The Dynamics of Social Assistance Benefit Receipt



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Abstract

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This dissertation consists of three articles on social assistance benefit receipt dynamics in European countries.
The first article presents an analysis of state dependence in benefit receipt in Germany based on annual survey data from the German Socio-Economic Panel. The observation period extends from 1995 to 2011, thus covering the 2005 'Hartz reforms'. I estimate a series of dynamic random-effects probit models to control for observed and unobserved heterogeneity and the endogeneity of initial conditions. The high observed state dependence has a substantial structural component, with benefit receipt one year ago being associated with an increase in the likelihood of receipt today by 13 percentage points. There is only little evidence for time-variation in state dependence.
The second article presents evidence on spell durations and the frequency of repeat spells using monthly administrative data from Luxembourg, the Netherlands, Norway and Sweden. In the two Nordic countries, short-term benefit receipt is the norm, with only around 6% and 11% of spells in Norway and Sweden lasting longer than 12 months. Most recipients however have multiple spells. In Luxembourg and the Netherlands, long-term benefit receipt is frequent, with median spell durations of 14 and 9 months, respectively, and one-third and one-quarter of all spells lasting 24 months or longer. The total duration of benefit receipt across spells is much higher in the Netherlands and Luxembourg than in Norway and Sweden.
The third article tests the validity of one of the central assumptions of dynamic discrete-choice models of benefit dynamics, the conditional Markov property. Using monthly administrative data from Norway, the article shows that the Markov property is violated as estimated state dependence is affected by the chosen time unit of analysis. The standard model can be improved by permitting for different entry and persistence equations and duration and occurrence dependence in benefit receipt.

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Chapter 1

Introduction

Most OECD countries operate extensive minimum-income benefit systems designed to guarantee an adequate standard of living to all households and individuals with insufficient means from other sources. 'Social assistance' or 'welfare' benefits target individuals out of work who lack the necessary employment and contribution record to be entitled to insurance-based unemployment benefits and those who have exhausted their rights to such benefits.[1] In many countries, social assistance moreover provides a top-up to earnings for low-income workers and their families. As a last-resort benefit, social assistance is by definition means-tested and, unlike insurance-based unemployment benefits, not contribution-based. The support provided through social assistance is typically intended – implicitly or even explicitly – to be temporary covering employable individuals during periods of economic hardship, while individuals unable to earn their own income for longer periods can draw on separate categorical sickness or disability benefits. Social assistance payments can however in principle be received for an unlimited period of time as long as the recipient continues to pass the means-test and satisfy other eligibility criteria.[2]

Rising caseload numbers in the 1980s and early 1990s (see for instance

[1] I use the terms 'social assistance' and 'welfare' as synonyms. Social assistance benefits may be paid in cash or in kind. Similar benefits often exist for seniors; the focus throughout this dissertation is however on individuals of working-age (as defined further below).

[2] An exception is the U.S. Temporary Assistance for Needy Families (TANF) that generally comes with a 60-month lifetime limit (for a discussion see Grogger (2003, 2004) and Grogger & Michalopoulos (2003)).

Gough, Bradshaw, Ditch, Eardley & Whiteford (1997)) and the resulting burden to public finances led to growing concerns about long-term social assistance receipt. Under the impression – be it right or wrong – that a significant share of recipients rely on benefits for prolonged time periods, public and academic debates evolved around the question whether benefit receipt might have 'scarring effects', creating its own dependence and thus leading to 'welfare traps' (Plant, 1984). Such debates and the financial pressure from rising welfare reliance resulted in major welfare reforms in a number of OECD countries – notably in the United States, the United Kingdom and Germany. These reforms put a greater emphasis on an 'activating' approach to benefit provision, typically through a combination of tightened eligibility criteria, increased conditionality, and strengthened support to employable recipients.[3]

Academic work on social assistance receipt has focused on studying the incidence of long-term dependence and the drivers of benefit receipt. Its aim is to contribute to a better understanding of the incentive structures of the benefit system and to permit the targeting of policies to support and 'activate' benefit recipients at those who are likely to benefit the most.

