis arguing that these schemes are explicitly designed to facilitate transitions from social assistance to work rather than being social assistance schemes themselves.

24. In this analysis, we follow Cappellari & Jenkins using the term social assistance to refer to benefit schemes that are resource-tested, non-contributory, and non-conditional on current employment. This broad definition enables us to jointly study benefit dynamics in Germany before and after the Hartz reforms. It will moreover facilitate a meaningful comparison of our results with those obtained by Cappellari & Jenkins for Britain.

25. Based on the discussion in the previous section, Table 1 gives a summary of the principal benefit schemes for working-age individuals in Germany before and after the Hartz reforms that can be categorised as social assistance. Before the Hartz reforms, both Social Assistance and Housing Benefits share the characteristics of being means-tested, non-contributory, and non-conditional on current employment. After 2005, the same holds true for the post-reform Social Assistance, now targeted at individuals incapable of working, as well as for Unemployment Benefit II, and again Housing Benefits.

Table 1. main eligibility conditions of the principal social assistance benefit schemes for working-age individuals

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

26. As an earnings-related benefit scheme, Unemployment Assistance scheme should arguably be dropped from the table and our definition of social assistance. The reason why we include it nonetheless is that as the direct precursor to Unemployment Benefit II, it shares a number of key features of the other

listed social assistance programmes. Other than Unemployment Insurance benefits, UA was means-tested and could be claimed for an infinite period of time. Also, it was not paid for through social security contributions but was tax-funded. Both of these features make it resemble social assistance benefit schemes like Social Assistance or Unemployment Benefit II.

27. Most importantly, treating UA as a social assistance scheme makes sense when thinking about the implied benefit receipt dynamics. The typical recipient of UA benefits in December 2004 went 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 receipt status for the purpose of our analysis. For these reasons, we include Unemployment Assistance in our analysis. In the remainder of this paper, we therefore use the term 'social assistance' to refer to the benefit schemes listed in Table 1.[7]

28. After having clarified which benefit schemes exactly we are referring to when using the term social assistance, it is worth highlighting the choices must in terms of the unit and time period of analysis. For a more extensive discussion of these issues, see Cappellari & Jenkins (2008).

29. Eligibility for social assistance is usually determined at the level of a larger 'benefit unit', which means that the financial status not only of the claimant alone but of other household members as well will be taken into account for the means test. This might suggest that the household rather than the individual is the most appropriate unit of observation for our analysis. Yet, due to the frequency of changes in household composition, it is not clear what it would mean in practice to analyse the benefit dynamics of a household. Specifically, there is no obvious solution as to how cases of divorce, re-partnering, or the entry to adulthood of a dependent child should be treated in terms of household dynamics.[8]

30. The obvious alternative is to track *individuals* over time, who may or may not be part of a larger benefit unit. In this case, a possible partner's characteristics and the composition of the benefit unit more generally will be important determinants of an individual's social assistance receipt status. For instance, in a traditional single-earner household the husband's characteristics are likely to be the key determinant of the wife's social assistance dynamics. Similarly, the birth of a child into a low-income household can make the difference between self-sufficiency and benefit dependency. Despite this caveat, it seems to be the most sensible approach to treat all individuals in a household as separate units of analysis.

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7 Our approach resembles the one used by Riphahn & Wunder (2012) and Riphahn, Sander, & Wunder (2013), who look at Unemployment Assistance, Unemployment Benefit II, and Social Assistance in their analyses of immigrant-native differences in social assistance receipt before and after the Hartz reform. Both of these studies however do not account for receipt of Housing Benefits.

8 An approach which redefines a household by allocating a new identifier as soon as its composition changes would drastically reduce the average period of analysis for each observation. More importantly, these frequent redefinitions would imply that some of the most interesting benefit transitions could not be analyzed, as we would expect that changes in household composition are important drivers of the dynamics of social assistance receipt.

31. In this paper, we follow the approach used in previous studies and categorise an individual as in receipt of social assistance if benefit receipt is reported for any individual in the household. Partner and household characteristics are added as explanatory variables in the specifications to account for the importance of household composition.[9] 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 administration to assess eligibility for income support payments.

32. Finally, we need to make a decision as to the appropriate period of analysis. Since in the SOEP, recipients are asked to provide information on receipt of income-support payments for each month of the previous calendar year, benefit dynamics could in theory be modelled at the monthly basis. Existing research using other survey-based datasets suggests, however, that the quality of this type of data is often poor. In particular, so-called 'seam bias' arises as respondents have apparent difficulties in answering questions that relate to early parts of the survey period, as for instance reported by Pavetti (1993) for the U.S. National Longitudinal Survey of Youth (NLSY). As a consequence, an untypically large number of benefit transitions seems to occur at the beginning points where survey periods adjoin or overlap. An additional complication for working with monthly data arises from the fact that we only record household composition at the yearly interview date. The benefit receipt status of an individual in any given month of the previous year would therefore have to be based on the household composition as recorded at the following interview.

33. Two different solutions have been used to deal with the problem of imprecise information about the timing of benefit receipt, both of which work by restricting the analysis to one observation per year. A first approach defines a 'social assistance year' by setting the binary social assistance variable equal to one if the respondent reports social assistance receipt for any single month of the year. This approach is often used for annual administrative data, where we might know the amount of benefits received in a given year and possibly the number of months of receipt in that year, but information on the exact timing of payments is lacking (see for instance Andrén & Gustafsson (2004), Hansen & Lofstrom (2006) and Andrén & Andrén (2013)). In their study of social assistance benefit dynamics in Britain, Cappellari & Jenkins (2008) instead focus on the respondent's benefit receipt status at the time of interview only remaining agnostic about any possible periods of receipt between interviews.[10] To allow for a comparison of our results with the ones obtained by Cappellari & Jenkins for Britain, this is also the approach we use in this paper.

34. To sum up, we define an individual as a social assistance benefit recipient in a given period if benefit receipt is reported by any member of the household at the time of the interview. The benefit schemes that are considered 'social assistance' programmes for this purpose are Unemployment Assistance, Social Assistance and Housing Benefits for the years prior to 2005, and Unemployment Benefits II,

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9 An alternative approach is used by Wunder & Riphahn (2013), who define benefit receipt at the individual level. This is possible because they restrict their analysis to the benefit dynamics of Unemployment Benefit II, for which individual-level information is available in the SOEP.

10 They however do use monthly data in one of their specifications to compare the benefit dynamics of individuals whose benefit receipt in two successive years reflects one continuing spell with those that had several shorter spells between interviews.

Social Assistance, and Housing Benefits after 2005. Working-age individuals in the same household are included in the analysis as separate observations.[11]

5. Description of data and sample selection

35. The data used in our analysis come from the German Socio-economic Panel (SOEP), 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. 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 personally interviewed and one of the household members additionally completes a separate household questionnaire. All members of a sampled household are followed up over time even if they leave the original household to form new households. Individuals moving into a sampled household become part of the panel and are followed up in case of split-up of that household from 1989. Household interviews are conducted annually, with the majority of interviews taking place early in the year.[12]

36. The SOEP currently consists of ten separate subsamples labelled A-J and oversamples 'guest workers' and immigrants, German residents of the former German Democratic Republic, and high-income individuals. Individual-level sampling weights for every year are included as part of the dataset to adjust for the differences in selection probabilities and attrition across individuals. We base our analysis on the subsamples A-H[13] and use the supplied cross-sectional sampling weights[14] for each individual and wave to account for differences in sample selection probabilities and attrition.[15] For a detailed description of the dataset and the method used for the construction of the sampling weights, see Haisken-DeNew & Frick (2005).

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11 In our estimations, we assume independence across individuals. Strictly speaking, this assumption is violated if in each period, households are represented by several observations that by construction have the same social assistance receipt status. Like earlier authors, we however ignore any potential inconsistencies induced by this lack of independence.

12 In the years we use for our analysis, just below 80% of interviews have been conducted in the months January to April.

13 "Incentive" sample *I* introduced in 2009 only consists of two waves and no sampling weights are provided for this subsample; "Refreshment" sample *J* was only introduced in 2011 such that only one observation is currently available for each sampled individual.

14 The SOEP also includes longitudinal sampling weights for various subsamples. However, these are constructed for individuals who are part of the respective subsample from the beginning of the observation period, while no allowance is made for respondents that join the sample later. Since our panel is strongly unbalanced, we do not make use of these weights.

15 Cappellari & Jenkins (2008) avoid the problem of different sample-inclusion probabilities by restricting their analysis to the initial sample of the BHPS and excluding subsequent extensions of the panel to Scotland, Wales, and Northern Ireland. When working with data from the SOEP, such an approach is not possible because already individuals from the initial two subsamples *A* and *B* have different sample selection probabilities. Specifically, subsample *B* only targets so-called 'guest workers' and consists of households whose head is Greek, Italian, Spanish, Turkish, or ex-Yugoslav. Subsample *A* covers all remaining households. A combination of those two subsamples hence oversamples 'guest worker' households, while subsample *A* alone excludes these households completely. Oversampling persists in the later refreshment subsamples.

37. The panel used for our analysis consists of the last 17 waves of the SOEP starting with the year 1995, when the question on the receipt of income-support benefits at the time of the interview was first introduced. Information on benefit receipt for the years before 1995 is available only from retrospective questions, which as discussed are less suited for our analysis.[16] The last wave currently available is for the year 2011.

Table 2. sample selection and size

selection criterion	# of individuals		# of observations	
	dropped	**remaining**	**dropped**	**remaining**
complete SOEP for the years 1995 – 2011 (17 waves):		**48,214**		**392,438**
sequentially drop observations…:				
- for individuals aged below 25 and above 59	21,398	26,816	126,235	193,906
- of dependent children	1,493	25,323	10,174	183,732
- with missing information on benefit receipt	114	25,209	764	179,723
- with missing information on key covariates	866	24,343	4,845	174,878
sample used for descriptive analysis:		**24,343**		**174,878**
for estimation, we additionally drop observations...:				
- for individuals with a partner aged below 25 and above 59	1,068	23,275	10,724	164,154
- for individuals in households with a person in full-time education	653	22,622	11,230	152,924
- after a gap in the interview sequence	0	22,622	29,868	123,056
- for individuals observed for only one period	4,889	17,733	4,889	118,167
- that correspond to the first period of an individual's observation sequence	0	17,733	17,733	100,434
sample used for estimation:		**17,733**		**100,434**

Source: SOEP, 1995-2011, subsamples *A-H*

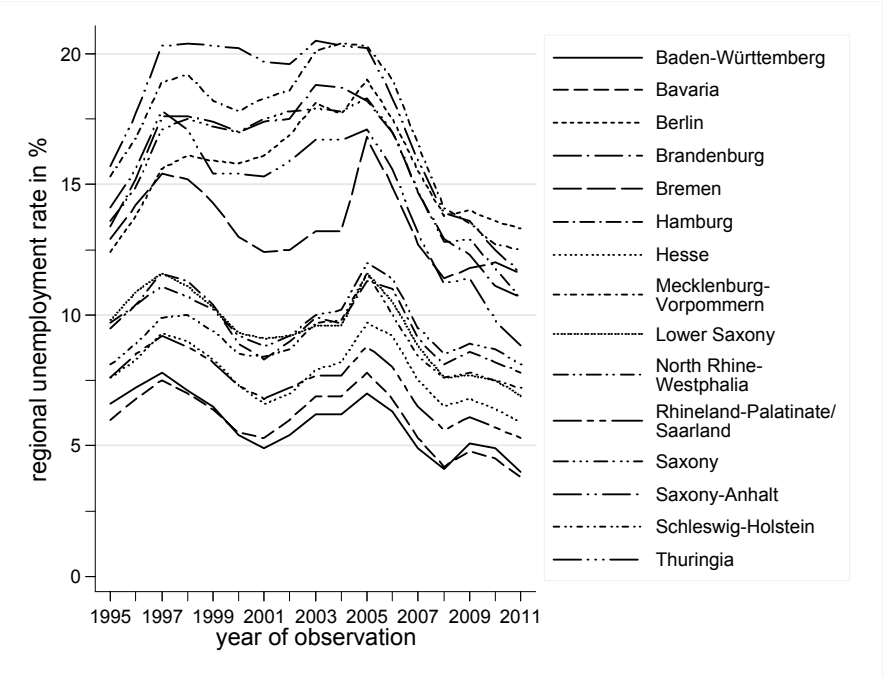
38. For our description of trends in social assistance benefit receipt in the next section and the results of our econometric analysis presented Section 8, we restrict our sample using selection criteria similar to those applied by Cappellari & Jenkins (2008). The resulting changes in the size of the available sample are documented in Table 2. Over the observation period from 1995 to 2011, the full SOEP includes information on over 48,000 individuals, which translates into about 392,000 person-year observations. Because we are interested in working-age individuals only, we drop from this sample all observations for individuals aged below 25 or above 59 years. The lower threshold is meant to help us avoid having to deal with the

[16] Voges & Rohwer (1992) use these data in their analysis of social assistance benefit dynamics.

complications of individuals' entry into the labour market; the upper threshold excludes individuals close to or above the retirement age. We further drop any remaining dependent children, and finally a few observations with missing information on benefit receipt and other key covariates. The resulting sample is the one we use for our descriptive analysis and consists of 24,343 individuals or 174,878 person-year observations.

39. For our econometric estimations, we make a few additional sample restrictions: First, following again Cappellari & Jenkins (2008), we drop observations for individuals with a partner aged below 25 or above 59 years. We also exclude observations for individuals who live in a household where another sample member is in full-time education. Finally, we drop all observations after a gap in an individual's interview sequence, and individuals who are observed for one period only. We also need to drop the initial observation for each individual, for which no suitable lag is available. The resulting estimation sample consists of 17,733 observations and 100,434 person-year observations.

Figure 1. regional unemployment rates



Source: German Federal Statistical Office (Statistisches Bundesamt, 2013). Data for the states of Rhineland-Palatinate and Saarland have been pooled because our version of the SOEP data does not allow for a distinction between those two states for some of the waves.

40. We match our SOEP sample with annual data on unemployment rates in the individuals' state of residence[17] for the years 1995 to 2011 to be able to control for differences in regional labour market conditions in our econometric analysis. These data come from the German Federal Statistical Office (Statistisch-

[17] The variable $BULA, which provides information on the state of residence of an individual in the standard SOEP data set, does not distinguish between the two states of Saarland and the Rhineland-Palatinate for all of the years of our data. We therefore allocate a weighted average of the annual unemployment rates of both federal states to all individuals living in either of these two states.

es Bundesamt, 2013). As summarised in Figure 1, there is a substantial degree of variation in unemployment rates both across states and over time. Looking at the cross-sectional variation, we note that the German states are roughly split into two groups in terms of their unemployment rates. A first group, with unemployment rates of around 5-10%, consists of eight Western German states with the lowest rates observed for the two southern-most states of Baden-Württemberg and Bavaria. A second group, with much higher unemployment rates of 10% to 20%, comprises all five Eastern German states as well as Berlin and Bremen. Next to these cross-sectional differences there is also substantial variation in the unemployment rate over time for any given state. In particular, we observe that the unemployment rate drops in the years of rapid economic growth around the turn of the millennium, rises towards the time of the Hartz reforms, and declines again afterwards.[18] The Great Recession only had a very modest impact on unemployment rates.

6 Trends in social assistance dynamics in Germany

41. Having discussed the definition of our social assistance variable and the data used in our analysis in the two previous sections, we provide in this section some first descriptive evidence of the trends in social assistance benefit receipt in Germany. In a first part, we look at the development of receipt probabilities and transition rates over time. The second part then focuses on the underlying changes in some of the important personal characteristics of social assistance recipients over the observation period.

6.1 Rates of benefit recipiency

42. Rates of social assistance benefit receipt in Germany have risen substantially over the observation period. As the top-left panel of Figure 2 shows, receipt rates are relatively stable at around 8% of working-age individuals in the first years of our panel, however start rising in 2001 to peak at 12.6% in 2006. The start of the rise in the rates of social assistance benefit receipt after the turn of the millennium coincides with the end of a period of strong economic growth in Germany. The frequency of benefit receipt remains elevated after 2006 during the years of the Great Recession and only drops to 10% in 2011, the last year of our panel.

43. The breakdown of social assistance into the different programmes in the same graph shows interesting differences in trends. Rates of receipt of Housing Benefits and Social Assistance are relatively stable until 2005, but then drop visibly with the introduction of the Hartz reforms. By contrast, rates of benefit receipt show a clear upward trend in the first decade of our panel for Unemployment Assistance, and are substantially higher still for the newly introduced Unemployment Benefit II after 2005. The drop in Social Assistance and the coinciding jump in rates of benefit from Unemployment Assistance to Unemployment Benefit II indicate that a large proportion of Social Assistance recipients were moved into UBII though the

[18] In 2005, the Federal Employment Agency changed its practices for reporting unemployment figures due to the merging of Unemployment Assistance and Social Assistance. To account for this change in our econometric analysis, we re-estimated our standard specification presented in Section 8 including an interaction term of the regional unemployment rate with a dummy variable for the post-Hartz years. The coefficient estimate for this interaction term turns out to be insignificant, however, which suggests that the change in reporting practices does not have a major impact on our results.

Hartz reforms. Similarly, rates of receipt for Housing Benefits drop as recipients who are transferred from UA to UBII lose eligibility to apply for Housing Benefits.

44. While the Hartz reforms are clearly reflected in the rates of benefit receipt for individual programmes, there is little evidence of a strong structural break in the overall frequency of benefit receipt for the year 2005. For ease of comparison, we plot the rate of receipt of either Social Assistance or Unemployment Assistance (until 2004) and Social Assistance or Unemployment Benefit II (from 2005) in the top-right panel of Figure 2. We also again plot the rate of social assistance receipt more broadly, the difference between the two lines thus representing the proportion of individuals who only receive Housing Benefits. We again observe a considerable rise in rates of benefit receipt from 2001, but no evidence of a jump in rates of benefit receipt from 2004 to 2005. Only a small share of individuals receives Housing Benefits only.

45. One of the drivers of the rising rates of social assistance benefit receipt might be a shift of individuals from unemployment insurance benefits into social assistance. As depicted in the bottom-left panel of Figure 2, rising rates of social assistance benefit receipt coincide with a decline in unemployment insurance benefit receipt over the same observation period. Indeed, the proportion of working-age individuals who report living in a household in which a member receives unemployment benefits at the time of interview drops by about two-thirds from 7.8% in 1995 to 2.5% in 2011. While part of this process might be the due to a shorter maximum duration of Unemployment Insurance benefit payments after the Hartz reforms, the process of rising social assistance and falling unemployment insurance recipiency rates already starts before the Hartz reforms.

46. Also, we do not find that rising rates of social assistance benefit receipt are associated with a larger share of individuals receiving benefits as a top-up to work (the so-called "*Aufstocker*"). As depicted in the bottom-right panel of Figure 2, the proportion of benefit-receiving households with at least one member in employment has risen during the Great Recession, but is not much higher than prior to the Hartz reforms. The proportion of households with a member in full-time employment even declined over the observation period.

47. A breakdown of benefit receipt by sex gives a higher rate of benefit receipt for females, but else very similar trends for both sexes. Figure 3 shows that for women, the average rate of benefit receipt during our observation period is 10.5%, compared to only 8.5% for males. Peak rates of in benefit receipt are just above 14% for females (in 2006) and about 11.5% for males (in 2010). Note that one of the reasons why we do not observe larger differences in the trends in benefit receipt between males and females might be that we have defined benefit receipt at the household level; differences in the frequency of receipt are thus primarily driven by differences in receipt patterns for singles with and without children.

48. Patterns of benefit receipt between Eastern and Western Germany differ much more substantially: As illustrated in Figure 4, we find that receipt rates in Eastern Germany are substantially higher averaging 17.7% compared to 7.9% for Western Germany. This is what we would expect given the much higher unemployment rates in Eastern Germany observed in Figure 1.[19] A notable exception is the rate of Social

[19] During our 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).