An obstacle for such work has been the limited availability of micro-level panel data on benefit receipt with sufficiently short observation intervals. One possible data source are national household surveys like the British Household Panel Survey (BHPS) or the U.S. Panel Study of Income Dynamics (PSID).[4] These data sets often have the drawback of providing reliable information on benefit receipt and household composition only at the annual level, because the information for such surveys is usually collected through yearly interviews.[5] An alternative data source are administrative records, which often have the advantage of a larger sample size, lower attrition rates, and an overall higher

[3]For a recent overview of the institutional features of social assistance systems in OECD countries see Immervoll (2010).

[4]Cross-national household surveys, such as the European Union Statistics on Income and Living Conditions (EU-SILC), are typically not suitable due to their smaller sample size.

[5]An exception is the U.S. Survey of Income and Programme Participation (SIPP) that is based on four-monthly interviews but whose panels only run for a maximum of four years.

reliability of the information provided.[6] In many cases, such data have however been compiled for tax purposes and therefore do not provide information on the timing of payments during the year.

As a consequence, surprisingly little evidence exists to date on the short-term individual-level dynamics of benefit receipt, *i.e.* the paths that individuals take into and out of benefits. Much of the existing research on spell durations and the frequency of long-term benefit receipt is based on annual data. An obvious limitation of such studies is that they do not distinguish between single long spells and a series of recurrent shorter spells. Where data at monthly or quarterly intervals are available, observation periods tend to be short, making an analysis of re-entry into benefits of benefit leavers (sometimes unfortunately referred to as 'recidivism') difficult.

Econometric studies of social assistance dynamics are often motivated by the strong persistence in benefit receipt. Indeed, a standard observation in micro-level sequences of benefit receipt is that period-to-period persistence rates in benefits are much higher than the corresponding entry rates. Current benefit receipt thus tends to be a very good predictor of benefit receipt in the future. In the absence of precise data on benefit spell durations that could be used for event-history analyses, studies of receipt dynamics have therefore typically focused on explaining the drivers of this 'state dependence' in social assistance.

Part of the observed (or 'raw') state dependence is simply due to individual heterogeneity: Individuals with less favourable labour market characteristics – for instance those with little education or health problems or singles with young children – are more likely to receive social assistance. To the extent that these characteristics persist over time, the share of recipients who remain in benefits will be higher than the proportion of non-recipients who enter. State dependence induced by individual heterogeneity will disappear if all relevant

[6]For a related discussion of the benefits of survey data *vs.* administrative data for the measurement of earnings see Atkinson, Bourguignon & Morrisson (1992).

individual and household characteristics can be controlled for. It is therefore commonly referred to as 'spurious' (Heckman, 1978, 1981a).

The gap between persistence and entry rates might however also hint at a detrimental causal effect of social assistance, with benefit receipt *per se* raising the likelihood of future receipt. A number of possible mechanisms for such an effect have been proposed: Benefit payments may have a behavioural impact on the recipient if they induce a loss of motivation or a feeling of helplessness and hence lead to a reduction in the effort to become self-sufficient (Bane & Ellwood, 1994). Claiming benefits may be associated with a perceived stigma (Moffitt, 1983; Blank, 1989) or information costs that are lower for individuals who received benefits before. Social assistance receipt may also reduce the prospects of finding (better-paid) employment if potential employers perceive a benefit history (or a recipient's time in unemployment) as a negative signal about the unobserved productivity of an applicant (Vishwanath, 1989; Lockwood, 1991). In all of these cases, there exists a direct causal link between current and future benefit receipt that gives rise to 'structural', 'genuine' or 'true' state dependence (Heckman, 1978, 1981a).[7]

The two potential causes of state dependence have very different implications for policy-making. If state dependence is entirely spurious, *i.e.* due to recipients' characteristics, policy-makers do not need to be much concerned about individuals' use of social assistance as a short-term support. Long-term benefit dependence indicates in this case that a recipient faces high hurdles to self-sufficiency, possibly a lack of skills or work experience or problems of physical or mental health. Measures that aim at encouraging exits from benefits are likely to have little impact unless these barriers are addressed directly. If, by contrast, state dependence is partly structural and benefit receipt *as such* increases the probability of future benefit receipt, policies should aim at helping

[7] Contini & Negri (2007) point out that the observed persistence in social assistance receipt may also result from state dependence in unemployment or poverty if these coincide with social assistance receipt. For a recent discussion of potential sources of state dependence in social assistance see Mood (2013).

low-income individuals avoid even short spells in social assistance. Measures that prevent entry or facilitate early exits from benefits, such as in-work benefits that raise the income from low-wage employment, can induce lasting reductions in receipt rates.

In this dissertation, I present three studies on the dynamics of social assistance benefit receipt in a selection of European countries. The main focus of this work is on the incidence of long-term benefit receipt and on state dependence as one of the potential drivers of persistence in benefits.[8]

In Chapter 2, I present an analysis of state dependence in benefit receipt in Germany based on annual survey data from the German Socio-Economic Panel (SOEP).[9] I estimate a series of dynamic random-effects probit models – the standard model in the existing literature on state dependence – to control for observed and unobserved heterogeneity and the endogeneity of initial conditions. The observation period extends from 1995 to 2011 thus covering the time of a substantial reform of the German social assistance benefit system, the 2005 'Hartz' reforms. There is considerable 'raw' state dependence in social assistance receipt in Germany, with an average year-to-year persistence rate of 68% comparing to an average entry rate of just above 3%. The econometric analysis suggests that observed state dependence has a substantial structural component. Benefit receipt one year ago is associated with an increase in the likelihood of benefit receipt today by a factor of 3.4. This corresponds to an average partial effect of 13 percentage points. Average predicted entry and per-

[8] In all three studies, I look exclusively at the dynamics of benefit *receipt* and do not consider the question of *eligibility*. There is an extensive literature documenting non-take-up of social assistance by individuals who are eligible but do not claim (see Hernanz, Malherbet & Pellizzari (2004) for an overview). Depending on the frequency of eligibility checks, also the opposite case may occur if benefits are granted to claimants who are not eligible (anymore). The empirical modelling of benefit eligibility is not straightforward because it requires precise information on family composition, income and assets (Atkinson, 1989). The administrative data used in Chapters 3 and 4 are not suited for such an analysis because they do not provide information on income (at the monthly level). For earlier analyses of non-take-up of social assistance benefits in Germany based on the same survey data used in Chapter 2, see Riphahn (2001) and Bruckmeier & Wiemers (2010).

[9] This is a slightly extended version of a paper that is forthcoming in *Research in Labor Economics* (Königs, 2014).

sistence rates and the absolute level of structural state dependence are higher in Eastern Germany than in Western Germany. There is only little evidence for time-variation in state dependence around the years of the Hartz reforms.

Chapter 3 adds to the scarce literature on social assistance spell durations and the frequency of repeat spells by presenting evidence on monthly benefit dynamics four European countries: Luxembourg, the Netherlands, Norway and Sweden.[10] The analysis is based on four separate sources of administrative data. In the first part of the chapter, I exploit the full length of the four panels, which vary from 9 years (for Sweden) to 23 years (for Luxembourg), to describe developments in benefit receipt and transition rates. There has been no uniform trend in benefit receipt over the last decade or so, with receipt rates having declined in the Netherlands and Norway, having remained stable in Sweden, and having risen in Luxembourg. In the late 2000s, the share of working-age individuals who received benefits in a given month varied between 2% in Norway and 4% in Luxembourg and the Netherlands. In the second part of the chapter, I present evidence on benefit spell durations and the frequency of repeat receipt in the four countries over an eight-year period from January 2001 to December 2008 during which the panels for all four countries overlap. In the two Nordic countries, short-term benefit receipt is the norm with only around 6% and 11% of spells in Norway and Sweden lasting longer than 12 months. Most recipients however have multiple spells, and the majority of benefit leavers returns into benefits within three months of leaving. In Luxembourg and the Netherlands, long-term benefit receipt is frequent, with median spell durations of 14 and 9 months, respectively, and one-third and one-quarter of all spells lasting 24 months or longer. The resulting total duration of benefit receipt across spells over the eight-year period is two to three times as high in the Netherlands and Luxembourg as in Norway and Sweden.