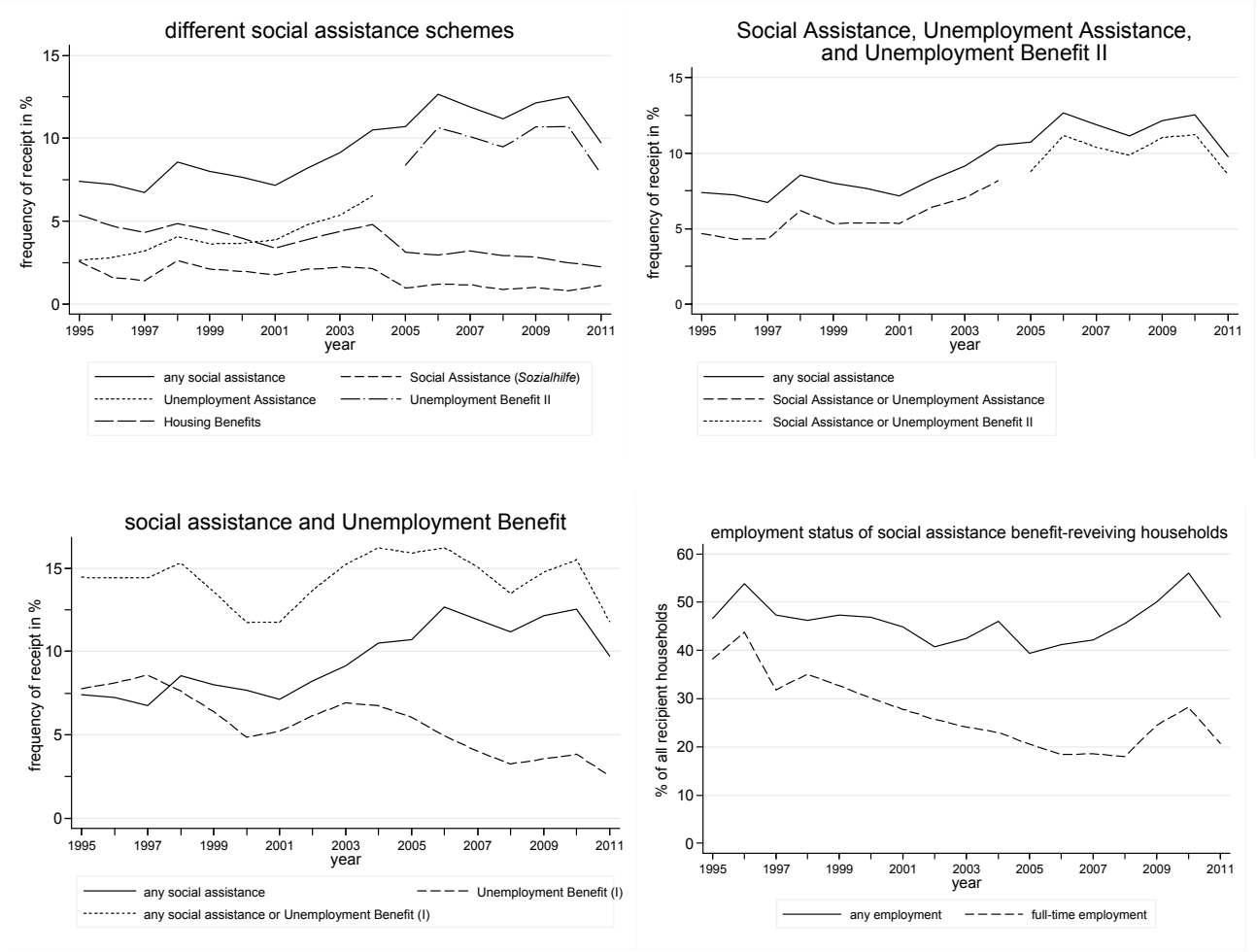
Assistance benefit receipt, which at least until the Hartz reforms is higher in Western Germany. The two parts of the country moreover display differences in the cyclical changes of social assistance benefit receipt rates over the observation periods. In Eastern Germany, rates of benefit receipt start rising in 1997, peak in the mid-2000s – a time of very high unemployment in Germany – and fall again thereafter. By contrast, rates of benefit receipt are low and stable in Western Germany until 2001 and reach their maximum in the years from 2006 to 2010.

49. Figure 5 confirms earlier findings of an 'immigrant-native gap' in benefit receipt, which has motivated a series of papers on immigrant participation in German social assistance systems (see for instance Riphahn, 1999; Riphahn & Wunder, 2012; Riphahn, Sander, & Wunder, 2013). We find that trends in benefit receipt over the observation period are very similar for the two subsamples, albeit at a level that is about twice as high for migrants compared to natives. Also, there are only small differences in the relative importance of the separate benefit programmes for the two groups. Note that we count both first- and second-generation migrants as belonging to the migrant subsample.

50. Finally, we find that there are large differences in the rates of benefit receipt for different family types, as summarized in Figure 6. Singles are much more likely to receive social assistance benefits than couples irrespective of whether they do or do not have children. For both singles and couples, households with a child however have a much larger probability of benefit receipt. The increase in rates of benefit receipt observed in Figure 2 seems to be primarily driven by rising rates of benefit receipt of childless households. The frequency of benefit receipt for single parents and couples with children in contrast has remained remarkably stable over the observation period.

51. Finally, we find that households with and without children differ in their propensity receive income support from different programmes. For childless households, especially couples, Unemployment Assistance / Unemployment Benefit II are the most important source of income support in terms of the rate of benefit receipt. Receipt of Social Assistance and Housing Benefits is much less frequent. By contrast, Housing Benefits and Social Assistance play an important role for single parents at least until the Hartz reforms, after which rates of UBII receipt are very high. The relatively high rates of Housing Benefit receipt for household with children certainly reflect the fact that the income thresholds used to determine eligibility for this benefit are a function of household size.

Figure 2. rates of benefit receipt – all individuals

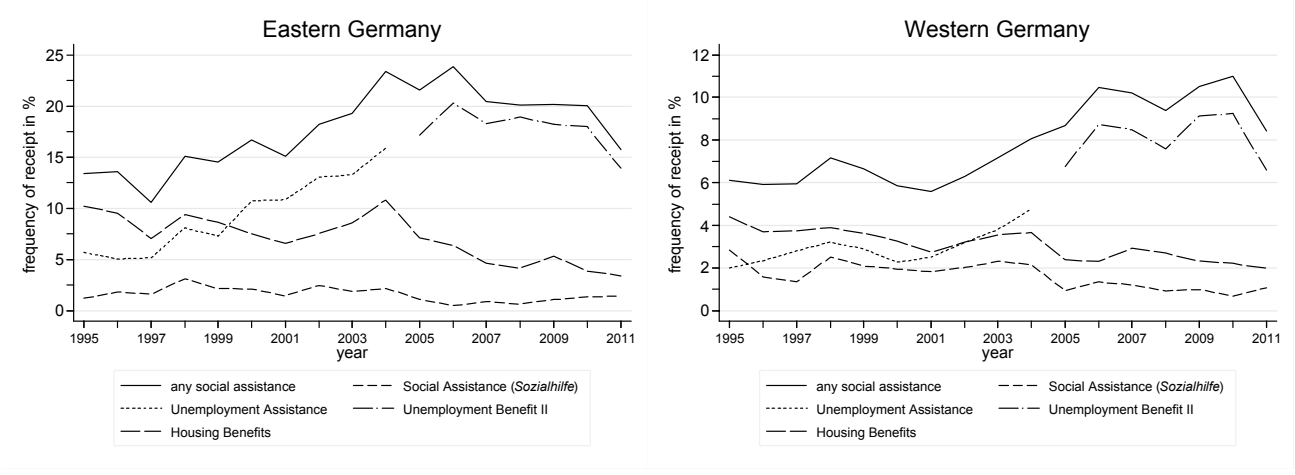


Source: SOEP, 1995-2011; rates of benefit receipt were calculated using cross-sectional individual sampling weights; the frequency of benefit receipt is the proportion of working-age individuals who live in a benefit-receiving household at the time of the interview. The employment status of benefit-receiving households refers to any individual in the household being employed (full-time) at the time of the interview. These numbers have been calculated using cross-sectional household sampling weights.

Figure 3. rates of benefit receipt – by sex

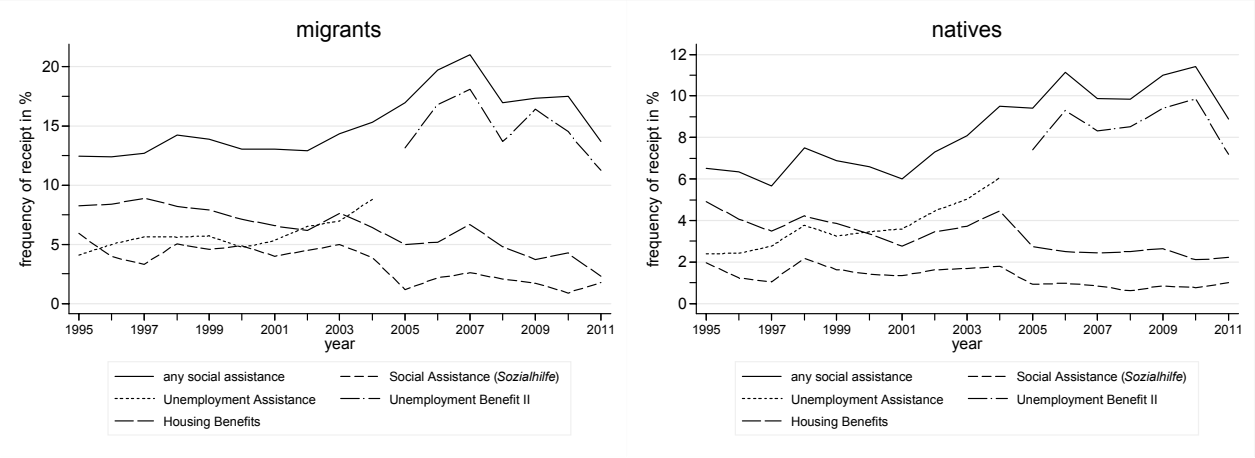
Source: SOEP, 1995-2011; rates of benefit receipt were calculated using cross-sectional individual sampling weights; the frequency of benefit receipt is the proportion of working-age individuals who live in a benefit-receiving household at the time of the interview.

Figure 4. rates of benefit receipt – Eastern vs. Western Germany



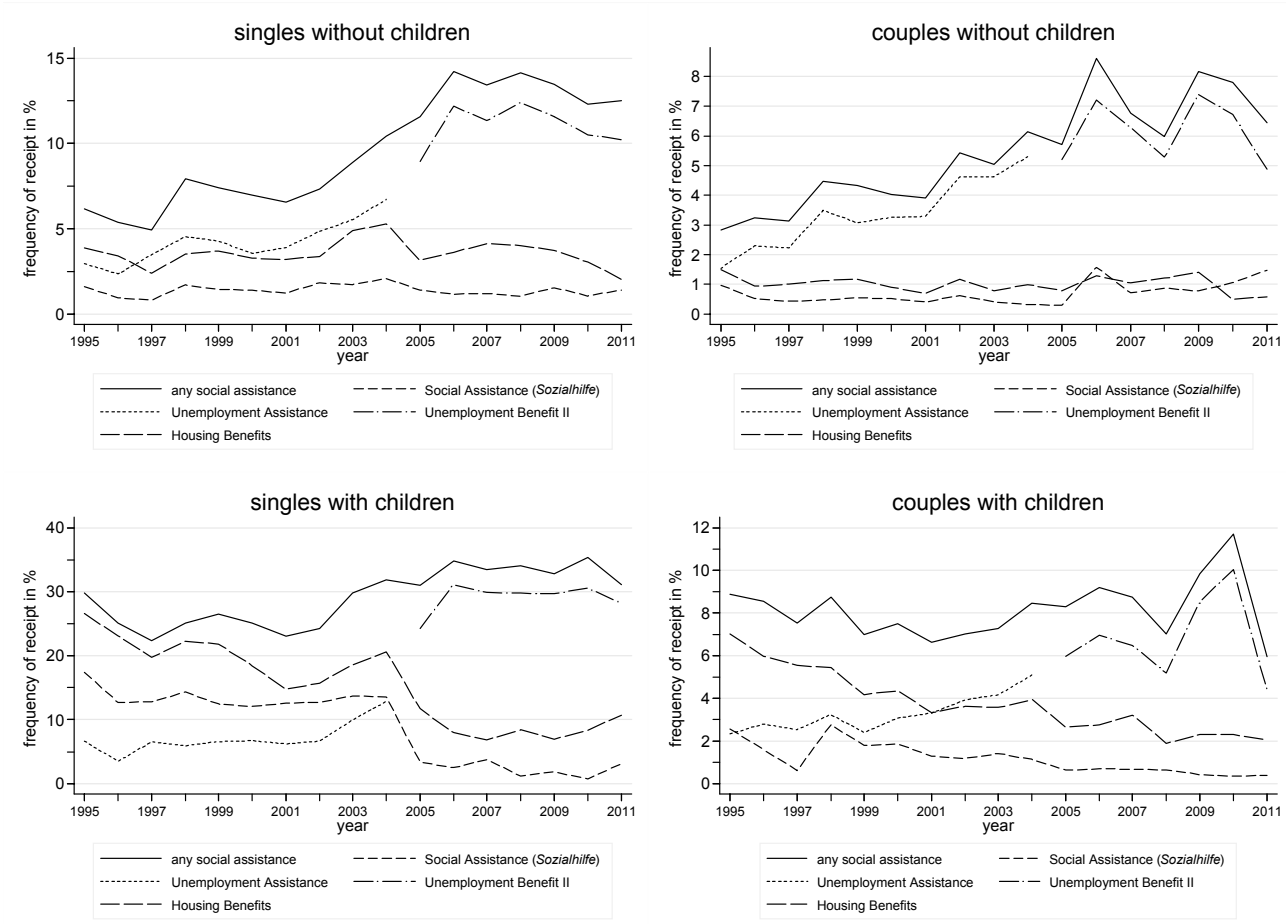
Source: SOEP, 1995-2011; rates of benefit receipt were calculated using cross-sectional individual sampling weights; the frequency of benefit receipt is the proportion of working-age individuals who live in a benefit-receiving household in that region at the time of the interview.

Figure 5. rates of benefit receipt – migrants vs. natives



Source: SOEP, 1995-2011; rates of benefit receipt were calculated using cross-sectional individual sampling weights; the frequency of benefit receipt is the proportion of working-age individuals who live in a benefit-receiving household in that region at the time of the interview.

Figure 6. rates of benefit receipt – by family type



Source: SOEP, 1995-2011; rates of benefit receipt were calculated using cross-sectional household sampling weights; the frequency of benefit receipt is the proportion of households of the given type at the time of the interview.

6.2 Benefit transition rates

52. A look at the frequency of transitions into and out of benefit receipt can help to better understand the dynamics that underlie the cross-sectional receipt rates presented in the previous subsection. Indeed, even in the case of relatively stable rates of benefit receipt, persistence in benefit receipt at the individual level might be low with individuals frequently entering and leaving benefit receipt. For the reasons outlined earlier, it is however relevant to know for policy makers whether the population of benefit recipients is made up of one uniform group of long-term recipients, or whether it rather consists of a heterogeneous group of individuals who stay on benefits for a short possibly for a repeated number of times. While our ability to describe the exact micro-dynamics of benefit receipt is limited based alone on annual data, some additional evidence on the dynamics of social assistance benefit receipt can be obtained from looking at the transition rates into and out of benefit receipt that underlie the patterns of receipt frequencies described above.

53. For this purpose, we define an **entry rate** into benefit receipt 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, we calculate **exit rates** by dividing the number individuals in receipt at time *t-1* but not anymore in receipt at time *t* by the total number of individuals in receipt at time -*t-1*. Note that unlike for the cross-sectional recipiency rates presented above, we require panel data for the calculation of transition rates since we only use individuals who are observed in both periods *t* and *t-1*. Breakdowns of transition rates between different groups by region of residence or family type are based on information provided at time *t*.

54. A plot of social assistance benefit transition rates for the entire sample in Figure 7 indicates that the rise in the rate of benefit receipt observed in Germany since 2001 seems to have been primarily due to a drop in the exit rate from around or above 40% until 2001 to below 30% after that. While entry rates also rise in the first years of the 2000s, they drop again in 2006 and generally do not show a clear trend over the observation period fluctuating between 3% and 4.5%. This finding is remarkable, since declines in the rates of benefit receipt observed for other countries seem to have been primarily driven by falling entry rates while exit rates generally remained stable (see for instance Cappellari & Jenkins (2008) for Britain or Finnie & Irvine (2008) for Canada).

55. An important implication of these benefit transition rates is that there is substantial observed (or 'raw') state dependence in social assistance benefit receipt in Germany. Average exit rates of around 32% over the observation period imply that 68% of social assistance benefit recipients in a given year will continue to receive benefits in the following year. This number stands in stark contrast to entry rates into social assistance of around 3.5% on average. It is of course worth emphasising again that we make no assumptions about the timing of benefit receipt during a given year. This implies that we cannot distinguish between longer, uninterrupted spells and multiple 'repeat spells'. In practice, repeated benefit receipt at subsequent interview dates will be perceived as persistence even if the individual did not receive benefits for the entire period between those two interview dates.

56. A disaggregation of benefit transition rates by sex as presented in Figure 8 shows that there are only small differences between females and males. The higher rates of benefit receipt for females appear to

be due to a combination of slightly lower exit rates, and substantially higher entry rates for a short period in the early and mid-2000s. Overall trends for the two subsamples however are very similar.

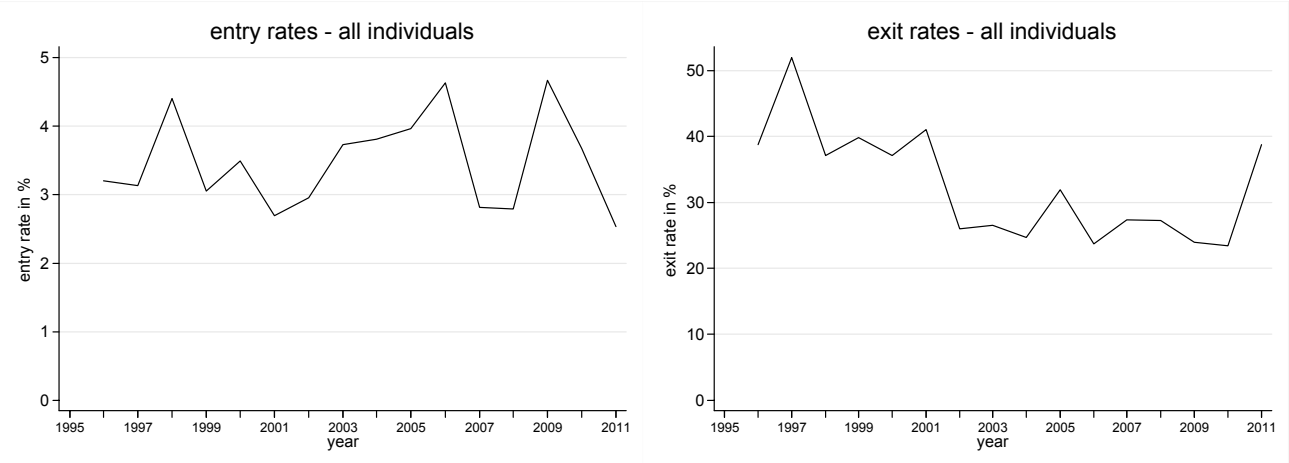
57. Much stronger differences in transition rates can be observed by region of residence. Figure 9 illustrates that while exit rates in Eastern and Western Germany are very similar, entry rates into benefit receipt in Eastern Germany are substantially higher than for Western Germany, especially from the late 1990s to the mid-2000s. We observe a convergence in exit rates after 2006. Differences in the level of benefit receipt between the two parts of the country however clearly seem to be driven by differences in entry rates. Note that the higher volatility of benefit transition rates for Eastern Germany is the result of the much smaller sample size.

58. A comparison of benefit transition rates between migrants and natives gives a similar finding. As displayed in Figure 10, we again find that exit rates for the two groups differ by very little, while entry rates for migrants are up to twice as high as for natives.

59. It is more difficult to derive clear results from a disaggregation of benefit transition rates by family types. As illustrated in Figure 11, the higher rates of benefit receipt for single parents seem to be clearly the result of much higher entry rates, though exit rates are also lowest for this group. For the three remaining family types, differences in benefit transition rates are hard to identify, keeping in mind that especially exit rates from benefit receipt have been calculated based on a very low number of observations.

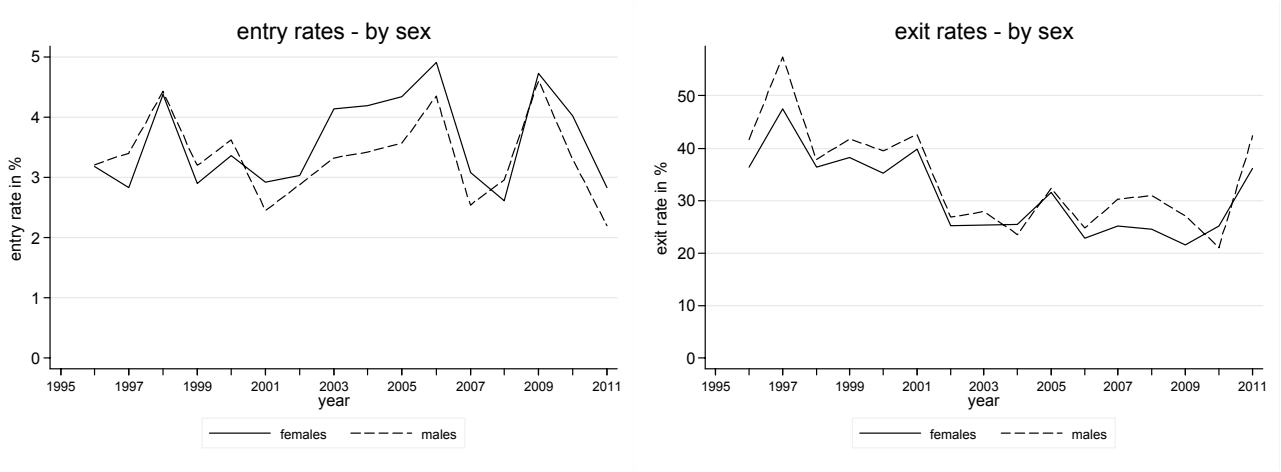
60. To sum up, we find that the rise in the rate of benefit receipt in Germany over time seems to be primarily driven by a drop in exit rates in 2001, while entry rates display no clear trend over the observation period. By contrast, we find that differences in rates of benefit receipt *between subsamples* appear to be primarily due to cross-group differences in entry rates. This applies at least for the differences in receipt rates observed between Eastern and Western Germany, between first- or second-generation migrants and natives, and for the high frequency of benefit receipt for single parents. Benefit transition rates for all subsamples imply substantial 'raw' state dependence, the level of which has moreover risen over the observation period due to the drop in exit rates from benefit receipt.

Figure 7. benefit transition rates – all individuals



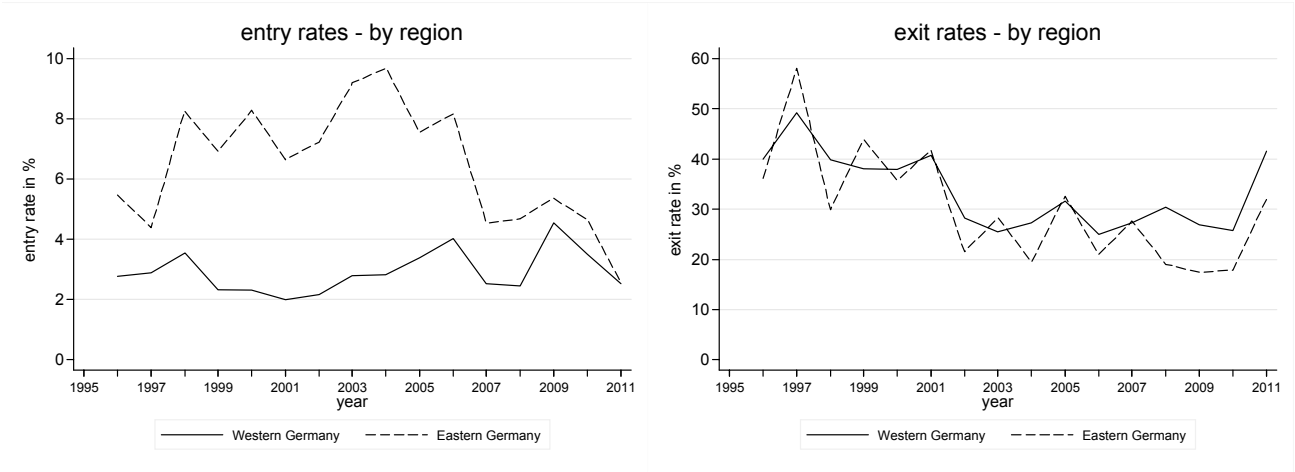
Source: SOEP, 1995-2011; benefit transition rates were calculated using cross-sectional individual sampling weights.

Figure 8. benefit transition rates – by sex



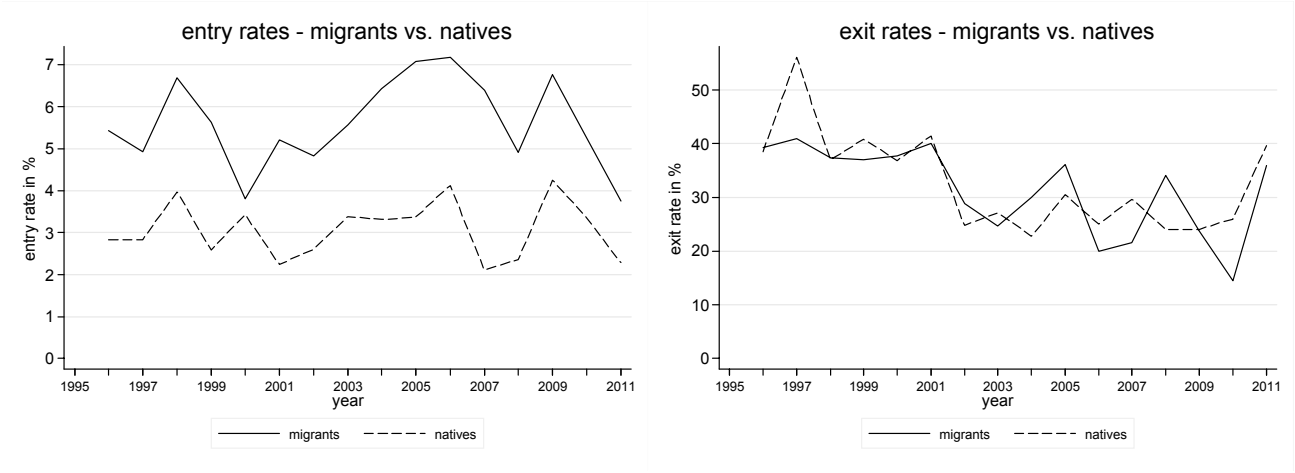
Source: SOEP, 1995-2011; benefit transition rates were calculated using cross-sectional individual sampling weights.

Figure 9. benefit transition rates – Eastern vs. Western Germany



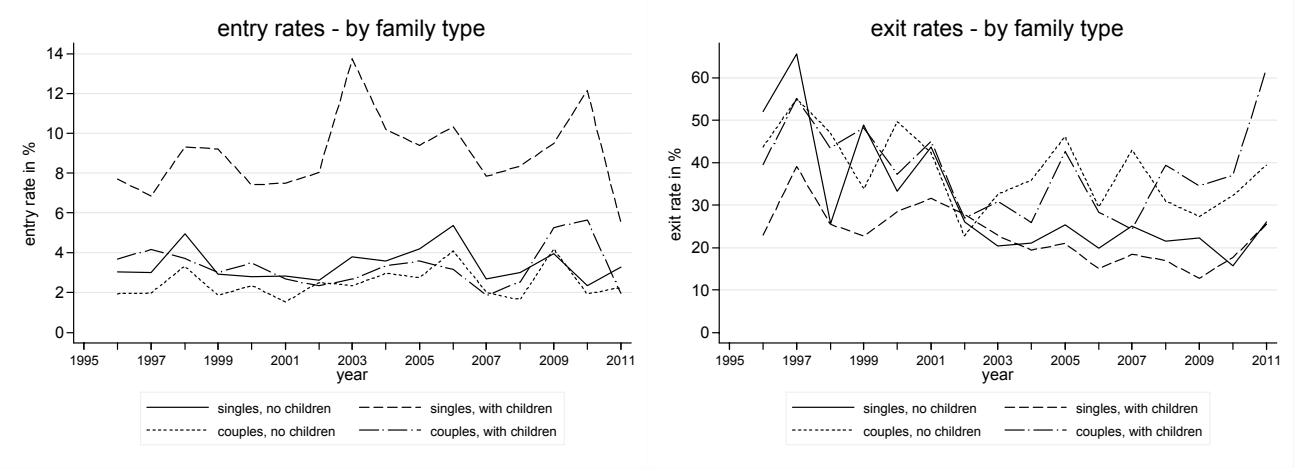
Source: SOEP, 1995-2011; benefit transition rates were calculated using cross-sectional individual sampling weights.

Figure 10. benefit transition rates – migrants vs. natives



Source: SOEP, 1995-2011; benefit transition rates were calculated using cross-sectional individual sampling weights.

Figure 11. benefit transition rates – by family type



Source: SOEP, 1995-2011; benefit transition rates were calculated using cross-sectional household sampling weights.

6.3 Changes in recipients' characteristics

61. The development of rates of benefit receipt over time described earlier should also be reflected in the observable characteristics of the recipient population. Changes in the institutional features of the income-support system, notably the Hartz reforms, will have affected which groups of individuals are eligible for receipt of minimum-income benefits. Similarly, falling unemployment rates will imply that also a smaller share of the population will require income support. The changing composition of the group of benefit recipients in turn will affect the rate at which individuals in benefits manage to become self-sufficient and stop receiving benefit payments.

62. In Figure 12, we track the development of some key personal or household characteristics for benefit recipients and non-recipients over the observation period. In particular, we focus on three variables that might be considered potential 'risk factors' of benefit receipt: low education, poor health status, and the presence of a young child in the household. For each of these variables, we first compare in left panel the characteristics of social assistance benefit recipients to those of the non-recipient population. In the right panel, we then disaggregate recipient characteristics by programme type.

63. The top-left panel of Figure 12 illustrates that the proportion of individuals with little educational attainment differs strongly by social assistance benefit receipt status. We find that among benefit recipients, about one in four individuals have less than ten years of education and this fraction is remarkably stable over the observation period. Among non-recipients by contrast, the corresponding share is only about 10% and gradually declines over time.

64. It is more difficult to identify any clear differences in the proportion of individuals with little education between benefit programmes. From the top-right panel of Figure 12, we take that in the years before the Hartz reforms, the fraction of recipients with little education is slightly higher for Social Assistance compared to the other benefit programmes. This would make intuitive sense given that low-skilled individuals will have been less likely to have the contribution history necessary for eligibility to Unemployment Assistance. These differences cease to persist after 2005, presumably as employable recipients with little education are moved into Unemployment Benefit II.

65. Recipients of social assistance benefits are also more likely than non-recipients to have poor health. In the centre-left panel of Figure 12, we plot the proportion of individuals who report suffering from 'poor' or 'bad' health at the time of the interview. [20] We observe that this share rises from around 20% early in the observation period to close to 30% in the late 2000s, compared to a stable 10-15% in the non-recipient population. When disaggregating recipients by benefit programme, we find few differences in the proportion of individuals with poor health across benefit schemes before 2005. The Hartz reforms then however induce a jump in the fraction of individuals with poor health among recipients of Social Assistance. This illustrates the stronger separation of benefit recipients into employable and non-employable individuals introduced through the reforms.[21]

66. Finally, a relatively large proportion of benefit recipients lives in a household with a small child, which might hint at the parents' care duties representing a hurdle to self-sufficiency. From the bottom-left graph, we take that the share of recipient households with a child aged six years or younger declines over the observation period from around 37% in 1995 to 17% in 2011. Still, the share is higher than the corresponding number among non-recipients, which averages around 15% over the period.

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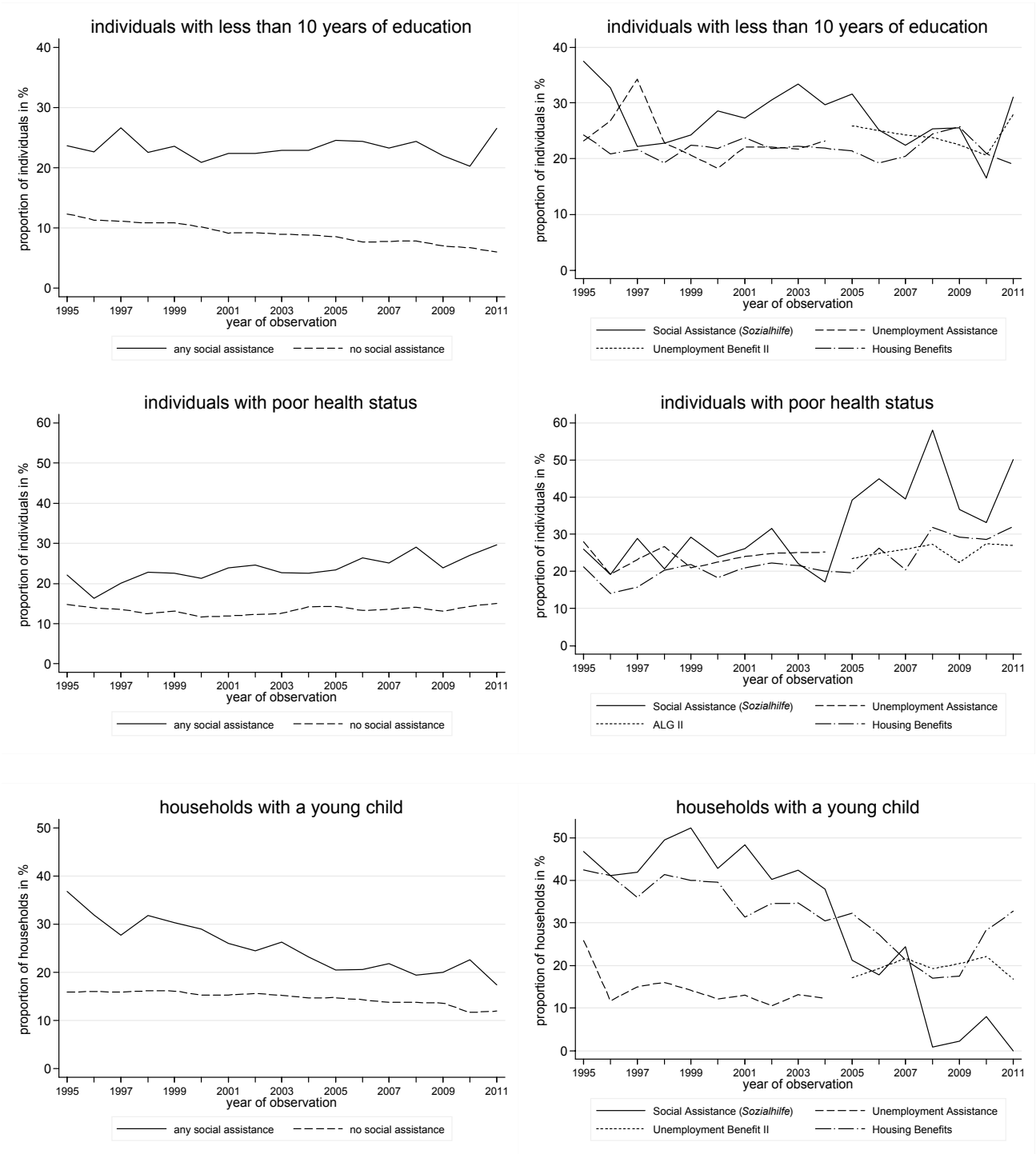
[20] The exact wording of the question is "How would you describe your current health" with the five possible responses being 'very good', 'good', 'satisfactory', 'poor', and 'bad'.

[21] If the gap in the proportion of individuals with health problems between the different programmes seems smaller than expected, it is important to keep in mind that we define social assistance benefit receipt status at the household level. As an individual's poor health status might be a threat to self-sufficiency also for other household members, the inclusion of these healthy individuals in the calculations depresses the reported share of social assistance recipients with health problems.

Figure 12. recipient characteristics



Source: SOEP, 1995-2011; the shares of individuals with low education and health problems were calculated using cross-sectional individual sampling weights; cross-sectional household sampling weights were used for the calculation of the proportion of households with a young child, which we define as a child aged 6 years and below.

67. The Hartz reforms induced a substantial change in share of households with children across the different benefit programmes. As shown in the bottom-right panel of Figure 12, 40-50% of Social Assistance recipient households and 30-40% of Housing Benefit recipient households have a young child prior to 2005. The corresponding fraction among household receiving Unemployment Assistance is only about 15%. After the reforms, the share of households with young children drops to below 10% for Social Assistance as employable recipients are moved to Unemployment Benefit II. An interpretation of the exact figures however is more difficult after the Hartz reforms due to the small cell sizes for Social Assistance and Housing Benefits.

68. Overall, these results indicate that social assistance benefit recipients and non-recipients differ strongly in terms of their personal and household characteristics. In particular, we have seen that the fraction of individuals with little education or health problems tends to be much larger among benefit recipients. Also, social assistance benefit recipients are more likely to live in a household with a small child. In the econometric analysis presented in the following section, we provide a more formal analysis of the relationship between these personal characteristics and the probability of benefit receipt. This will help us explain to what extent the persistence observed in benefit receipt is simply due to a higher prevalence of 'unfavourable' labour market characteristics among benefit recipients as opposed to reflecting genuine state dependence.

7. Econometric approach

69. In the previous section, we demonstrated that the dynamics of social assistance benefit receipt in Germany are characterised by a substantial degree of persistence. While this persistence might have a 'structural' component, our comparison of personal characteristics of recipients and non-recipients shows that we can observe considerable heterogeneity among individuals. Similarly, we would expect recipients and non-recipients to differ in terms of unobservable characteristics.

70. In this section, we discuss the model used in our econometric analysis. The specification chosen allows us to control for observable characteristics and persistent unobserved heterogeneity and thus helps us to better assess the influence of past benefit receipt and an individual's socio-economic characteristics on the probability of social assistance benefit receipt.

71. The standard model in recent empirical work on the dynamics of social assistance benefit receipt is the dynamic random-effects probit model. Let y_{it} be a binary outcome variable such that for $y_{it} = 1$ individual *i* is in receipt of social assistance benefits in period *t*. We can write a latent variable specification for this outcome as

$$\begin{aligned} y_{it} &= 1\{y_{it}^* > 0\} \\ &= 1\{x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + u_{it} > 0\} \quad \forall\ i = 1, \dots, N;\ t = 1, \dots, T \end{aligned} \tag{1}$$

where $u_{it} = \alpha_i + \varepsilon_{it}$. An individual receives social assistance benefits if the latent variable y_{it}^* is larger than zero, with the value of this latent variable depending linearly on a vector of observable characteristics

$x_{i(t-1)}$[22] and the observed receipt status in the previous period $y_{i(t-1)}$. The error term u_{it} enters additively within the indicator function and can be decomposed into an individual-specific random intercept α_i and a white noise component ε_{it}. Intuitively, we may think of this type of specification as arising from a situation in which an individual chooses between alternatives that yield different levels of utility. The individual will pick the option – in our case social assistance benefit receipt or non-receipt – that provides the greatest utility.

Based on this specification, the probability of receiving social assistance benefits follows as

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

where $F(\cdot) = \Phi(\cdot)$ is the cumulative distribution function under the assumption that ε_{it} follows a standard normal distribution. Both components of the error term are assumed to be mean zero, $E(\alpha_i) = E(\varepsilon_{it}) = 0$, and uncorrelated with each other, $E(\alpha_i \varepsilon_{it}) = 0$. The white noise error term ε_{it} is further assumed to be serially uncorrelated.[23]

72. Following Heckman (1981a), the coefficient of the lagged dependent variable λ in this model can be interpreted as measuring genuine or 'structural' state dependence. 'Spurious' state dependence induced by permanent unobserved heterogeneity is captured by the persistent individual-specific error term α_i, which could represent differences in unobserved labour market ability or an individual's distaste for the receipt of social assistance benefits.

73. The specification assumes implicitly that the dynamics of benefit receipt are correctly represented by a first-order Markov model and that the observed characteristics $x_{i(t-1)}$ are strictly exogenous. While the first restriction can be easily relaxed by the inclusion of additional lags, the strict exogeneity assumption may be more problematic. Intuitively, only the values of our observables x_i in period *t-1* and the value of the dependent variable in the previous period $y_{i(t-1)}$ matter for determining y_{it}. An impact of earlier lags or feedback effects between the dependent variable and future values of x_i are excluded (Wooldridge, 2004). This rules out the possibility of benefit receipt affecting fertility or partnering decisions if we include household size, the age of an individual's children, or the family status as explanatory variables in our specification. While the assumption is thus clearly restrictive, previous studies on social assistance dynamics have nonetheless relied on strict exogeneity since allowing for feedback effects makes this type of models considerably less workable.[24]

74. One difficulty for the estimation of dynamic discrete-choice models is that the specification proposed above suffers from an **initial conditions bias**. As for linear dynamic panel data models with unobserved heterogeneity, the individual-specific error component α_i induces a correlation between the error term and the lagged dependent variable. Under the assumptions given above, this correlation is constant

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22 Note that alternative specifications of the model use current rather than lagged values of the observable characteristics x_{it}. In our approach, we follow Cappellari & Jenkins (2009).

23 For a relaxation of this assumption, see Stewart (2006; 2007).

24 For a possibility of incorporating feedback effects into this type of models, see Biewen's (2009) analysis of poverty dynamics in Germany.

across periods at $\rho = \sigma_\alpha^2/(\sigma_\alpha^2 + \sigma_\varepsilon^2)$ and results in inconsistent coefficient estimates even for a large number of individuals *N* as long as the number of time periods *T* is small. For linear models, this problem can be addressed by a transformation of the model through first-differencing and subsequent use of instrumental variable techniques (Anderson & Hsiao, 1981; Arellano & Bond, 1991). No such transformation is available, however, in the case of non-linear discrete-choice models.

75. The endogeneity induced through persistent unobserved heterogeneity can be dealt with by modelling the initial outcome of the dynamic process y_{i0}. To make this more obvious, we write the joint density of a sequence of outcomes for one individual $y_i' = (y_{i1}, \dots, y_{iT})$ as

$$f(y_{i1}, \dots, y_{iT}|y_{i0}, x_i, \alpha_i; \theta) = \prod_{t=1}^{T} f(y_{it}|y_{i(t-1)}, \dots, y_{i1}, y_{i0}, x_i, \alpha_i; \theta)$$

$$= \prod_{t=1}^{T} \left[\Phi\left(x_{i(t-1)}'\beta + \lambda y_{i(t-1)} + \alpha_i\right)\right]^{y_{it}} \left[1 - \Phi\left(x_{i(t-1)}'\beta + \lambda y_{i(t-1)} + \alpha_i\right)\right]^{1-y_{it}} \quad (3)$$

where $\theta = (\beta', \lambda')'$ denotes the vector of coefficients. This expression cannot be used directly to construct a likelihood function and to estimate θ because the α_i are unknown. While the individual-specific effect α_i could be integrated out, we first need to specify its relationship with the outcome in the initial period y_{i0}, which is part of the set of variables we condition on. Typically, we will have to assume that the outcome in the initial period and the unobserved heterogeneity term are correlated, in which case naively ignoring the endogeneity of the initial conditions will lead to inconsistent estimates.

76. Different methods have been proposed to deal with this problem, the simplest of which is the approach introduced by Wooldridge (2005) which we use in this study.[25] Wooldridge suggests to specify a density for the unobserved individual-specific effect conditional on the outcome in the initial period, $\alpha_i|y_{i0}, x_i$, which can be used to obtain the density of $y_{i1}, \dots, y_{iT}|y_{i0}, x_i$ as

$$f(y_{i1}, \dots, y_{iT}|y_{i0}, x_i, \zeta) = \int f(y_{i1}, \dots, y_{iT}|y_{i0}, x_i, \alpha_i; \theta) g(\alpha_i|y_{i0}, x_i; \gamma) d\alpha_i.$$

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[25] The earliest and most widely used method was proposed by Heckman (1981b), who suggests to multiply the product given in equation (3) with an approximation of the unknown density of $y_{i0} \mid x_i, \alpha_i$. This removes the conditioning on y_{i0} to give $f(y_{i0}, y_{i1}, \dots, y_{iT}|x_i, \alpha_i; \theta)$. For a chosen density of $\alpha_i|x_i$, we can then integrate out the dependence of the modified joint density on the individual-specific effects. The resulting density can then be used for maximum likelihood estimation. An alternative approach proposed to solving the initial conditions problem was proposed by Orme (2001) but 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. Simulation results indicate that the Heckman approach works better for short panels, whereas the Wooldridge method is preferable for methods of medium length. For long panels, little difference is found in the performance of the two methods. Similarly, Cappellari & Jenkins (2008) find in their analysis that all three approaches yield nearly identical results.

77. A natural choice for the distribution of $\alpha_i|y_{i0}, x_i$ in the case of the dynamic probit model is the normal distribution. Specifically, Wooldridge proposes to specify the distribution of the unobserved heterogeneity as $\alpha_i|y_{i0}, x_i \sim \mathcal{N}(\gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2, \sigma_a^2)$, where x_i is a vector that includes the values of the past and future values for all observable characteristics. For $\alpha_i = \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i$, we can rewrite the joint density from equation (3) as

$$\prod_{t=1}^{T}\left[\Phi\left(x_{i(t-1)}'\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i\right)\right]^{y_{it}} \left[1 - \Phi\left(x_{i(t-1)}'\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i\right)\right]^{1-y_{it}}.$$

We can now integrate out the modified unobserved heterogeneity component to write the joint density of $y_{i1}, \ldots, y_{iT}|y_{i0}, x_i$ (i.e. unconditional on a_i) as

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

78. This likelihood is comparable to the one of the standard random-effects probit model with the additional explanatory variables y_{i0} and x_i added in each period *t*. It can be used for maximum likelihood estimation in which we additionally condition on x_i and y_{i0}. Intuitively, the Wooldridge procedure rests on the assumption that once we include the initial value of the outcome variable y_{i0} and the lags and leads of all explanatory variables x_i as additional regressors in the model, all remaining unobserved heterogeneity a_i is uncorrelated with the explanatory variables. Under this assumption, the estimated coefficients will be consistent as the number of observations *N* tends to infinity.

79. A simplification of this specification that has become widely used in the empirical practice includes the vector of individual longitudinal averages of all time-varying observed characteristics $\bar{x}_i$ instead of all past and future values of these variables x_i. This has the advantage of substantially reducing the number of regressors and thus the computation time of the model.[26]

80. Rabe-Hesketh & Skrondal (2013) warn that especially for short panels, this simplification can lead to biased results and suggest that at least the initial values of all explanatory variables should be added to the simplified specification. In his study of poverty dynamics in Germany, Biewen (2009) instead includes additional interaction terms between the initial value of the outcome variable and the averaged time-varying observables. We have tested both extensions of the simplified model but find that for the relatively long panel we use adding these extra regressors does not impact the estimated effect of the lagged dependent variable.

81. The advantage of the Wooldridge approach is that we can easily implemented it using the standard panel commands in statistical software packages. We use Stata's `–gllamm–` command for estimation because it allows for robust estimation and the use of sampling weights (Rabe-Hesketh, Skrondal, &

[26] This specification is referred to as the Mundlak-Chamberlain approach.

Pickles, 2004; 2005). The command uses adaptive quadrature methods with eight quadrature points for evaluation of the integrals and we have found the results to be robust to an increase in the number of integration points. We present the results from these estimations in the following section.

8. Estimation results

82. The estimation results presented in this section come from a series of dynamic random-effects probit models estimated for different subsamples. In these models, we include three main sets of covariates, *(i)* personal characteristics, *(ii)* household characteristics, *(iii)* and partner characteristics, as follows:

- **personal characteristics** include the individual's sex and (first- or second-generation) migrant status in the form of dummy variables. Age and years of education enter through a simple and a quadratic term[27] to allow for non-linear relationships with benefit receipt. The dummy variables 'good health' and 'poor health' indicate whether an individual reports her health status to be 'very good'/'good' or 'poor'/'bad' in the current period. The base category is 'satisfactory'.
- **household characteristics** include the family type in the form of three dummy variables for singles with children, couples without children, and couples with children ('single without children' being the base category). We also add a dummy variable that indicates the presence of a child aged six years or younger in the household, as well as a measure of household size.
- **partner characteristics** include again linear and squared age and years of education and dummies for health status and migrant status. These variables are set equal to zero if the individual is single or where partner information is missing. Intuitively, this is equivalent to interacting partner variables with a dummy that indicates whether an individual is partnered or not: for an individual with partner the interaction takes the value of the partner's variable whereas for an individual without partner the interaction term is zero.

83. Finally, the specifications include year dummies and, unless noted otherwise, dummies for the month in which the SOEP interview was conducted. These are meant to capture time trends in social assistance benefit receipt over the years as well as seasonal effects. We also control for residence in Eastern Germany and the annual state-level unemployment rate.

8.1 The baseline specification

84. The initial step of the econometric analysis is to examine the importance of initial conditions bias and the impact of using sampling weights. This exercise helps us determine an appropriate baseline specification, which can then be adapted to study changes in the benefit dynamics over time and for specific subsamples. We start off with the simplest specification without any controls for initial conditions as presented in Equation (1), which we refer to as **Model I**. In this model, the probability of social assistance benefit receipt in the current period is given as

$$P\left(y_{it} = 1 \middle| y_{i0}, \dots, y_{i(t-1)}, x_i, \alpha_i\right) = \Phi(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \alpha_i).$$

85. As discussed, we however expect this model to be inconsistent due to the correlation between the unobserved individual-specific effect α_i and the lagged dependent variable $y_{i(t-1)}$. Specifically, since un-

[27] The values for age^2 have been divided by 100 to produce suitably-sized coefficient estimates.

observed heterogeneity affects the probability of current and past social assistance receipt alike, α_i will be positively correlated with the lagged dependent variable and λ will be biased upwards.

86. To account for the possibility of endogenous initial conditions, we extend this specification following the Wooldridge approach by including individual longitudinal averages of the time-varying variables and the receipt status in the initial period as additional regressors. Specifically, we use time-averages of the different family-type variables, the dummy for a child aged under six years living in the household, household size, the respondent's and her partner's health status, and the regional unemployment rate. The probability of social assistance benefit receipt for this modified specification, which we call **Model II**, can be written as

$$P\left(y_{it} = 1 \middle| y_{i0}, \dots, y_{i(t-1)}, x_i, a_i\right) = \Phi(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + \overline{x}'_i \gamma_2 + a_i),$$

where $\overline{x}_i$ is the vector of the individual time-averages of observable characteristics and y_{i0} is the social assistance benefit receipt status in the initial period. Note that in both of these specifications our explanatory variables enter in the form of lags rather than their current values as discussed in the previous section.

87. Panel A of Table 3 presents coefficient estimates for the two specifications. From column I, we take that the coefficient of the lagged dependent variable of Model I is positive and highly significant. This can be interpreted as evidence that even after controlling for individual heterogeneity there might be a structural component in the observed persistence of social assistance benefit receipt. In the bottom of the table, we provide parameter estimates that relate to the error term. The estimated standard deviation of the individual-specific effect σ_α is 0.58, which translates into an estimate for the serial correlation in the error term $\rho = Corr(u_{is}, u_{it}) = \sigma_\alpha^2/(1 + \sigma_\alpha^2)$ of 0.25. Intuitively, one quarter of the error variance in Model I can be attributed to persistent unobserved heterogeneity.

88. Yet, there is good reason to believe that these estimates suffer from initial conditions bias that spuriously attributes unobserved heterogeneity to the lagged dependent variable. Column II therefore presents the coefficient estimates of our Wooldridge specification. We find that the coefficient of the lagged dependent variable indeed drops substantially from 1.725 to 1.152. The reduction in $\hat{\lambda}$ coincides with a strong rise in the estimated standard deviation of the random effect $\widehat{\sigma_a}$ from 0.58 to 0.86. This translates into an increase in $\hat{\rho}$ from 0.25 to 0.43, which implies that in our Wooldridge specification 43% of the composite variance come from the persistent individual-specific component. The control for the receipt status in the initial period SA_0 is strongly significant and positive. This illustrates that individuals who receive benefits in the first period for which they are observed also have a significantly higher probability of receiving social assistance in following periods. Our results thus confirm that the failure to account for endogenous initial conditions in Model I leads to a serious overestimate of the degree of state dependence.

89. While the sign of the coefficient estimates is indicative of the direction of the effect of the respective variables, their magnitudes are not directly informative due to the non-linear nature of the model. Coefficient estimates can however be used to predict entry and persistence rates in social assistance and to calculate partial effects. To assess the estimated degree of state dependence in our models, we calculate the average partial effect of past social assistance receipt on receipt in the current period for our two specifications.

90. Following Wooldridge (2005), under the conditions discussed earlier, one can consistently estimate an individual's expected probability of social assistance receipt in period *t*

$$E\left[\Phi(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \gamma_0 + \gamma_1 y_{i0} + \bar{x}'_i\gamma_2 + a_i)\right]$$

as

$$\frac{1}{N}\sum_{i=1}^{N}\left(\Phi\left(x'_{i(t-1)}\hat{\beta} + \hat{\lambda} y_{i(t-1)} + \hat{\gamma}_0 + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2\right)(1-\hat{\rho})^{\frac{1}{2}}\right),$$

where $\hat{\rho} = \widehat{\sigma_a^2}/(1 + \widehat{\sigma_a^2})$ is the estimate of the inter-temporal correlation of the composite error terms for any two different periods. Based on this expression and using the coefficient estimates for Model II, we calculate the average probability of social assistance receipt conditional on receipt in the previous period across all individuals and time periods to be 20.4% as reported in Panel B of Table 3. The probability of benefit receipt conditional on non-receipt in the previous period is 6.3%.

91. From these numbers, we can now calculate the average partial effect (APE) of past social assistance receipt on the receipt probability in the current period and the predicted probability ratio (PPR) for individuals with and without social assistance receipt in the previous period. Both numbers serve as measures of state dependence.

92. The APE is defined as the difference in average predicted probabilities of social assistance receipt across individuals and time with and without social assistance receipt in the previous period, i.e. the average difference between rates of entry to and persistence in social assistance:

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

93. Using the entry and persistence rates calculated for Model II above, we obtain the average partial effect of social assistance receipt in the previous period as $APE = 20.4 - 6.3 = 14.1$. Intuitively, an individual who received social assistance benefits in the previous period is hence on average 14 percentage points more likely to receive social assistance in the current period than if she had not received social assistance.

The PPR is defined as the ratio of the average predicted probabilities with and without social assistance receipt in the past period

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

and can be calculated as $\text{PPR} = \frac{20.4}{6.3} = 3.3$. Hence, an individual is on average three times more likely to receive social assistance if she already received social assistance in the last period compared to the counterfactual where she did not receive social assistance.

94. For Model I, we indeed calculate a much higher persistence rate of 37.0% and a lower entry rate of only 4.5%, which translates into an APE of 32.5 percentage points and a PPR of 8.2. Failing to adequately control for the endogeneity of initial conditions thus leads us to overestimate the degree of state dependence by a factor of more than two in terms of both measures.

95. For the interpreting of the results from our models it is important to note that the prediction of entry and persistence rates is a hypothetical exercise designed to evaluate the degree of structural state dependence. The numbers are *not* meant to be predictions of our actual transition rates presented in Section 6. The reason is that our predictions are based on the actual and counterfactual receipt status for *every* individual in the sample. Observed entry rates by contrast are by definition calculated for the subsample of individuals *who did not receive* social assistance benefits in the previous period. As shown, these individuals differ systematically from social assistance benefits recipients in terms of their personal characteristics. Intuitively, our predicted entry rate gives an answer to the hypothetical question what fraction of individuals would enter social assistance if all individuals had been off social assistance in the previous period. We would expect this rate to be higher than the observed entry rate because we have seen that for instance the incidence of health problems is much lower among non-recipients than among recipients. For the same reason, observed persistence rates will be higher than those predicted for the full sample. It should be noted nonetheless that the APE calculated on the basis of our estimations for the entire sample is substantially lower than the 'raw' difference in persistence and entry rates we observe in the data. The most substantial part of observed state dependence is thus due to observed and unobserved heterogeneity across individuals.

96. Looking at the impact of individual-level explanatory variables, we are surprised to find that, *ceteris paribus*, the sex of the respondent does not seem to be related to the risk of social assistance receipt as indicated by the insignificant coefficient on the female dummy. We further examine this issue below. The effect of age on the outcome variable is u-shaped with a minimum at age 43, which implies that young adults and more senior individuals have a higher probability of social assistance 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 18 years of education). As we would expect, good self-reported health is associated with a lower probability of social assistance benefit receipt. While the coefficient for poor health is insignificant, this result can be attributed to the high correlation of this variable with its strongly significant time-average, which is included among the Wooldridge controls. Even after controlling for personal characteristics, first- or second-generation migrants are significantly more likely to receive social assistance than natives.

97. If we turn to the controls for household-level characteristics, a first impression may be that these variables do not play a significant role in determining social assistance status since all of our family-type variables for instance are insignificant. Again, however, one needs to take into account the correlation of these variables with their respective time-averages. The results from our estimation suggest that couples with and without children are less likely to receive benefit payments than singles as indicated by negative, strongly significant coefficients for their time-averages. Our previous finding that single parents are more likely to receive social assistance benefits than singles without children (our base category) is also confirmed. Both household size and the dummy variable for a child aged six years and younger in the household enter positively, which suggests that having children especially of young age is associated with a higher risk of benefit receipt.

98. A look at our third set of variables shows that the partner's characteristics are highly relevant for determining an individual's social assistance receipt. The dummy variable for the partner's migrant status has the same sign as the respondent's variable indicating that individuals with a migrant partner are less likely to receive social assistance. Yet, this effect is only weakly significant. The partner's age displays a profile similar to that of the respondent, with the effect falling until age 37 and rising thereafter. The partner's education does not have a significant impact on the probability of social assistance benefit receipt in this specification. While also a good health status of the partner does not seem to have an impact, individuals whose partner suffers from poor health are more like to receive benefits.

99. Our model captures time trends in benefit receipt during our observation period through a series of year dummies. The year 1995 is not included in the estimation because as the first period of our sample it lacks a corresponding lag. The year 1996 is the base category against which the other time-effects are measured. We find that the large majority of year coefficients in our model is insignificant, which suggests that our model does relatively well at explaining the time trends observed in Section 6. We obtain a significantly positive coefficient for the year 2006 indicating *ceteris paribus* a higher probability of social assistance benefit receipt in the year after implementation of the Hartz reforms. Yet, this rise in the probability of benefit receipt is transitory since the estimated coefficients for the years 2007 and 2008 are very close to zero and insignificant. Moreover, the coefficient estimates for the years 2009 and 2010 are positive and highly significant suggesting a higher probability of benefit receipt during the financial crisis. Generally, however, we observe neither a clear time trend nor systematic differences in the probability of benefit receipt before and after the Hartz reforms.

100. The results for our month-of-interview dummy variables are omitted from the table for brevity. The set of dummies is however jointly significant at the 5% confidence level. This is mainly driven by significantly negative dummies for July and August and significantly positive dummies for November and December. This suggests that there is some seasonality in rates of benefit receipt, with lower receipt probabilities in summer and higher receipt probabilities in winter months.

101. The positive coefficient of the dummy for residence in Eastern Germany indicates that even once socio-economic characteristics are controlled for, the probability of social assistance receipt is higher for individuals living in the east. Finally, we surprisingly do not obtain a significant coefficient for our control of the state-level unemployment rate.

102. To check how sensitive results are to the selection of the variables used to control for initial conditions, we have re-estimated Model II extending the set of included controls for initial conditions. Specifically, we first follow Biewen (2009) by including interaction terms between the receipt status in the initial period and the longitudinal averages of all time-varying variables; alternatively, we then extend Model II by adding the initial values of all time-varying characteristics in addition to their time-averages as proposed by Rabe-Hesketh & Skrondal (2013). In neither of the two specifications, any of the additional regressors are significant at the 5%-level and estimated average predicted transition rates from both models are virtually identical to those from Model II. We have therefore chosen not to report these results and to use the simpler specification of Model II for the remainder of the analysis.

103. ***The impact of using sampling weights:*** As outlined in Section 5, a distinct feature of the SOEP is that immigrants, Eastern German residents, and high-income individuals are oversampled. We therefore

used sampling weights to obtain the estimation results presented in Table 3. To evaluate the importance of this weighting procedure for our findings, we have replicated the analysis from above ignoring any differences in sample selection probabilities between individuals. **Models I*** and **II*** presented in Table 4 correspond to simplified versions of Models I and II that have been estimated without sampling weights.

104. We find that our results are largely robust to the exclusion of sampling weights. Comparing the bottom panels of Tables 3 and 4, we see that dropping sampling weights leads to a slight reduction in both predicted entry and persistence rates. The average partial effect of the Wooldridge specification however remains virtually unchanged. While coefficient estimates differ somewhat in magnitude, the signs of significant coefficients in the two models are identical. Standard errors in the specifications that do not use weights are smaller by about one third compared to those of Models I and II. This suggests that the population groups that are oversampled in the SOEP contribute more to the precision of the estimation, because standard errors rise once they are given less relative weight.

105. The finding that overall results differ little between Models II and II* makes us confident that we do not have to worry too much about our results being affected by differences in sample selection probabilities. Since however the effect of using sampling weights on estimated standard errors is sizeable, we feel that the more conservative approach is to use the weights in the estimation. All results presented in the remainder of this paper however are robust to not using sampling weights in the estimations.

106. ***The impact of using unbalanced panels:*** A second concern we have relates to the fact that we use an unbalanced panel for the estimation, i.e. that not all individuals in our dataset are followed over the entire observation period. While the Wooldridge method does not rule out the use of unbalanced panels, it requires that sample attrition be unrelated to the outcome variable. This assumption however is questionable as some of the variables that affect the risk of social assistance benefit receipt are probably also determinants of item non-response or panel attrition.

107. The clear drawback of using a balanced sample from the SOEP for estimation however is that the implied sample restriction would bring about an enormous drop in the number of observations. Indeed, out of our total estimation sample of 17,733 individuals, only 585 persons are observed for the entire observation period from 1995 to 2011. A specification estimated on a fully balanced sample will thus not only yield results that are much less precise than those presented thus far; it is more importantly likely to suffer from strong sample selection bias.

108. An alternative would be to use a 'weakly balanced' panel, i.e a dataset in which all individuals are observed for the same *number* of annual waves, while the starting and end years of individuals' observation periods may differ. More specifically, we construct a dataset made up only of individuals who are observed for a minimum of ten years, from which each individual is dropped after those ten observations. Since individuals enter the sample in different time periods, the panel still extends over the complete observation period, with each individual contributing exactly ten annual observations. Using these sample selection criteria gives us a dataset of 3,710 individuals. This is still substantially fewer than for the unbalanced panel, but six times as many as for the fully balanced panel.

109. Estimation results from a fully balanced and a weakly balanced panel are reported in Table 5. We find that the estimated average partial effects for both specifications are higher than that reported for the Wooldridge specification in Table 3. The magnitude of this difference however is relatively small: We

recall that for Model II, we had estimated an average partial effect of 14.1 percentage points; the corresponding figures for the fully and weakly balanced panels are 17.7 and 14.3 percentage points, respectively. This difference in APEs is primarily due to lower average predicted entry rates in the two balanced panels.

110. Since the differences in results between the models are not very large but the downside of using balanced panels in terms of the implied reduction in sample size is substantial, we believe that use of unbalanced panels for estimation is justified.

8.2 Changes in state dependence over time

111. Given our finding of a strongly positive average partial effect of past social assistance benefit receipt of the probability of current benefit receipt, an important question is whether the degree of state dependence has been affected by the institutional reforms that were implemented in Germany during the observation period. Specifically, since one declared aim of the Hartz reforms was to improve labour market prospects of employable social assistance recipients, we might expect to find a variation in the structural component of state dependence in benefit receipt around the year 2005 when the reforms were implemented.

112. In this subsection, we follow Cappellari & Jenkins (2008) in their approach of adding to the standard specification a series of interaction terms between the lagged dependent variable and calendar-year dummies. Such a specification allows us to capture potential changes in the degree of state dependence over time and thus to explicitly compare the degree of state dependence before and after the Hartz reforms. The coefficients of all covariates in the model by contrast are assumed to be constant across time periods.

113. Table 6 presents results from three variants of the Wooldridge specification that allow the coefficient of the lagged dependent variable to vary over time. The model presented in column I is the most flexible one, interacting the lagged dependent variable with a dummy variable for each year in the observation period from 1997. As before, the year 1996 is the base period while 1995 as the initial period of the panel is not included in the estimation. To explicitly compare the degree of state dependence before and after the Hartz reforms, we present an alternative specification in column II that includes an interaction of the lagged dependent variable with a 'post-Hartz' dummy variable instead of the 13 yearly interaction terms. This 'post-Hartz' dummy is equal to one for the years 2005 to 2011 and zero otherwise. As usual, we keep the 13 *non-interacted* calendar-year dummies in the specification to control for a possible time trend in benefit receipt. Finally, column III of Table 6 restricts this specification further by also replacing these 13 year dummies by a single post-Hartz dummy.

114. In none of the three specifications, we find any evidence for a change in state dependence following the 2005 Hartz reforms. As shown in column I, among the calendar-year interactions added to the model, only the one for the year 2010 has a coefficient estimate that is significant at the 5%-level. The positive sign of this coefficient indicates that in that year, the level of structural state dependence was higher than in the base year 1996. Four additional coefficient estimates are weakly significant, two of which are for years after the Hartz reforms; all remaining coefficient estimates are insignificant. Standard errors for all interaction terms however are large.

115. The more restricted specifications confirm these results: In column II, we see that the estimate for the interaction term of the lagged dependent variable with a post-Hartz dummy is close to zero and again insignificant. Reassuringly, this coefficient is moreover estimated much more precisely than those of the calendar-year interaction terms shown in column I. We obtain nearly identical results when also replacing the non-interacted year dummies by a post-Hartz dummy, as shown in column III. Note that all of these results are robust to estimating the same specifications without sampling weights.

116. Based on the model estimates for the entire sample, there is thus no evidence for a change in structural state dependence through the Hartz reforms. In the following subsections, we disaggregate our sample to analyse the benefit dynamics for different subgroups separately, distinguishing individuals by sex, region of residence, and migrant status.

8.3 Benefit dynamics by sex

117. A remarkable result from Table 3 is that, *ceteris paribus*, a respondent's sex does not seem to matter for the probability of social assistance benefit receipt. One contributing factor to this finding is certainly that we have chosen to define our social assistance benefit variable at the household level. Females and males who live in couples are thus treated symmetrically. Yet, the probability of benefit receipt might still differ for single individuals, and the descriptive analysis provided in Section 6 indeed suggests that rates of benefit receipt are considerably higher for females than for males. To examine more closely potential differences in benefit dynamics by sex, we split our dataset and re-estimate our Wooldridge specification separately for females and males. The results from these estimations are reported in Tables 7 and 8, respectively.

118. Both average predicted entry and persistence rates are higher for females than for males. As summarised in Panel B of Tables 7 and 8, the average probability of social assistance benefit receipt conditional on receipt in the previous period for females is 22.1%, compared to a receipt probability of 6.9% for individuals who did not receive social assistance benefits in the previous year. For males, we calculate a receipt probability conditional on receipt (non-receipt) in the previous period of 18.3% (5.6%).

119. An important technical aspect to note is that these results reflect alone differences in *coefficient estimates* across sexes rather than differences in personal characteristics between the two subsamples. Indeed, to ensure the comparability of results, we have calculated predicted transition probabilities over the entire sample of males and females jointly rather than for the specific subgroup only. Differences in predicted transition rates between females and males thus do not reflect for instance the higher incidence of single parenthood among females, but only differences in the way observable characteristics affect the likelihood of benefit receipt.

120. A comparison of state dependence between females and males does not yield unambiguous results. We find that as a consequence of higher average predicted entry and persistence rates, also state dependence is higher in absolute terms for females (15.1 percentage points) than for males (12.6 percentage points). The benefit dynamics of both sexes however display the same degree of state dependence measured in relative terms, with benefit receipt in the previous period raising the probability of benefit receipt in the current period by a factor of 3.2.

121. The effects of covariates on the risk of benefit receipt are generally very similar for females and males, with a few differences that we however find difficult to interpret. The control for residence in Eastern Germany is highly significant for males but insignificant for females, which might imply that the labour market environment in Eastern Germany is more favourable to female employment. We also find that female singles with children have a higher probability of social assistance receipt whereas the corresponding coefficient for males is insignificant. Observed differences between males and females in the significance levels of the dummies for couples with and without children are not very meaningful for a social assistance variable that is defined at the household level. Finally, we find that the control for the partner's age is strongly significant for females but not for males. A potential explanation for this finding might be that for couples where the male partner remains to be the main breadwinner, his characteristics are important determinants of household income and hence social assistance benefit receipt status.

122. We obtain some more interesting differences across sexes when letting the degree of state dependence vary over the observation period. When again adding survey-year interactions to our specification in column II, we find that none of the 15 interacted year dummies is significant for females. However, we do observe a pattern of mostly negative coefficients until 2005 and mostly positive coefficients from 2006. For males, we do observe two significantly positive coefficients for 2002 and 2010 and a number of additional weakly significantly positive coefficients, however again no evidence of a structural break in 2005.

123. The pattern is somewhat reversed when we include the interaction with the post-Hartz dummy variable instead. We now observe a positive coefficient estimate for females that is significant at the 10% level (the p-value is 0.072), while the interaction term for males is estimated to be exactly zero. In an unweighted version of the same specifications (*not shown*), the post-Hartz interaction term however is much more precisely estimated and insignificant for both sexes.

124. Overall, we thus conclude that there is little variation in state dependence over the observation period, with very weak evidence for a possible increase in state dependence for females, but no change in state dependence for males.

8.4 Benefit dynamics in Western and Eastern Germany

125. Our findings thus far indicate that benefit dynamics differ considerably between Western and Eastern Germany. From the trends in benefit receipt rates discussed in Section 6, we recall that the frequency of social assistance benefit receipt is much higher in Eastern Germany, a result that we found to be driven by substantially higher entry rates into benefit receipt. Our analysis of benefit dynamics by sex then suggested that at least for males, the probability of benefit receipt is higher in Eastern Germany even conditional on household characteristics and the regional level of unemployment.

126. To identify what these patterns imply for the degree of state dependence and possibly identify differing effects of the Hartz reforms in the two parts of the country, we split our sample between Western and Eastern Germany based on residence in period *t*. Arguably, region of residence is endogenous to individual characteristics and past benefit receipt status and should therefore not be used as a criterion for splitting the sample. Indeed, the proportion of individuals who move from Eastern to Western Germany in our data is slightly higher among social assistance recipients than among non-recipients. However, only about 0.5% of individuals who live in Eastern Germany move to Western Germany in a given year, and the share

of individuals moving in the opposite direction is even lower. We therefore feel that we can safely neglect this issue. Again, we first present the results from our standard specification to then extend this specification by including interactions between the lagged dependent variable and dummies for the calendar year and the post-Hartz years, respectively. The results of these specifications are presented in Tables 9 and 10.

127. As reported in Panel B, both average predicted entry and persistence rates are substantially higher in Eastern Germany compared to Western Germany. Averaged again over all members in the sample, we calculate a predicted year-to-year entry rate in Eastern Germany of 13.4%, compared to only 4.9% in Western Germany. Similarly, the predicted persistence rate of 29.2% in the East is substantially higher than the 19.7% for the West. This implies that for given characteristics and conditional on the unemployment rate, an individual has a much higher predicted probability of receiving social assistance benefits when living in Eastern Germany.

128. The effect on state dependence of both higher average predicted entry and persistence rates in Eastern Germany is again ambiguous: We observe that in absolute terms, an individual who received social assistance benefits in the previous period is 15.8 percentage points more likely to also receive benefits again next period. This effect is one percentage point larger than the corresponding number for Western Germany. By contrast, a look at the predicted probability ratio shows that past benefit receipt leads to an increase in the probability of future benefit receipt by a factor of 2.2 in Eastern Germany, substantially less than the value of 4.0 that we calculate for Western Germany.

129. Coefficient estimates of the covariates are very similar for both parts of the country, with higher standard errors however in the specification for Eastern Germany due to the much smaller sample size. Note that we had to drop the dummy variables that control for the month of the interview from these specifications, because the number of individuals interviewed in the months October to December is very low for Eastern Germany. An effect of these variables could therefore not be identified.

130. The results from columns II and III show striking differences between the two countries in the development of state dependence over time. As reported in Table 9, we do not find any evidence for a change in state dependence over the observation period for Western Germany. In the specification with calendar-year interactions, we do not obtain coefficient estimates that are significantly different from zero for any of the years of the observation period. While the standard errors are large again in this specification, the result is confirmed when we include the post-Hartz dummy instead. As for the entire country, we estimate for Western Germany a coefficient that is very close to zero with a relatively small standard error as reported in column III. We thus conclude that estimated state dependence did not change in Western Germany during our 17-year observation period.

131. Interestingly, we reach a very different conclusion for Eastern Germany, where the level of state dependence increased in Eastern Germany after 2005. The specification presented in column II of Table 10 gives a series of positive coefficients for the survey-year interactions from 2006 while most of the coefficients before 2005 are negative. However, standard errors are large again and none of these coefficient estimates is statistically significant at the 5% level. When replacing the calendar-year interactions with our post-Hartz dummy, however, we estimate a positive coefficient that is statistically significant. We thus conclude that the 2005 Hartz reforms were followed by an increase in state dependence in Eastern Germany.

132. The finding of an *increase* in state dependence after 2005 at first seems puzzling. Even though we know little about the potential drivers of state dependence in social assistance benefit receipt, we would expect a labour market reform like the one introduced through the Hartz packages – which emphasized the employability of benefit recipients and strengthened the activating requirements of the benefit system – to have if anything an attenuating effect on the degree of state dependence in benefit receipt. The results presented in Table 10 however suggest the opposite.

133. The result of an increase in state dependence following the Hartz reforms is however consistent with a situation in which the reforms lowered the propensity to start receiving social assistance benefits without having an equally strong effect on raising predicted exit rates. Recall that the lagged dependent variable in our specifications simply measures the difference between conditional predicted persistence and entry rates As illustrated in Figure 9, observed entry rates into benefits dropped sharply in 2006, which led to a convergence of entry rates between Eastern and Western Germany. Similarly, we observe from Table 10 that the conditional probability of benefit receipt falls after 2006 when the significantly positive non-interacted calendar year effects disappear. The rise in state dependence in benefit receipt observed for Eastern Germany thus results from a fall in the predicted entry rate relative to the persistence rate in benefit receipt.

134. It is worth emphasising however that based on our model and the data we use we are not able to establish whether the link between the Hartz reforms and the observed rise in state dependence is causal, or through what channels such an effect might work. Instead, note for instance that the benefit entry rates presented for Eastern Germany in Figure 9 closely follow the pattern of regional unemployment rates shown in Figure 1, rising in the late 1990s and declining in the mid-2000s. The observed increase in state dependence in 2006 might thus potentially reflect more generally an improvement in the economic environment that is not adequately controlled for by the regional unemployment rate which we include in our model as a regressor.

8.5 Receipt dynamics of natives and migrants

135. In Section 6, we highlighted that there is a substantial immigrant-native gap in the frequency of social assistance benefit receipt, with rates of benefit receipt of first- or second generation migrants being about twice as high as those for non-immigrants. We also showed that this gap is primarily driven by much higher rates of entry into benefit receipt for migrants, while exit rates are very similar for both groups. In this section, we present results obtained from separately estimating our dynamic random-effects probit model for migrants and natives focusing again on potential differences in state dependence between these two groups.

136. The results presented in Tables 11 and 12 indicate that estimated state dependence in benefit receipt is substantially higher for migrants compared to natives both in absolute and in relative terms. As summarised in Panel B of Table 11, we calculate that benefit receipt in the previous period is *ceteris paribus* associated with an 11.9 percentage point increase in the probability of benefit receipt for natives. This compares to an effect of 23.2 percentage points for migrants as shown in Panel B of Table 12. Due to the much higher predicted rates of benefit receipt for migrants more generally, the difference in relative state dependence is much smaller. We find that past benefit receipt increases the probability of benefit receipt in the current period by a factor of 3.1 and 3.4 for natives and immigrants, respectively. Note again that the

reported differences in predicted transition rates result alone from differences in coefficient estimates in the two specifications, whose effect is averaged over the entire sample.

137. We find no systematic variation in state dependence over time in either of the two subsamples as illustrated by the results presented in column II of Tables 11 and 12. For natives, we estimate significantly positive coefficients for the interaction terms of the lagged dependent variable with the calendar-dummy for the years 1997 (negative), 2002, and 2007 (both positive), and another weakly positive coefficient for 2008. There is however little indication of a break in state dependence in the mid-2000s. Column III then reports an interaction term for the post-Hartz dummy that is close to zero and statistically insignificant. For migrants, standard errors are larger and we only find one single significant coefficient estimate for the interaction term of the lagged dependent variable with the dummy for the year 2010 (column II). Again, the post-Hartz dummy however is estimated more precisely and also not significantly different from zero.

138. Coefficient estimates for the covariates are generally very similar across the two subsamples with differences in significance levels being primarily due to the larger standard errors in the specification for immigrants.

8.6 The impact of changing the definition of the benefit variable

139. In the previous subsections, we have shown that there is substantial evidence for structural state dependence in Germany. While we found the degree of estimated state dependence to differ across recipient groups, variation over time in the level state dependence appears to be weak with the exception of Eastern Germany, where we observed an increase in state dependence after the 2005 Hartz reforms. All of these results were derived for the relatively 'broad' definition of our social assistance benefit variable proposed in Section 4, which takes into account receipt of Social Assistance, Unemployment Assistance / Unemployment Benefit II, or Housing Benefits.

140. One motive for having used this broad definition of our benefit variable was that it would allow us to consistently estimate one single specification for the complete observation period and to thus evaluate changes in benefit dynamics before and after the Hartz reforms. As implied by Figure 2, there is moreover a large overlap between the recipient populations of Housing Benefits on the one side, and Social Assistance or Unemployment Assistance / Unemployment Benefits II on the other. An exclusion of Housing Benefits from our definition of social assistance is therefore unlikely to much affect our findings.

141. In the final part of our analysis, we check the robustness of our main result to changing the definition of our social assistance benefit variable. We define a narrower social assistance variable by ignoring Housing Benefits and looking at the receipt of either Social Assistance or Unemployment Assistance / Unemployment Benefit II only. This approach resembles the one used in earlier studies by Riphahn & Wunder (2012) and Riphahn, Sander, & Wunder (2013). Similarly, we look at the dynamics of Housing Benefit receipt separately. A further subdivision of the narrow social assistance variable into the three different benefit programmes does not make sense for our purposes, because it would necessarily mean having to split up the observation period into the pre-Hartz and post-Hartz periods. Tables 13 and 14 present estimation results for our new, narrow definition of social assistance and for Housing Benefits, respectively

142. We find that as expected dropping Housing Benefits from our social assistance variable does not have a major impact on our estimation results. Average predicted entry and persistence rates for our new

social assistance variable are lower than those reported earlier. As summarized in Panel B of Table 13, we now obtain an average predicted entry rate of 4.8% compared to the 6.3% reported in Table 3, and an average predicted persistence rate of 18.5% rather than the 20.4% we calculated before. The finding of a lower average predicted probability of benefit receipt is of course not surprising: the narrower definition of the outcome variable means that individuals are now less likely to enter benefits and that benefit recipients are more likely to leave.

143. State dependence slightly drops in absolute terms from 14.1 percentage points in the standard specification to 13.7 for our narrow social assistance variable. By contrast, the drop in rates of entry and persistence translates into an increase in the predicted probability ratio from 3.3 for the standard benefit definition to 3.9 after dropping Housing Benefits. Overall, the magnitude of these changes however is very moderate.

144. As for our standard benefit definition, we do not find any variation in state dependence over the observation period for the narrower social assistance variable. A specification which interacts the lagged dependent variable with the calendar-year dummies again only gives insignificant coefficients (column II). If we instead add an interaction of the lagged dependent variable with a dummy for the post-Hartz years, we obtain once more an insignificant coefficient estimate that is relatively precisely estimated. Our earlier finding that the degree of state dependence in the entire sample has not changed over the observation period is thus robust to dropping Housing Benefits from our benefit variable.

145. For Housing Benefits, we find lower average predicted entry and persistence rates and thus also a weaker degree of state dependence in absolute terms as summarized in Panel B of Table 14. In relative terms, the level of state dependence is however again very similar to the one we calculated for the narrow social assistance variable.

146. Columns II and III provide evidence for a substantial drop in the level of state dependence in Housing Benefits during the observation period. We obtain significantly negative coefficient estimates for the interactions of the lagged dependent variable with the calendar-year dummy for 1997 and five out of the nine dummies after 2002. An additional five interaction terms have coefficient estimates that are significantly negative at the 10% level. We also obtain a significantly negative coefficient estimate for the interaction of the lagged dependent variable with the post-Hartz dummy as reported in column III. The pattern observed in the coefficient estimates for the interaction terms in column II however suggests that there has been a long-term decline in state dependence in Housing Benefits after the base year 1996, rather than a sudden drop in state dependence following the Hartz reforms.

9. Conclusion

147. In this study, we analyse the dynamics of social assistance benefit receipt in Germany using annual data from the German Socio-Economic Panel (SOEP) for the years 1995 to 2011. The case of Germany is of special interest for studying social assistance dynamics, because the country underwent far-reaching structural reforms in 2005, the so-called 'Hartz reforms'. Our work adds to the existing work on social assistance benefit dynamics in Germany by focusing explicitly on the issue of state dependence in benefit receipt and more specifically by examining potential differences in state dependence in the period before and after the 2005 Hartz reforms. Our approach moreover differs from the ones seen in previous

studies in that we look at a relatively broad range of benefit programmes, including Social Assistance ("*Sozialhilfe*"), Unemployment Assistance, Unemployment Benefit II, and Housing Benefits.

148. The rate of social assistance benefit receipt in Germany has been fairly stable at around 8% of working-age individuals in the late 1990s. From 2001, the frequency of benefit receipt has however risen to over 12% in 2006 and stayed elevated since. We find this rise in the benefit receipt rates to primarily have been driven by a drop in exit rates in 2001, while entry rates remained relatively steady. By contrast, the large East/West differences in the frequency of benefit receipt reflect much higher entry rates into benefits in Eastern Germany particularly in the late 1990s and early 2000s. Similarly, the higher receipt rate of migrants compared to natives is driven primarily by higher entry rates.

149. Our econometric analysis shows that there is substantial structural state dependence in benefit receipt. We estimate a series of dynamic random-effects probit models that allow us to control for the effects of persistent unobserved heterogeneity and account for the endogeneity of initial conditions using the method proposed by Wooldridge (2005). Results suggest that an individual in our sample is on average 3.3 times more likely to receive social assistance benefits at the interview date if she already received benefits at the last interview. This corresponds to an average partial effect of past benefit receipt of 14.1 percentage points. The level of state dependence differs by subsample and is highest for migrants, for whom we calculate an average partial effect of 23 percentage points. Structural state dependence however is substantially weaker than a simple comparison of observed persistence and entry rates would imply. This reaffirms that observed and unobserved individual characteristics are important determinants of benefit transitions.

150. An analysis of the variation in state dependence over time suggests that the Hartz reforms led to an increase in state dependence in Eastern Germany. We argue that this effect is likely to be the consequence of a drop in predicted entry rates into benefits in 2006 that was not matched by a corresponding rise in predicted exit rates from benefits. We do not find a comparable effect for Western Germany or any of the other subsamples we study. Also, there is no significant variation in state dependence over the observation period for the country overall.

151. Our finding of substantial state dependence in social assistance benefit receipt confirms similar results from comparable earlier studies for other countries. Indeed, the magnitude of the effect we find is strikingly similar to the one found by Cappellari & Jenkins (2008) for Britain, who – based on similar data and the same methodology – report a predicted probability ratio of 4 and an average partial effect of 14.4 percentage points. By contrast, our conclusions differ from those drawn in a comparable analysis for Germany by Wunder & Riphahn (2013), who also study state dependence in social assistance benefit receipt in Germany using data from the SOEP.

152. While we do not explicitly examine the reasons for this disagreement in findings, it is worth highlighting that sample selection criteria and the methodology used in the two studies differ in some important respects. First, Wunder & Riphahn limit their attention to receipt of post-Hartz Unemployment Benefits II and define their benefit variable at the *individual* level, both of which leads to lower rates of benefit receipt. Second, they restrict their analysis to Western Germany and drop disabled individuals from their sample. As we have shown, the magnitude of estimated state dependence is heterogeneous across recipient groups and the groups excluded by Wunder & Riphahn are ones for whom state dependence is likely to be stronger. We should thus be not surprised that Wunder & Riphahn find much weaker evidence of state dependence in benefit receipt than we do.

153. However, the difference in findings between the two studies also raises important methodological concerns. As emphasised by Wunder & Riphahn (2013), the interpretation of the state dependence parameter in dynamic discrete-choice models depends crucially on the definition of the competing state(s). Specifically, the rate of entry into social assistance benefits is affected by institutional factors outside the social assistance system like the design of employment protection legislation and the characteristics of the unemployment insurance system. Unless these factors are sufficiently accounted for, it remains unclear whether the state dependence estimated in models like the one we use in our analysis can be thought of as approximating the causal effect of past social assistance receipt rather than for instance to simply measure persistence in (un-)employment. Future research will therefore have to find better ways of accounting for these institutional factors when analysing state dependence in benefit receipt possibly by allowing for a richer set of competing states in the models estimated along the lines proposed by Wunder & Riphahn.

154. Finally, it is worth noting that our findings should not be understood a verdict on the success or failure of the Hartz reforms. As discussed, the rise in state dependence that we found for Eastern Germany seems to result from a fall in predicted entry rates without a corresponding drop in persistence rates. In principle, a reform that contributes to keeping individuals off benefits while doing little to raise exit rates might however be considered as having had beneficial effects even as it increases state dependence. A limitation of our analysis is moreover that we do not study any effects the reforms might have had on the relationship between personal characteristics and the probability of benefit receipt as measured by the coefficient estimates of the covariates. More specifically, we impose that the effect of variables like family composition or health status on the likelihood of benefit receipt has not been influenced by the Hartz reforms. Overall, our results however provide little evidence of structural differences in benefit dynamics between the pre- and post-Hartz period.

Table 3. coefficient estimates and APEs

Panel A – coefficient estimates

dependent variable: SA_t	(I) Model I: exogenous initial conditions		(II) Model II : Wooldridge method	
SA_{t-1}	1.725***	(0.058)	1.152***	(0.048)
individual characteristics				
female	0.028	(0.032)	-0.010	(0.041)
immigrant	0.239***	(0.057)	0.299***	(0.071)
age	-0.091***	(0.015)	-0.095***	(0.019)
age^2	0.106***	(0.018)	0.111***	(0.022)
education in years	-0.344***	(0.052)	-0.329***	(0.064)
education in years^2	0.010***	(0.002)	0.009***	(0.002)
good health	-0.115***	(0.029)	-0.083**	(0.038)
poor health	0.151***	(0.037)	0.038	(0.048)
household characteristics				
single with children	0.228***	(0.061)	0.010	(0.111)
couple without children	-0.146**	(0.073)	0.245*	(0.126)
couple with children	-0.144**	(0.067)	0.102	(0.126)
child aged 6 or younger	0.165***	(0.038)	0.098*	(0.057)
household size	0.128***	(0.017)	0.091***	(0.035)
partner characteristics				
immigrant	0.127**	(0.062)	0.133*	(0.075)
age	-0.020***	(0.005)	-0.023***	(0.006)
age^2	0.022***	(0.008)	0.031***	(0.010)
education in years	0.029**	(0.015)	0.026	(0.018)
education in years^2	-0.002***	(0.001)	-0.002*	(0.001)
good health	-0.006	(0.035)	-0.016	(0.046)
poor health	0.211***	(0.042)	0.152***	(0.053)
year dummies				
1997	-0.147*	(0.083)	-0.100	(0.091)
1998	-0.028	(0.073)	0.089	(0.086)
1999	-0.178***	(0.068)	-0.075	(0.079)
2000	-0.086	(0.078)	0.002	(0.088)
2001	-0.121*	(0.065)	-0.113	(0.074)
2002	-0.070	(0.068)	-0.063	(0.079)
2003	0.039	(0.066)	0.083	(0.078)
2004	0.015	(0.069)	0.107	(0.081)
2005	0.051	(0.072)	0.154*	(0.085)
2006	0.132*	(0.071)	0.280***	(0.089)
2007	-0.055	(0.074)	0.033	(0.088)
2008	0.047	(0.072)	0.052	(0.083)
2009	0.399***	(0.077)	0.410***	(0.093)
2010	0.340***	(0.079)	0.399***	(0.091)
2011	0.014	(0.085)	0.030	(0.101)
Wooldridge controls				
SA_0			1.417***	(0.077)
good health			-0.043	(0.077)
poor health			0.322***	(0.097)
single with children			0.340**	(0.149)

couple without children			-0.490***	(0.156)
couple with children			-0.352**	(0.166)
child aged 6 or younger			0.193**	(0.090)
household size			0.035	(0.044)
good health (partner)			0.074	(0.082)
poor health (partner)			0.222**	(0.105)
regional unemployment rate			0.027	(0.017)
constant	1.873***	(0.446)	1.398**	(0.543)
Eastern Germany	0.246***	(0.063)	0.244***	(0.080)
regional unemployment rate	0.047***	(0.006)	0.023	(0.016)
month-of-interview dummies	yes		yes	
σ_α	0.577		0.860	
ρ	0.249		0.425	
log Likelihood	-53,944,517		-51,369,911	
# of observations	100,434		100,434	
# of individuals	17,733		17,733	

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

Note: robust standard errors in parentheses. Estimates were obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. 'Wooldridge controls' include the social assistance benefit receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	Model I: exogenous initial conditions	**Model II : Wooldridge method**
average predicted entry rate	4.5%	6.3%
average predicted persistence rate	37.0%	20.4%
Average Partial Effect (APE)	32.5	14.1
Predicted Probability Ratio (PPR)	8.2	3.3

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A.

Table 4. coefficient estimates and APEs – unweighted

Panel A – coefficient estimates

dependent variable: SA_t	(I) Model I*: exogenous initial conditions		(II) Model II* : Wooldridge method	
SA_{t-1}	1.617***	(0.038)	1.178***	(0.033)
individual characteristics				
female	0.020	(0.023)	-0.010	(0.027)
immigrant	0.204***	(0.037)	0.244***	(0.044)
age	-0.080***	(0.011)	-0.070***	(0.013)
age2	0.089***	(0.013)	0.078***	(0.015)
education in years	-0.354***	(0.034)	-0.331***	(0.039)
education in years2	0.010***	(0.001)	0.009***	(0.002)
good health	-0.106***	(0.021)	-0.048*	(0.027)
poor health	0.181***	(0.027)	0.068**	(0.034)
household characteristics				
single with children	0.248***	(0.042)	0.056	(0.070)
couple without children	-0.175***	(0.046)	0.115	(0.075)
couple with children	-0.169***	(0.045)	-0.024	(0.077)
child aged 6 or younger	0.160***	(0.028)	0.078*	(0.040)
household size	0.125***	(0.012)	0.069***	(0.023)
partner characteristics				
immigrant	0.157***	(0.039)	0.155***	(0.046)
age	-0.017***	(0.003)	-0.019***	(0.004)
age2	0.020***	(0.006)	0.025***	(0.007)
education in years	0.031***	(0.010)	0.033***	(0.011)
education in years2	-0.003***	(0.001)	-0.003***	(0.001)
good health	-0.028	(0.024)	0.020	(0.031)
poor health	0.220***	(0.032)	0.141***	(0.040)
year dummies				
1997	-0.143***	(0.054)	-0.115**	(0.058)
1998	-0.037	(0.051)	0.023	(0.057)
1999	-0.156***	(0.052)	-0.108*	(0.059)
2000	-0.126**	(0.053)	-0.094	(0.059)
2001	-0.059	(0.046)	-0.077	(0.052)
2002	-0.072	(0.047)	-0.081	(0.053)
2003	0.066	(0.047)	0.080	(0.053)
2004	0.016	(0.048)	0.051	(0.055)
2005	0.074	(0.050)	0.117**	(0.058)
2006	0.097**	(0.049)	0.166***	(0.060)
2007	-0.054	(0.050)	-0.025	(0.059)
2008	0.083*	(0.050)	0.060	(0.058)
2009	0.461***	(0.051)	0.441***	(0.059)
2010	0.430***	(0.053)	0.439***	(0.061)
2011	0.155***	(0.059)	0.134**	(0.067)

Wooldridge controls				
SA0			1.207***	(0.049)
good health			-0.112**	(0.053)
poor health			0.307***	(0.068)
single with children			0.308***	(0.094)
couple without children			-0.375***	(0.090)
couple with children			-0.213**	(0.096)
child aged 6 or younger			0.285***	(0.063)
household size			0.045*	(0.027)
good health (partner)			-0.107*	(0.055)
poor health (partner)			0.205***	(0.077)
regional unemployment rate			0.012	(0.010)
constant	1.691***	(0.310)	0.985***	(0.368)
Eastern Germany	0.212***	(0.044)	0.191***	(0.054)
regional unemployment rate	0.058***	(0.004)	0.046***	(0.010)
month-of-interview dummies	yes		yes	
σ_α	0.606		0.794	
ρ	0.269		0.387	
log Likelihood	-15,969.78		-15,325.793	
# of observations	100,434		100,434	
# of individuals	17,733		17,733	

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

Note: robust standard errors in parentheses. All household, individual, and partner characteristics are lagged by one period. 'Wooldridge controls' include the social assistance benefit receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	Model I*: exogenous initial conditions	**Model II* : Wooldridge method**
average predicted entry rate	4.3%	5.4%
average predicted persistence rate	31.4%	19.7%
Average Partial Effect (APE)	27.2	14.2
Predicted Probability Ratio (PPR)	7.4	3.6

Note: calculations are based on the coefficient estimates presented in Panel A.

Table 5. coefficient estimates and APEs – balanced samples

Panel A – coefficient estimates

dependent variable: SA_t	(I) fully balanced panel		(II) weakly balanced panel	
SA_{t-1}	1.470***	(0.121)	1.237***	(0.077)
individual characteristics				
female	-0.017	(0.136)	0.092	(0.082)
immigrant	0.116	(0.209)	0.231*	(0.133)
age	-0.140	(0.093)	-0.108***	(0.037)
age^2	0.157	(0.098)	0.128***	(0.044)
education in years	-0.064	(0.192)	-0.357***	(0.106)
education in years^2	-0.003	(0.008)	0.009**	(0.004)
good health	-0.058	(0.120)	-0.086	(0.066)
poor health	0.414***	(0.135)	0.104	(0.075)
household characteristics				
single with children	-0.004	(0.244)	0.065	(0.162)
couple without children	0.082	(0.224)	0.168	(0.206)
couple with children	-0.260	(0.236)	0.153	(0.208)
child aged 6 or younger	0.089	(0.153)	0.112	(0.076)
household size	0.193**	(0.080)	0.090	(0.055)
partner characteristics				
immigrant	0.319	(0.207)	0.138	(0.127)
age	-0.044**	(0.018)	-0.012	(0.010)
age^2	0.046	(0.031)	0.014	(0.019)
education in years	0.151**	(0.066)	-0.001	(0.032)
education in years^2	-0.009**	(0.004)	-0.001	(0.002)
good health	-0.104	(0.133)	-0.073	(0.081)
poor health	0.347**	(0.140)	0.241***	(0.074)
year dummies				
1997	0.417	(0.284)	-0.156	(0.157)
1998	0.170	(0.254)	-0.027	(0.154)
1999	-0.377	(0.277)	-0.238*	(0.134)
2000	-0.026	(0.254)	-0.170	(0.147)
2001	-0.559*	(0.334)	-0.406***	(0.125)
2002	0.040	(0.339)	-0.239*	(0.136)
2003	-0.386	(0.309)	0.062	(0.128)
2004	0.404	(0.331)	-0.093	(0.138)
2005	0.444	(0.414)	0.124	(0.145)
2006	0.315	(0.394)	0.227	(0.160)
2007	0.003	(0.306)	-0.155	(0.152)
2008	0.029	(0.394)	-0.056	(0.149)
2009	0.226	(0.397)	0.255	(0.179)
2010	-0.095	(0.419)	0.267	(0.169)
2011	-0.210	(0.412)	-0.559	(0.371)
Wooldridge controls				
SA_0	0.849***	(0.186)	1.248***	(0.117)
good health	0.130	(0.330)	0.082	(0.148)
poor health	0.155	(0.376)	0.736***	(0.202)

single with children	0.203	(0.444)	0.009	(0.269)
couple without children	-0.146	(0.342)	-0.577**	(0.256)
couple with children	-0.691*	(0.412)	-1.068***	(0.301)
child aged 6 or younger	0.624	(0.438)	0.348*	(0.186)
household size	0.170	(0.108)	0.160**	(0.073)
good health (partner)	-0.252	(0.240)	0.258*	(0.153)
poor health (partner)	0.111	(0.446)	0.257	(0.206)
regional unemployment rate	0.084**	(0.036)	0.071***	(0.026)
constant	0.321	(2.014)	1.671	(1.016)
Eastern Germany	-0.389**	(0.183)	0.241*	(0.139)
regional unemployment rate	0.007	(0.030)	-0.020	(0.025)
month-of-interview dummies	no		no	
σ_α	0.613		0.838	
ρ	0.273		0.413	
log Likelihood	-2,352,666.2		-16,689,197	
# of observations	9360		37,100	
# of individuals	585		3,710	

*p<0.10, **p<0.05, ***p<0.01
note: robust standard errors are given in parentheses. All household, individual, and partner characteristics are lagged by one period. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. The fully balanced panel consists of all individuals who are observed for the entire 17-year observation period. The weakly balanced panel is made up of the first ten observations for each individual. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	fully balanced panel	**weakly balanced panel**
average predicted entry rate	3.2%	5.4%
average predicted persistence rate	20.9%	19.7%
Average Partial Effect (APE)	17.7	14.3
Predicted Probability Ratio (PPR)	6.5	3.6

note: calculations are based on the coefficient estimates presented in Panel A.

Table 6. coefficient estimates – specification with time-varying lags

dependent variable: SA_t	(I) standard specification + survey-year interactions		(II) standard specification + post-Hartz interaction + year dummies		(III) standard specification + post-Hartz interaction + post-Hartz dummy	
SA_{t-1}	1.034***	(0.144)	1.112***	(0.054)	1.117***	(0.054)
survey-year interactions						
SA_{t-1} * 1997	-0.344*	(0.179)				
SA_{t-1} * 1998	-0.199	(0.193)				
SA_{t-1} * 1999	0.181	(0.181)				
SA_{t-1} * 2000	0.146	(0.188)				
SA_{t-1} * 2001	0.151	(0.168)				
SA_{t-1} * 2002	0.318*	(0.178)				
SA_{t-1} * 2003	0.158	(0.179)				
SA_{t-1} * 2004	0.056	(0.180)				
SA_{t-1} * 2005	-0.117	(0.178)				
SA_{t-1} * 2006	0.316*	(0.176)				
SA_{t-1} * 2007	0.301*	(0.182)				
SA_{t-1} * 2008	0.239	(0.183)				
SA_{t-1} * 2009	-0.131	(0.189)				
SA_{t-1} * 2010	0.455**	(0.200)				
SA_{t-1} * 2011	0.122	(0.201)				
SA_{t-1} * post-Hartz dummy			0.090	(0.065)	0.060	(0.065)
individual characteristics						
female	-0.009	(0.041)	-0.009	(0.041)	-0.009	(0.041)
immigrant	0.299***	(0.071)	0.295***	(0.071)	0.292***	(0.071)
age	-0.097***	(0.019)	-0.095***	(0.019)	-0.095***	(0.018)
age^2	0.112***	(0.022)	0.110***	(0.022)	0.111***	(0.022)
education in years	-0.324***	(0.064)	-0.329***	(0.063)	-0.332***	(0.064)
education in years^2	0.009***	(0.002)	0.009***	(0.002)	0.010***	(0.002)
good health	-0.080**	(0.038)	-0.081**	(0.038)	-0.084**	(0.039)
poor health	0.042	(0.048)	0.044	(0.048)	0.053	(0.048)
household characteristics						
single with children	0.006	(0.111)	0.004	(0.110)	-0.007	(0.110)
couple without children	0.234*	(0.125)	0.226*	(0.125)	0.230*	(0.127)
couple with children	0.088	(0.125)	0.085	(0.125)	0.073	(0.126)
child aged 6 or younger	0.094*	(0.056)	0.101*	(0.056)	0.081	(0.056)
household size	0.095***	(0.035)	0.093***	(0.035)	0.098***	(0.035)
partner characteristics						
immigrant	0.138*	(0.075)	0.136*	(0.074)	0.133*	(0.074)
age	-0.023***	(0.006)	-0.023***	(0.006)	-0.023***	(0.006)
age^2	0.030***	(0.010)	0.031***	(0.010)	0.031***	(0.010)
education in years	0.028	(0.018)	0.027	(0.018)	0.025	(0.018)
education in years^2	-0.002*	(0.001)	-0.002*	(0.001)	-0.002*	(0.001)
good health	-0.008	(0.046)	-0.013	(0.046)	-0.019	(0.046)
poor health	0.156***	(0.053)	0.152***	(0.053)	0.156***	(0.053)
year dummies						
1997	-0.003	(0.093)	-0.100	(0.089)		

1998	0.132	(0.093)	0.083	(0.085)		
1999	-0.130	(0.092)	-0.085	(0.079)		
2000	-0.030	(0.100)	-0.001	(0.086)		
2001	-0.161*	(0.083)	-0.130*	(0.072)		
2002	-0.146	(0.091)	-0.064	(0.077)		
2003	0.049	(0.087)	0.079	(0.076)		
2004	0.099	(0.092)	0.103	(0.080)		
2005	0.201**	(0.091)	0.129	(0.085)		
2006	0.212**	(0.098)	0.255***	(0.091)		
2007	-0.067	(0.109)	-0.005	(0.091)		
2008	-0.019	(0.096)	0.019	(0.084)		
2009	0.438***	(0.101)	0.383***	(0.093)		
2010	0.276***	(0.104)	0.367***	(0.091)		
2011	0.003	(0.117)	-0.003	(0.103)		
post-Hartz dummy					0.173***	(0.036)
Wooldridge controls						
SA_0	1.417***	(0.077)	1.410***	(0.077)	1.392***	(0.075)
good health	-0.044	(0.076)	-0.046	(0.076)	-0.042	(0.076)
poor health	0.317***	(0.096)	0.314***	(0.097)	0.295***	(0.096)
single with children	0.344**	(0.150)	0.342**	(0.149)	0.355**	(0.148)
couple without children	-0.482***	(0.156)	-0.468***	(0.156)	-0.485***	(0.157)
couple with children	-0.330**	(0.166)	-0.330**	(0.165)	-0.317*	(0.166)
child aged 6 or younger	0.189**	(0.089)	0.188**	(0.090)	0.214**	(0.089)
household size	0.030	(0.044)	0.031	(0.044)	0.023	(0.044)
good health (partner)	0.064	(0.082)	0.072	(0.081)	0.076	(0.081)
poor health (partner)	0.214**	(0.104)	0.221**	(0.104)	0.212**	(0.103)
regional unemployment rate	0.030*	(0.017)	0.028*	(0.017)	0.034***	(0.013)
constant	1.402***	(0.541)	1.392***	(0.538)	1.425***	(0.535)
Eastern Germany	0.247***	(0.080)	0.245***	(0.079)	0.259***	(0.078)
regional unemployment rate	0.021	(0.016)	0.022	(0.016)	0.014	(0.011)
month-of-interview dummies	no		no		no	
σ_a	0.855		0.853		0.845	
ρ	0.422		0.421		0.421	
log Likelihood	-51,292,521		-51,443,558		-51,443,558	
# of observations	100,434		100,434		100,434	
# of individuals	17,733		17,733		17,733	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Table 7. coefficient estimates and APE – females

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.184***	(0.065)	1.179***	(0.194)	1.111***	(0.074)
survey-year interactions						
SA_{t-1} * 1997			-0.275	(0.240)		
SA_{t-1} * 1998			-0.409	(0.256)		
SA_{t-1} * 1999			0.055	(0.246)		
SA_{t-1} * 2000			0.207	(0.252)		
SA_{t-1} * 2001			-0.059	(0.226)		
SA_{t-1} * 2002			0.140	(0.240)		
SA_{t-1} * 2003			-0.057	(0.240)		
SA_{t-1} * 2004			-0.249	(0.241)		
SA_{t-1} * 2005			-0.185	(0.241)		
SA_{t-1} * 2006			0.179	(0.237)		
SA_{t-1} * 2007			0.250	(0.245)		
SA_{t-1} * 2008			0.292	(0.248)		
SA_{t-1} * 2009			-0.190	(0.254)		
SA_{t-1} * 2010			0.223	(0.267)		
SA_{t-1} * 2011			0.006	(0.275)		
SA_{t-1} * post-Hartz dummy					0.161*	(0.090)
individual characteristics						
immigrant	0.207**	(0.092)	0.208**	(0.092)	0.206**	(0.091)
age	-0.090***	(0.025)	-0.091***	(0.025)	-0.089***	(0.025)
age^2	0.098***	(0.030)	0.099***	(0.030)	0.098***	(0.030)
education in years	-0.388***	(0.088)	-0.386***	(0.088)	-0.384***	(0.087)
education in years^2	0.012***	(0.003)	0.012***	(0.003)	0.012***	(0.003)
good health	-0.072	(0.051)	-0.072	(0.051)	-0.073	(0.051)
poor health	-0.025	(0.064)	-0.023	(0.064)	-0.023	(0.063)
household characteristics						
single with children	-0.090	(0.140)	-0.099	(0.142)	-0.094	(0.140)
couple without children	0.168	(0.155)	0.172	(0.157)	0.170	(0.155)
couple with children	0.038	(0.159)	0.031	(0.160)	0.034	(0.159)
child aged 6 or younger	0.115	(0.075)	0.114	(0.075)	0.123	(0.075)
household size	0.100**	(0.049)	0.102**	(0.049)	0.100**	(0.049)
partner characteristics						
immigrant	0.168	(0.107)	0.173	(0.107)	0.168	(0.107)
age	-0.032***	(0.008)	-0.032***	(0.008)	-0.032***	(0.008)
age^2	0.046***	(0.013)	0.046***	(0.013)	0.046***	(0.013)
education in years	0.037	(0.024)	0.038	(0.024)	0.037	(0.024)
education in years^2	-0.003*	(0.001)	-0.003*	(0.001)	-0.003*	(0.001)
good health	-0.048	(0.062)	-0.043	(0.063)	-0.046	(0.062)
poor health	0.122*	(0.071)	0.127*	(0.070)	0.119*	(0.071)
year dummies						
1997	-0.089	(0.117)	-0.011	(0.131)	-0.086	(0.114)
1998	0.092	(0.116)	0.184	(0.126)	0.090	(0.114)

1999	-0.054	(0.107)	-0.067	(0.126)	-0.049	(0.105)
2000	0.039	(0.116)	-0.015	(0.137)	0.037	(0.114)
2001	-0.109	(0.097)	-0.083	(0.110)	-0.102	(0.096)
2002	-0.004	(0.104)	-0.043	(0.120)	0.001	(0.102)
2003	0.168	(0.102)	0.186	(0.116)	0.168*	(0.100)
2004	0.148	(0.110)	0.229*	(0.124)	0.153	(0.108)
2005	0.210*	(0.114)	0.272**	(0.124)	0.169	(0.114)
2006	0.329***	(0.120)	0.290**	(0.131)	0.290**	(0.122)
2007	0.045	(0.113)	-0.056	(0.145)	-0.008	(0.117)
2008	0.141	(0.108)	0.044	(0.132)	0.096	(0.109)
2009	0.447***	(0.119)	0.500***	(0.131)	0.411***	(0.119)
2010	0.389***	(0.119)	0.328**	(0.140)	0.347***	(0.120)
2011	0.116	(0.135)	0.124	(0.158)	0.071	(0.139)
Wooldridge controls						
SA_0	1.338***	(0.106)	1.340***	(0.107)	1.338***	(0.105)
good health	-0.094	(0.103)	-0.092	(0.103)	-0.092	(0.102)
poor health	0.250*	(0.130)	0.247*	(0.130)	0.245*	(0.129)
single with children	0.526***	(0.186)	0.540***	(0.188)	0.526***	(0.185)
couple without children	-0.378*	(0.210)	-0.387*	(0.211)	-0.379*	(0.209)
couple with children	-0.248	(0.220)	-0.234	(0.221)	-0.241	(0.219)
child aged 6 or younger	0.208*	(0.118)	0.205*	(0.118)	0.197*	(0.117)
household size	0.011	(0.060)	0.008	(0.060)	0.010	(0.060)
good health (partner)	0.144	(0.115)	0.133	(0.116)	0.141	(0.114)
poor health (partner)	0.456***	(0.139)	0.444***	(0.139)	0.452***	(0.138)
regional unemployment rate	0.043*	(0.024)	0.043*	(0.024)	0.042*	(0.024)
constant	1.582**	(0.716)	1.578**	(0.721)	1.585**	(0.712)
Eastern Germany	0.143	(0.110)	0.143	(0.109)	0.142	(0.109)
regional unemployment rate	0.023	(0.023)	0.023	(0.023)	0.023	(0.023)
month-of-interview dummies	yes		yes		yes	
σ_a	0.863		0.863		0.853	
ρ	0.427		0.427		0.421	
log Likelihood	-26,811,725		-26,726,843		-26,799,142	
# of observations	51,707		51,707		51,707	
# of individuals	9,071		9,071		9,071	

*p<0.10, **p<0.05, ***p<0.01
Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	6.9%
average predicted persistence rate	22.1%
Average Partial Effect (APE)	15.1
Predicted Probability Ratio (PPR)	3.2

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (of females and males).

Table 8. coefficient estimates and APE – males

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.100***	(0.070)	0.838***	(0.213)	1.100***	(0.079)
survey-year interactions						
SA_{t-1} * 1997			-0.388	(0.277)		
SA_{t-1} * 1998			0.061	(0.289)		
SA_{t-1} * 1999			0.373	(0.264)		
SA_{t-1} * 2000			0.117	(0.277)		
SA_{t-1} * 2001			0.433*	(0.252)		
SA_{t-1} * 2002			0.520**	(0.263)		
SA_{t-1} * 2003			0.421	(0.271)		
SA_{t-1} * 2004			0.463*	(0.265)		
SA_{t-1} * 2005			-0.037	(0.265)		
SA_{t-1} * 2006			0.470*	(0.261)		
SA_{t-1} * 2007			0.370	(0.271)		
SA_{t-1} * 2008			0.182	(0.269)		
SA_{t-1} * 2009			-0.076	(0.284)		
SA_{t-1} * 2010			0.746**	(0.297)		
SA_{t-1} * 2011			0.308	(0.296)		
SA_{t-1} * post-Hartz dummy					-0.000	(0.096)
individual characteristics						
immigrant	0.392***	(0.105)	0.389***	(0.103)	0.392***	(0.105)
age	-0.094***	(0.030)	-0.098***	(0.030)	-0.094***	(0.030)
age^2	0.114***	(0.035)	0.118***	(0.035)	0.114***	(0.035)
education in years	-0.286***	(0.094)	-0.275***	(0.093)	-0.286***	(0.094)
education in years^2	0.007**	(0.004)	0.007*	(0.004)	0.007**	(0.004)
good health	-0.095	(0.058)	-0.097*	(0.058)	-0.095	(0.058)
poor health	0.119	(0.072)	0.120*	(0.070)	0.119	(0.072)
household characteristics						
single with children	0.048	(0.192)	0.071	(0.189)	0.048	(0.192)
couple without children	0.290	(0.206)	0.309	(0.205)	0.290	(0.206)
couple with children	0.140	(0.200)	0.157	(0.198)	0.140	(0.200)
child aged 6 or younger	0.076	(0.086)	0.070	(0.083)	0.076	(0.086)
household size	0.069	(0.047)	0.067	(0.047)	0.069	(0.047)
partner characteristics						
immigrant	0.093	(0.099)	0.094	(0.097)	0.093	(0.099)
age	-0.010	(0.009)	-0.010	(0.009)	-0.010	(0.009)
age^2	0.013	(0.015)	0.011	(0.015)	0.013	(0.015)
education in years	0.013	(0.028)	0.013	(0.028)	0.013	(0.028)
education in years^2	-0.001	(0.002)	-0.001	(0.002)	-0.001	(0.002)
good health	0.009	(0.067)	0.014	(0.066)	0.009	(0.067)
poor health	0.170**	(0.079)	0.170**	(0.080)	0.170**	(0.079)
year dummies						
1997	-0.112	(0.137)	0.004	(0.131)	-0.112	(0.137)
1998	0.089	(0.125)	0.090	(0.135)	0.089	(0.125)
1999	-0.094	(0.117)	-0.174	(0.130)	-0.095	(0.116)
2000	-0.033	(0.133)	-0.038	(0.145)	-0.033	(0.133)

2001	-0.114	(0.113)	-0.201	(0.125)	-0.114	(0.112)
2002	-0.122	(0.119)	-0.234*	(0.135)	-0.122	(0.118)
2003	-0.007	(0.117)	-0.083	(0.128)	-0.007	(0.117)
2004	0.065	(0.118)	-0.027	(0.131)	0.065	(0.118)
2005	0.094	(0.126)	0.139	(0.133)	0.094	(0.129)
2006	0.219*	(0.128)	0.140	(0.140)	0.219*	(0.133)
2007	0.028	(0.134)	-0.049	(0.159)	0.028	(0.140)
2008	-0.047	(0.126)	-0.063	(0.141)	-0.047	(0.130)
2009	0.369**	(0.145)	0.391**	(0.154)	0.369**	(0.145)
2010	0.409***	(0.135)	0.242	(0.149)	0.409***	(0.138)
2011	-0.066	(0.150)	-0.128	(0.170)	-0.066	(0.152)
Wooldridge controls						
SA_0	1.512***	(0.112)	1.518***	(0.112)	1.512***	(0.112)
good health	0.025	(0.114)	0.032	(0.113)	0.025	(0.114)
poor health	0.391***	(0.145)	0.391***	(0.142)	0.391***	(0.146)
single with children	-0.042	(0.249)	-0.075	(0.248)	-0.042	(0.249)
couple without children	-0.682***	(0.233)	-0.706***	(0.233)	-0.682***	(0.233)
couple with children	-0.560**	(0.249)	-0.578**	(0.249)	-0.560**	(0.249)
child aged 6 or younger	0.189	(0.135)	0.188	(0.133)	0.189	(0.135)
household size	0.076	(0.063)	0.079	(0.062)	0.076	(0.063)
good health (partner)	0.011	(0.116)	0.005	(0.115)	0.011	(0.116)
poor health (partner)	-0.024	(0.157)	-0.029	(0.156)	-0.024	(0.157)
regional unemployment rate	0.008	(0.022)	0.011	(0.022)	0.008	(0.022)
constant	1.207	(0.819)	1.255	(0.813)	1.207	(0.819)
Eastern Germany	0.388***	(0.119)	0.390***	(0.118)	0.388***	(0.119)
regional unemployment rate	0.023	(0.021)	0.019	(0.021)	0.023	(0.021)
month-of-interview dummies	yes		yes		yes	
σ_a	0.860		0.848		0.860	
ρ	0.425		0.418		0.425	
log Likelihood	-24,398,784		-24,269,910		-24,398,788	
# of observations	48,727		48,727		48,727	
# of individuals	8,662		8,662		8,662	

*p<0.10, **p<0.05, ***p<0.01
Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	5.6%
average predicted persistence rate	18.3%
Average Partial Effect (APE)	12.6
Predicted Probability Ratio (PPR)	3.2

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (of females and males).

Table 9. coefficient estimates and APE –Western Germany

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.246***	(0.061)	1.106***	(0.176)	1.233***	(0.068)
survey-year interactions						
SA_{t-1} * 1997			-0.257	(0.219)		
SA_{t-1} * 1998			-0.161	(0.222)		
SA_{t-1} * 1999			0.366*	(0.218)		
SA_{t-1} * 2000			0.383*	(0.228)		
SA_{t-1} * 2001			0.242	(0.202)		
SA_{t-1} * 2002			0.281	(0.219)		
SA_{t-1} * 2003			0.227	(0.218)		
SA_{t-1} * 2004			-0.025	(0.218)		
SA_{t-1} * 2005			-0.043	(0.219)		
SA_{t-1} * 2006			0.395*	(0.215)		
SA_{t-1} * 2007			0.265	(0.221)		
SA_{t-1} * 2008			0.188	(0.220)		
SA_{t-1} * 2009			-0.291	(0.235)		
SA_{t-1} * 2010			0.416*	(0.240)		
SA_{t-1} * 2011			0.019	(0.245)		
SA_{t-1} * post-Hartz dummy					0.028	(0.080)
individual characteristics						
female	-0.009	(0.047)	-0.007	(0.048)	-0.009	(0.047)
immigrant	0.304***	(0.073)	0.309***	(0.073)	0.304***	(0.073)
age	-0.086***	(0.022)	-0.089***	(0.022)	-0.086***	(0.022)
age^2	0.102***	(0.026)	0.105***	(0.026)	0.102***	(0.026)
education in years	-0.336***	(0.066)	-0.333***	(0.067)	-0.335***	(0.066)
education in years^2	0.010***	(0.003)	0.010***	(0.003)	0.010***	(0.003)
good health	-0.083*	(0.046)	-0.083*	(0.046)	-0.083*	(0.046)
poor health	0.042	(0.055)	0.039	(0.054)	0.043	(0.055)
household characteristics						
single with children	0.108	(0.144)	0.102	(0.146)	0.107	(0.144)
couple without children	0.283**	(0.141)	0.288**	(0.142)	0.284**	(0.141)
couple with children	0.124	(0.148)	0.132	(0.149)	0.123	(0.148)
child aged 6 or younger	0.168***	(0.064)	0.154**	(0.063)	0.169***	(0.063)
household size	0.074*	(0.041)	0.075*	(0.041)	0.074*	(0.041)
partner characteristics						
immigrant	0.131*	(0.076)	0.134*	(0.076)	0.131*	(0.076)
age	-0.021***	(0.007)	-0.021***	(0.007)	-0.021***	(0.007)
age^2	0.027**	(0.011)	0.027**	(0.011)	0.027**	(0.011)
education in years	0.013	(0.020)	0.014	(0.020)	0.013	(0.020)
education in years^2	-0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)
good health	-0.016	(0.056)	-0.013	(0.056)	-0.016	(0.056)
poor health	0.158**	(0.062)	0.161***	(0.062)	0.158**	(0.062)
year dummies						
1997	0.048	(0.108)	0.116	(0.110)	0.048	(0.107)
1998	0.227**	(0.101)	0.275**	(0.112)	0.227**	(0.101)
1999	0.033	(0.091)	-0.059	(0.109)	0.034	(0.091)

2000	0.000	(0.105)	-0.090	(0.128)	0.001	(0.105)
2001	-0.134	(0.088)	-0.187*	(0.100)	-0.133	(0.088)
2002	-0.098	(0.099)	-0.164	(0.114)	-0.097	(0.098)
2003	0.067	(0.091)	0.024	(0.102)	0.067	(0.091)
2004	0.135	(0.096)	0.159	(0.106)	0.136	(0.096)
2005	0.190**	(0.096)	0.220**	(0.105)	0.183*	(0.097)
2006	0.417***	(0.105)	0.346***	(0.118)	0.411***	(0.109)
2007	0.133	(0.106)	0.067	(0.128)	0.125	(0.111)
2008	0.020	(0.099)	-0.025	(0.114)	0.013	(0.101)
2009	0.391***	(0.112)	0.447***	(0.119)	0.386***	(0.112)
2010	0.379***	(0.106)	0.284**	(0.120)	0.372***	(0.108)
2011	0.015	(0.120)	0.029	(0.134)	0.008	(0.123)
Wooldridge controls						
SA_0	1.343***	(0.096)	1.361***	(0.097)	1.343***	(0.096)
good health	-0.031	(0.091)	-0.028	(0.091)	-0.031	(0.090)
poor health	0.351***	(0.113)	0.358***	(0.112)	0.350***	(0.113)
single with children	0.212	(0.180)	0.226	(0.182)	0.212	(0.180)
couple without children	-0.611***	(0.174)	-0.625***	(0.175)	-0.611***	(0.174)
couple with children	-0.442**	(0.189)	-0.446**	(0.190)	-0.440**	(0.189)
child aged 6 or younger	0.096	(0.100)	0.104	(0.101)	0.094	(0.101)
household size	0.057	(0.051)	0.057	(0.051)	0.057	(0.051)
good health (partner)	0.173*	(0.097)	0.164*	(0.097)	0.173*	(0.096)
poor health (partner)	0.292**	(0.121)	0.289**	(0.121)	0.291**	(0.121)
regional unemployment rate	0.090***	(0.025)	0.094***	(0.025)	0.090***	(0.025)
constant	1.131*	(0.605)	1.174*	(0.611)	1.132*	(0.604)
regional unemployment rate	-0.040	(0.024)	-0.043*	(0.025)	-0.040	(0.024)
month-of-interview dummies	no		no		no	
σ_a	0.817		0.824		0.816	
ρ	0.401		0.404		0.400	
log Likelihood	-38,712,575		-38,575,138		-38,712,069	
# of observations	79,829		79,829		79,829	
# of individuals	14,096		14,096		14,096	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	4.9%
average predicted persistence rate	19.7%
Average Partial Effect (APE)	14.8
Predicted Probability Ratio (PPR)	4.0

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (Western and Eastern Germany).

Table 10. coefficient estimates and APE – Eastern Germany

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	0.964***	(0.071)	0.900***	(0.263)	0.844***	(0.086)
survey-year interactions						
SA_{t-1} * 1997			-0.401	(0.285)		
SA_{t-1} * 1998			-0.310	(0.388)		
SA_{t-1} * 1999			-0.371	(0.319)		
SA_{t-1} * 2000			-0.428	(0.343)		
SA_{t-1} * 2001			-0.101	(0.304)		
SA_{t-1} * 2002			0.408	(0.320)		
SA_{t-1} * 2003			-0.090	(0.322)		
SA_{t-1} * 2004			0.319	(0.321)		
SA_{t-1} * 2005			-0.348	(0.316)		
SA_{t-1} * 2006			0.101	(0.308)		
SA_{t-1} * 2007			0.449	(0.326)		
SA_{t-1} * 2008			0.387	(0.327)		
SA_{t-1} * 2009			0.354	(0.333)		
SA_{t-1} * 2010			0.630*	(0.362)		
SA_{t-1} * 2011			0.431	(0.352)		
SA_{t-1} * post-Hartz dummy					0.283**	(0.111)
individual characteristics						
female	0.023	(0.069)	0.024	(0.067)	0.025	(0.068)
immigrant	0.417	(0.265)	0.416	(0.267)	0.413	(0.266)
age	-0.119***	(0.034)	-0.110***	(0.033)	-0.116***	(0.034)
age^2	0.136***	(0.041)	0.125***	(0.040)	0.132***	(0.040)
education in years	-0.541***	(0.189)	-0.486***	(0.184)	-0.528***	(0.186)
education in years^2	0.015**	(0.007)	0.013*	(0.007)	0.014**	(0.007)
good health	-0.075	(0.065)	-0.066	(0.065)	-0.070	(0.065)
poor health	0.030	(0.096)	0.031	(0.093)	0.029	(0.095)
household characteristics						
single with children	-0.232	(0.160)	-0.238	(0.155)	-0.240	(0.159)
couple without children	0.069	(0.182)	0.088	(0.176)	0.071	(0.181)
couple with children	0.063	(0.187)	0.041	(0.180)	0.044	(0.185)
child aged 6 or younger	-0.113	(0.115)	-0.089	(0.115)	-0.104	(0.114)
household size	0.154**	(0.063)	0.144**	(0.062)	0.150**	(0.063)
partner characteristics						
immigrant	0.737***	(0.268)	0.728***	(0.270)	0.736***	(0.269)
age	-0.026**	(0.011)	-0.026**	(0.011)	-0.026**	(0.010)
age^2	0.033**	(0.017)	0.033**	(0.017)	0.034**	(0.017)
education in years	0.109***	(0.037)	0.110***	(0.036)	0.109***	(0.036)
education in years^2	-0.008***	(0.002)	-0.008***	(0.002)	-0.008***	(0.002)
good health	0.027	(0.075)	0.030	(0.074)	0.027	(0.074)
poor health	0.137	(0.100)	0.136	(0.100)	0.135	(0.100)
year dummies						
1997	-0.368**	(0.151)	-0.219	(0.150)	-0.358**	(0.145)
1998	0.065	(0.159)	0.117	(0.169)	0.053	(0.156)
1999	-0.113	(0.163)	0.011	(0.178)	-0.107	(0.160)

2000	0.228	(0.161)	0.317*	(0.178)	0.219	(0.157)
2001	0.009	(0.143)	0.060	(0.158)	0.020	(0.139)
2002	0.184	(0.140)	0.064	(0.163)	0.186	(0.137)
2003	0.314**	(0.149)	0.342**	(0.168)	0.315**	(0.146)
2004	0.414**	(0.162)	0.311*	(0.189)	0.418***	(0.159)
2005	0.394**	(0.167)	0.520***	(0.183)	0.301*	(0.170)
2006	0.507***	(0.164)	0.472**	(0.184)	0.422**	(0.165)
2007	0.111	(0.157)	-0.085	(0.200)	0.003	(0.160)
2008	0.142	(0.139)	0.006	(0.170)	0.048	(0.141)
2009	0.232	(0.154)	0.123	(0.192)	0.146	(0.155)
2010	0.280*	(0.153)	0.058	(0.174)	0.188	(0.152)
2011	-0.191	(0.163)	-0.379*	(0.207)	-0.298*	(0.164)
Wooldridge controls						
SA_0	1.470***	(0.115)	1.449***	(0.113)	1.465***	(0.113)
good health	-0.042	(0.123)	-0.046	(0.121)	-0.048	(0.122)
poor health	0.199	(0.177)	0.216	(0.172)	0.204	(0.175)
single with children	0.747***	(0.232)	0.706***	(0.223)	0.730***	(0.229)
couple without children	-0.011	(0.219)	-0.051	(0.210)	-0.023	(0.216)
couple with children	0.011	(0.253)	0.014	(0.243)	0.013	(0.249)
child aged 6 or younger	0.507***	(0.182)	0.500***	(0.181)	0.500***	(0.181)
household size	-0.070	(0.080)	-0.055	(0.079)	-0.062	(0.079)
good health (partner)	-0.253*	(0.135)	-0.247*	(0.132)	-0.248*	(0.133)
poor health (partner)	0.042	(0.195)	0.051	(0.190)	0.053	(0.192)
regional unemployment rate	-0.035*	(0.021)	-0.037*	(0.021)	-0.035*	(0.021)
constant	5.410***	(1.471)	4.862***	(1.442)	5.657***	(0.954)
regional unemployment rate	-0.009	(0.023)	-0.007	(0.023)	-0.008	(0.023)
month-of-interview dummies	no		no		no	
σ_a	0.911		0.882		0.895	
ρ	0.454		0.437		0.445	
log Likelihood	-12,189,480		-12,083,856		-12,171,497	
# of observations	20,605		20,605		20,605	
# of individuals	3,779		3,779		3,779	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	13.4%
average predicted persistence rate	29.2%
Average Partial Effect (APE)	15.8
Predicted Probability Ratio (PPR)	2.2

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (Western and Eastern Germany).

Table 11. coefficient estimates and APE – natives

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.122***	(0.056)	0.943***	(0.174)	1.093***	(0.065)
survey-year interactions						
SA_{t-1} * 1997			-0.458**	(0.226)		
SA_{t-1} * 1998			-0.129	(0.251)		
SA_{t-1} * 1999			0.298	(0.225)		
SA_{t-1} * 2000			0.117	(0.230)		
SA_{t-1} * 2001			0.246	(0.208)		
SA_{t-1} * 2002			0.433**	(0.214)		
SA_{t-1} * 2003			0.181	(0.217)		
SA_{t-1} * 2004			0.301	(0.211)		
SA_{t-1} * 2005			-0.047	(0.215)		
SA_{t-1} * 2006			0.265	(0.209)		
SA_{t-1} * 2007			0.456**	(0.214)		
SA_{t-1} * 2008			0.396*	(0.220)		
SA_{t-1} * 2009			-0.009	(0.220)		
SA_{t-1} * 2010			0.320	(0.233)		
SA_{t-1} * 2011			0.100	(0.235)		
SA_{t-1} * post-Hartz dummy					0.064	(0.076)
individual characteristics						
female	0.006	(0.049)	0.005	(0.049)	0.005	(0.049)
age	-0.083***	(0.022)	-0.085***	(0.022)	-0.083***	(0.022)
age^2	0.094***	(0.027)	0.096***	(0.027)	0.094***	(0.027)
education in years	-0.570***	(0.094)	-0.559***	(0.094)	-0.567***	(0.094)
education in years^2	0.018***	(0.004)	0.017***	(0.004)	0.018***	(0.004)
good health	-0.086*	(0.046)	-0.084*	(0.046)	-0.085*	(0.046)
poor health	0.009	(0.060)	0.010	(0.060)	0.009	(0.060)
household characteristics						
single with children	0.105	(0.110)	0.108	(0.109)	0.105	(0.110)
couple without children	0.210	(0.152)	0.213	(0.152)	0.211	(0.152)
couple with children	0.097	(0.138)	0.099	(0.138)	0.096	(0.138)
child aged 6 or younger	-0.025	(0.071)	-0.019	(0.070)	-0.022	(0.071)
household size	0.122***	(0.039)	0.124***	(0.039)	0.122***	(0.039)
partner characteristics						
immigrant	0.168	(0.133)	0.171	(0.134)	0.167	(0.133)
age	-0.024***	(0.007)	-0.024***	(0.007)	-0.024***	(0.007)
age^2	0.034***	(0.012)	0.034***	(0.012)	0.035***	(0.012)
education in years	0.028	(0.022)	0.027	(0.022)	0.027	(0.022)
education in years^2	-0.002*	(0.001)	-0.002*	(0.001)	-0.002*	(0.001)
good health	0.025	(0.052)	0.028	(0.052)	0.025	(0.052)
poor health	0.173**	(0.068)	0.179***	(0.069)	0.172**	(0.069)
year dummies						
1997	-0.189	(0.117)	-0.062	(0.116)	-0.188	(0.116)
1998	0.126	(0.108)	0.150	(0.117)	0.124	(0.108)
1999	-0.170*	(0.102)	-0.240**	(0.115)	-0.167*	(0.101)
2000	0.037	(0.104)	0.020	(0.115)	0.036	(0.104)

2001	-0.179*	(0.093)	-0.225**	(0.104)	-0.177*	(0.092)
2002	-0.116	(0.095)	-0.221**	(0.109)	-0.114	(0.094)
2003	0.072	(0.099)	0.044	(0.110)	0.073	(0.098)
2004	0.102	(0.099)	0.041	(0.113)	0.103	(0.098)
2005	0.140	(0.106)	0.178	(0.114)	0.124	(0.107)
2006	0.269**	(0.109)	0.225*	(0.117)	0.254**	(0.111)
2007	-0.002	(0.102)	-0.141	(0.124)	-0.022	(0.104)
2008	0.095	(0.099)	-0.006	(0.118)	0.079	(0.102)
2009	0.476***	(0.108)	0.478***	(0.119)	0.463***	(0.109)
2010	0.431***	(0.107)	0.356***	(0.123)	0.415***	(0.109)
2011	0.082	(0.121)	0.078	(0.139)	0.064	(0.124)
Wooldridge controls						
SA_0	1.529***	(0.094)	1.529***	(0.094)	1.528***	(0.094)
good health	-0.063	(0.090)	-0.061	(0.090)	-0.063	(0.089)
poor health	0.411***	(0.116)	0.407***	(0.116)	0.409***	(0.116)
single with children	0.408**	(0.164)	0.407**	(0.165)	0.406**	(0.164)
couple without children	-0.408**	(0.189)	-0.414**	(0.187)	-0.410**	(0.188)
couple with children	-0.264	(0.199)	-0.257	(0.199)	-0.263	(0.199)
child aged 6 or younger	0.298***	(0.111)	0.283**	(0.110)	0.294***	(0.111)
household size	-0.012	(0.053)	-0.014	(0.053)	-0.012	(0.053)
good health (partner)	-0.022	(0.097)	-0.023	(0.097)	-0.021	(0.097)
poor health (partner)	0.226*	(0.128)	0.215*	(0.128)	0.225*	(0.128)
regional unemployment rate	0.031*	(0.018)	0.032*	(0.018)	0.031*	(0.018)
constant	2.695***	(0.744)	2.713***	(0.742)	2.688***	(0.741)
Eastern Germany	0.260***	(0.091)	0.272***	(0.091)	0.259***	(0.091)
regional unemployment rate	0.023	(0.018)	0.021	(0.018)	0.024	(0.018)
month-of-interview dummies	no		no		no	
σ_a	0.908		0.902		0.903	
ρ	0.452		0.448		0.449	
log Likelihood	-35,794,106		-35,665,987		-35,791,632	
# of observations	82,625		82,625		82,625	
# of individuals	14,492		14,492		14,492	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	5.6%
average predicted persistence rate	17.5%
Average Partial Effect (APE)	11.9
Predicted Probability Ratio (PPR)	3.1

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (natives and migrants).

Table 12. coefficient estimates and APE – migrants

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.217***	(0.088)	1.253***	(0.250)	1.157***	(0.093)
survey-year interactions						
SA_{t-1} * 1997			-0.143	(0.275)		
SA_{t-1} * 1998			-0.282	(0.296)		
SA_{t-1} * 1999			-0.094	(0.296)		
SA_{t-1} * 2000			0.194	(0.333)		
SA_{t-1} * 2001			-0.113	(0.280)		
SA_{t-1} * 2002			0.023	(0.314)		
SA_{t-1} * 2003			0.034	(0.317)		
SA_{t-1} * 2004			-0.489	(0.329)		
SA_{t-1} * 2005			-0.330	(0.319)		
SA_{t-1} * 2006			0.418	(0.322)		
SA_{t-1} * 2007			-0.033	(0.333)		
SA_{t-1} * 2008			-0.083	(0.321)		
SA_{t-1} * 2009			-0.423	(0.359)		
SA_{t-1} * 2010			0.922**	(0.379)		
SA_{t-1} * 2011			0.156	(0.398)		
SA_{t-1} * post-Hartz dummy					0.138	(0.121)
individual characteristics						
female	-0.041	(0.071)	-0.040	(0.072)	-0.040	(0.071)
age	-0.091***	(0.033)	-0.092***	(0.033)	-0.090***	(0.033)
age^2	0.111***	(0.040)	0.112***	(0.040)	0.111***	(0.040)
education in years	-0.200**	(0.090)	-0.196**	(0.091)	-0.200**	(0.089)
education in years^2	0.006	(0.004)	0.006	(0.004)	0.006	(0.004)
good health	-0.059	(0.071)	-0.059	(0.071)	-0.059	(0.071)
poor health	0.119	(0.078)	0.124	(0.077)	0.121	(0.078)
household characteristics						
single with children	-0.292	(0.250)	-0.333	(0.257)	-0.298	(0.250)
couple without children	0.290	(0.208)	0.315	(0.210)	0.290	(0.208)
couple with children	0.091	(0.256)	0.077	(0.257)	0.086	(0.256)
child aged 6 or younger	0.269***	(0.085)	0.258***	(0.085)	0.275***	(0.086)
household size	0.062	(0.065)	0.072	(0.065)	0.064	(0.065)
partner characteristics						
immigrant	0.118	(0.097)	0.126	(0.098)	0.120	(0.097)
age	-0.030***	(0.010)	-0.033***	(0.010)	-0.030***	(0.010)
age^2	0.035**	(0.016)	0.039**	(0.016)	0.035**	(0.016)
education in years	0.019	(0.032)	0.026	(0.032)	0.018	(0.032)
education in years^2	-0.001	(0.002)	-0.001	(0.002)	-0.001	(0.002)
good health	-0.083	(0.089)	-0.082	(0.089)	-0.083	(0.089)
poor health	0.110	(0.079)	0.110	(0.078)	0.110	(0.078)
year dummies						
1997	0.062	(0.139)	0.103	(0.152)	0.065	(0.136)
1998	-0.033	(0.140)	0.055	(0.154)	-0.029	(0.139)
1999	0.051	(0.127)	0.084	(0.152)	0.055	(0.126)
2000	-0.101	(0.155)	-0.172	(0.211)	-0.095	(0.153)

2001	-0.050	(0.117)	-0.019	(0.139)	-0.044	(0.116)
2002	0.037	(0.141)	0.023	(0.167)	0.043	(0.140)
2003	0.083	(0.123)	0.067	(0.139)	0.087	(0.122)
2004	0.088	(0.142)	0.234	(0.156)	0.095	(0.140)
2005	0.178	(0.136)	0.271*	(0.153)	0.143	(0.139)
2006	0.277*	(0.166)	0.173	(0.197)	0.243	(0.173)
2007	0.077	(0.175)	0.087	(0.210)	0.034	(0.186)
2008	-0.048	(0.142)	-0.025	(0.168)	-0.091	(0.144)
2009	0.240	(0.184)	0.361*	(0.199)	0.210	(0.182)
2010	0.319**	(0.159)	0.102	(0.190)	0.287*	(0.161)
2011	-0.131	(0.180)	-0.184	(0.213)	-0.170	(0.183)
Wooldridge controls						
SA_0	1.112***	(0.129)	1.148***	(0.130)	1.116***	(0.128)
good health	0.005	(0.147)	0.019	(0.148)	0.006	(0.147)
poor health	0.106	(0.170)	0.116	(0.166)	0.101	(0.170)
single with children	0.051	(0.318)	0.093	(0.325)	0.055	(0.317)
couple without children	-0.583**	(0.266)	-0.596**	(0.268)	-0.581**	(0.266)
couple with children	-0.483*	(0.290)	-0.451	(0.292)	-0.472	(0.288)
child aged 6 or younger	0.024	(0.146)	0.033	(0.149)	0.020	(0.146)
household size	0.101	(0.077)	0.095	(0.077)	0.098	(0.077)
good health (partner)	0.302**	(0.148)	0.292*	(0.150)	0.299**	(0.147)
poor health (partner)	0.279	(0.171)	0.286*	(0.173)	0.274	(0.170)
regional unemployment rate	0.023	(0.051)	0.027	(0.051)	0.022	(0.050)
constant	0.756	(0.886)	0.704	(0.905)	0.762	(0.885)
Eastern Germany	0.235	(0.250)	0.187	(0.250)	0.234	(0.250)
regional unemployment rate	0.028	(0.051)	0.028	(0.052)	0.029	(0.051)
month-of-interview dummies	no		no		no	
σ_a	0.719		0.734		0.713	
ρ	0.340		0.423		0.337	
log Likelihood	-15,338,095		-15,227,517		-15,332,409	
# of observations	17,809		17,809		17,809	
# of individuals	3,241		3,241		3,241	

*p<0.10, **p<0.05, ***p<0.01
Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	9.5%
average predicted persistence rate	32.7%
Average Partial Effect (APE)	23.2
Predicted Probability Ratio (PPR)	3.4

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A. Average predicted transition rates, APE, and PPR have been calculated for the entire sample (natives and migrants).

Table 13. coefficient estimates and APE – 'narrow' social assistance variable

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.232***	(0.048)	1.121***	(0.179)	1.215***	(0.060)
survey-year interactions						
SA_{t-1} * 1997			-0.182	(0.218)		
SA_{t-1} * 1998			-0.284	(0.238)		
SA_{t-1} * 1999			0.184	(0.222)		
SA_{t-1} * 2000			-0.034	(0.230)		
SA_{t-1} * 2001			0.214	(0.205)		
SA_{t-1} * 2002			0.186	(0.211)		
SA_{t-1} * 2003			0.198	(0.210)		
SA_{t-1} * 2004			0.140	(0.217)		
SA_{t-1} * 2005			-0.219	(0.212)		
SA_{t-1} * 2006			0.179	(0.218)		
SA_{t-1} * 2007			0.262	(0.213)		
SA_{t-1} * 2008			0.325	(0.214)		
SA_{t-1} * 2009			-0.145	(0.219)		
SA_{t-1} * 2010			0.441*	(0.228)		
SA_{t-1} * 2011			0.046	(0.229)		
SA_{t-1} * post-Hartz dummy					0.035	(0.075)
individual characteristics						
female	-0.024	(0.044)	-0.025	(0.044)	-0.024	(0.044)
immigrant	0.324***	(0.076)	0.328***	(0.076)	0.324***	(0.076)
age	-0.066***	(0.020)	-0.066***	(0.020)	-0.065***	(0.020)
age^2	0.078***	(0.024)	0.078***	(0.024)	0.078***	(0.024)
education in years	-0.351***	(0.068)	-0.348***	(0.068)	-0.351***	(0.068)
education in years^2	0.010***	(0.003)	0.010***	(0.003)	0.010***	(0.003)
good health	-0.073*	(0.042)	-0.073*	(0.042)	-0.073*	(0.042)
poor health	0.087*	(0.050)	0.087*	(0.051)	0.087*	(0.050)
household characteristics						
single with children	-0.063	(0.119)	-0.068	(0.120)	-0.063	(0.119)
couple without children	0.149	(0.136)	0.155	(0.138)	0.150	(0.136)
couple with children	-0.001	(0.135)	-0.006	(0.136)	-0.001	(0.135)
child aged 6 or younger	0.027	(0.061)	0.022	(0.061)	0.028	(0.061)
household size	0.144***	(0.039)	0.146***	(0.039)	0.144***	(0.039)
partner characteristics						
immigrant	0.079	(0.081)	0.082	(0.081)	0.079	(0.081)
age	-0.023***	(0.006)	-0.023***	(0.006)	-0.023***	(0.006)
age^2	0.035***	(0.010)	0.034***	(0.010)	0.035***	(0.010)
education in years	0.022	(0.020)	0.023	(0.020)	0.022	(0.020)
education in years^2	-0.002	(0.001)	-0.002	(0.001)	-0.002	(0.001)
good health	0.033	(0.050)	0.036	(0.050)	0.033	(0.050)
poor health	0.177***	(0.057)	0.180***	(0.057)	0.176***	(0.056)
year dummies						
1997	-0.100	(0.098)	-0.052	(0.102)	-0.099	(0.098)
1998	0.205**	(0.099)	0.255**	(0.107)	0.205**	(0.099)
1999	-0.049	(0.091)	-0.090	(0.105)	-0.047	(0.091)

2000	0.096	(0.103)	0.113	(0.113)	0.096	(0.103)
2001	-0.001	(0.085)	-0.042	(0.096)	0.000	(0.084)
2002	0.095	(0.090)	0.062	(0.101)	0.097	(0.089)
2003	0.088	(0.089)	0.050	(0.099)	0.089	(0.088)
2004	0.231**	(0.092)	0.208**	(0.102)	0.232**	(0.092)
2005	0.263***	(0.096)	0.338***	(0.103)	0.255***	(0.097)
2006	0.481***	(0.100)	0.454***	(0.110)	0.473***	(0.103)
2007	0.191*	(0.101)	0.122	(0.123)	0.181*	(0.105)
2008	0.192**	(0.093)	0.102	(0.108)	0.183*	(0.094)
2009	0.606***	(0.103)	0.643***	(0.112)	0.600***	(0.103)
2010	0.616***	(0.101)	0.517***	(0.115)	0.608***	(0.101)
2011	0.121	(0.112)	0.133	(0.127)	0.111	(0.113)
Wooldridge controls						
SA_0	1.443***	(0.084)	1.445***	(0.084)	1.442***	(0.084)
good health	-0.099	(0.082)	-0.097	(0.082)	-0.099	(0.082)
poor health	0.218**	(0.103)	0.220**	(0.103)	0.217**	(0.103)
single with children	0.399**	(0.161)	0.412**	(0.163)	0.398**	(0.161)
couple without children	-0.393**	(0.169)	-0.405**	(0.170)	-0.394**	(0.169)
couple with children	-0.232	(0.180)	-0.225	(0.182)	-0.232	(0.180)
child aged 6 or younger	0.140	(0.099)	0.138	(0.099)	0.139	(0.099)
household size	-0.065	(0.050)	-0.067	(0.050)	-0.065	(0.050)
good health (partner)	-0.032	(0.089)	-0.040	(0.089)	-0.032	(0.089)
poor health (partner)	0.149	(0.110)	0.141	(0.110)	0.148	(0.110)
regional unemployment rate	0.032*	(0.017)	0.032*	(0.017)	0.032*	(0.017)
constant	0.793	(0.586)	0.785	(0.590)	0.792	(0.585)
Eastern Germany	0.252***	(0.085)	0.252***	(0.086)	0.251***	(0.085)
regional unemployment rate	0.019	(0.017)	0.019	(0.017)	0.020	(0.017)
month-of-interview dummies	no		no		no	
σ_a	0.876		0.878		0.874	
ρ	0.434		0.435		0.433	
log Likelihood	-44,368,466		-44,245,166		-44,367,580	
# of observations	100,434		100,434		100,434	
# of individuals	17,733		17,733		17,733	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	4.8%
average predicted persistence rate	18.5%
Average Partial Effect (APE)	13.7
Predicted Probability Ratio (PPR)	3.9

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A.

Table 14. coefficient estimates and APE – Housing Benefits

dependent variable: SA_t	(I) standard specification		(II) standard specification + survey-year interactions		(III) standard specification + post-Hartz interaction	
SA_{t-1}	1.033***	(0.065)	1.445***	(0.175)	1.125***	(0.070)
survey-year interactions						
SA_{t-1} * 1997			-0.433**	(0.211)		
SA_{t-1} * 1998			-0.376*	(0.225)		
SA_{t-1} * 1999			-0.311	(0.225)		
SA_{t-1} * 2000			-0.063	(0.234)		
SA_{t-1} * 2001			-0.408*	(0.210)		
SA_{t-1} * 2002			-0.150	(0.219)		
SA_{t-1} * 2003			-0.456**	(0.229)		
SA_{t-1} * 2004			-0.532**	(0.222)		
SA_{t-1} * 2005			-0.833***	(0.227)		
SA_{t-1} * 2006			-0.447*	(0.237)		
SA_{t-1} * 2007			-0.365	(0.254)		
SA_{t-1} * 2008			-0.475*	(0.259)		
SA_{t-1} * 2009			-0.668**	(0.287)		
SA_{t-1} * 2010			-0.677**	(0.278)		
SA_{t-1} * 2011			-0.721*	(0.383)		
SA_{t-1} * post-Hartz dummy					-0.253**	(0.102)
individual characteristics						
female	0.042	(0.047)	0.045	(0.048)	0.044	(0.048)
immigrant	0.248***	(0.077)	0.254***	(0.079)	0.250***	(0.078)
age	-0.074***	(0.021)	-0.075***	(0.022)	-0.074***	(0.022)
age^2	0.085***	(0.026)	0.087***	(0.026)	0.086***	(0.026)
education in years	-0.142*	(0.075)	-0.143*	(0.077)	-0.143*	(0.076)
education in years^2	0.002	(0.003)	0.002	(0.003)	0.002	(0.003)
good health	-0.032	(0.048)	-0.035	(0.048)	-0.033	(0.048)
poor health	-0.031	(0.065)	-0.037	(0.065)	-0.036	(0.065)
household characteristics						
single with children	0.020	(0.103)	0.019	(0.106)	0.027	(0.104)
couple without children	0.202	(0.123)	0.205*	(0.124)	0.206*	(0.124)
couple with children	0.206*	(0.125)	0.212*	(0.127)	0.214*	(0.126)
child aged 6 or younger	0.129**	(0.066)	0.116*	(0.066)	0.117*	(0.066)
household size	0.093**	(0.040)	0.097**	(0.041)	0.092**	(0.040)
partner characteristics						
immigrant	0.169**	(0.084)	0.175**	(0.086)	0.170**	(0.085)
age	-0.017**	(0.007)	-0.018**	(0.007)	-0.017**	(0.007)
age^2	0.008	(0.012)	0.008	(0.012)	0.008	(0.012)
education in years	0.038	(0.024)	0.040	(0.025)	0.039	(0.024)
education in years^2	-0.004**	(0.002)	-0.004***	(0.002)	-0.004**	(0.002)
good health	-0.045	(0.057)	-0.043	(0.057)	-0.044	(0.057)
poor health	0.041	(0.071)	0.042	(0.071)	0.037	(0.071)
year dummies						
1997	-0.041	(0.094)	0.080	(0.114)	-0.039	(0.097)
1998	-0.004	(0.092)	0.098	(0.107)	-0.000	(0.094)
1999	-0.044	(0.091)	0.042	(0.111)	-0.044	(0.093)

2000	-0.170*	(0.089)	-0.166	(0.109)	-0.171*	(0.091)
2001	-0.187**	(0.081)	-0.077	(0.097)	-0.190**	(0.083)
2002	-0.138*	(0.083)	-0.099	(0.102)	-0.138	(0.085)
2003	0.004	(0.086)	0.119	(0.102)	0.007	(0.088)
2004	-0.002	(0.090)	0.129	(0.106)	-0.006	(0.092)
2005	-0.196**	(0.092)	0.023	(0.106)	-0.132	(0.093)
2006	-0.254**	(0.102)	-0.148	(0.116)	-0.199*	(0.103)
2007	-0.163	(0.108)	-0.077	(0.129)	-0.111	(0.109)
2008	-0.229**	(0.096)	-0.118	(0.112)	-0.181*	(0.097)
2009	-0.129	(0.112)	0.016	(0.125)	-0.087	(0.112)
2010	-0.270**	(0.126)	-0.117	(0.144)	-0.222*	(0.124)
2011	-0.237*	(0.126)	-0.090	(0.131)	-0.197	(0.127)
Wooldridge controls						
SA_0	1.189***	(0.087)	1.175***	(0.088)	1.181***	(0.088)
good health	-0.012	(0.091)	-0.007	(0.094)	-0.010	(0.092)
poor health	0.437***	(0.112)	0.458***	(0.115)	0.447***	(0.113)
single with children	0.353**	(0.144)	0.366**	(0.147)	0.346**	(0.145)
couple without children	-0.570***	(0.153)	-0.573***	(0.155)	-0.572***	(0.154)
couple with children	-0.327**	(0.160)	-0.327**	(0.162)	-0.335**	(0.161)
child aged 6 or younger	0.285***	(0.094)	0.307***	(0.097)	0.302***	(0.096)
household size	0.010	(0.049)	0.007	(0.050)	0.012	(0.049)
good health (partner)	0.207**	(0.100)	0.210**	(0.103)	0.206**	(0.101)
poor health (partner)	0.500***	(0.129)	0.518***	(0.133)	0.510***	(0.130)
regional unemployment rate	0.023	(0.019)	0.023	(0.019)	0.024	(0.019)
constant	-0.162	(0.651)	-0.273	(0.668)	-0.187	(0.658)
Eastern Germany	0.242**	(0.096)	0.244**	(0.098)	0.244**	(0.097)
regional unemployment rate	0.009	(0.019)	0.010	(0.019)	0.008	(0.019)
month-of-interview dummies	no		no		no	
σ_a	0.694		0.726		0.708	
ρ	0.325		0.345		0.334	
log Likelihood	-28,872,893		-28,778,806		-28,843,876	
# of observations	100,434		100,434		100,434	
# of individuals	17,733		17,733		17,733	

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

Note: robust standard errors are given in parentheses. Estimates are obtained using cross-sectional respondent weights. All household, individual, and partner characteristics are lagged by one period. The 'post-Hartz dummy' is equal to one for the years 2005-2011 and zero otherwise. 'Wooldridge controls' include the social assistance receipt status in the initial period (SA_0) and individual longitudinal averages of the remaining listed variables. Source: SOEP 1995-2011

Panel B – predicted transition rates, APE, and PPR

	standard model
average predicted entry rate	2.5%
average predicted persistence rate	10.0%
Average Partial Effect (APE)	7.6
Predicted Probability Ratio (PPR)	4.0

Note: calculations done using cross-sectional respondent weights based on the coefficient estimates presented in Panel A.

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