[10]This chapter is based on the single-authored part of a forthcoming OECD working paper, which contains two other parts produced by Herwig Immervoll and Stephen Jenkins (Immervoll, Jenkins & Königs, 2014).

Chapter 4, which is joint work with Manudeep Bhuller (Statistics Norway) and Christian N. Brinch (BI Norwegian Business School), tests the validity of one of the central assumptions of dynamic discrete-choice models of benefit dynamics, the conditional Markov property.[11] The Markov property has easy-to-derive implications for how estimated period-to-period benefit transition probabilities should relate when receipt processes are aggregated over time, for instance from the monthly to the annual level. Using monthly data from Norwegian administrative records, we assess whether the conditional Markov property holds in benefit receipt dynamics and find that the standard conditional Markov model is misspecified. Estimated state dependence is affected substantially by the chosen time unit of analysis, with the average partial effect of past benefit receipt rising with the level of aggregation. The model can be improved considerably by permitting richer types of benefit dynamics: Allowing for different entry and persistence equations, we find that the effect of observed and unobserved characteristics differs between the two processes. Extending the model further, we obtain strong evidence for duration and occurrence dependence in benefit receipt. Based on our preferred model, the average predicted month-to-month persistence probability in benefit receipt for a first-time entrant is 37 percentage points higher than the average predicted entry rate of an individual with no previous benefit receipt history. Aggregated over a 12-month period, the average partial effect is about 5 percentage points.

Chapter 5 of this dissertation discusses some implications of these findings and outlines potential avenues for future research.

[11] This chapter is a slightly modified version of a recent Statistics Norway Research Department Discussion Paper (Bhuller, Brinch & Königs, 2014).

Chapter 2

State Dependence in Social Assistance Benefit Receipt in Germany before and after the Hartz Reforms

2.1 Introduction

A standard observation in data on social assistance benefit receipt is that current recipients are much more likely than non-recipients to receive benefits also in the next period. For instance, as described below, the average year-to-year persistence rate on benefits for recipients in Germany was 68% over the years 1995-2011 compared to a year-to-year entry rate for non-recipients of only 3%. As outlined in Chapter 1, the gap between benefit persistence and entry rates is explained in the empirical literature as being due to a combination of individual heterogeneity and the potential causal effect of benefit receipt on the likelihood of receipt in the future. While the decomposition of observed 'raw' state dependence into its structural and spurious components (Heckman, 1978, 1981a) has been the focus of much recent work on social assistance dynamics, the number of studies that provide evidence on the level of structural state dependence remains small to date and existing work is limited to a few countries.

This chapter presents an empirical analysis of state dependence in social assistance receipt in Germany for the years 1995 to 2011 based on annual survey

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data from the German Socio-Economic Panel (SOEP). The period studied is of particular interest because it covers the far-reaching 'Hartz reforms' implemented from 2003 to 2005, which fundamentally changed the system of social assistance benefit provision in Germany. While the type of model estimated is not suited for assessing potential causal reform effects, the analysis presents evidence on both the level of state dependence as well as on its variations over the observation period and around the time of the reforms. Sample selection criteria and the estimation technique used are similar to those in earlier analyses for other countries such that results can be compared across studies. A methodological contribution of the chapter lies in that it contrasts the results obtained to those reported in two other recent studies for Germany, which – for narrower samples and based on a different modelling approach – obtain results very different from those presented in this chapter.

The earliest and most influential study on state dependence in social assistance is due to Chay & Hyslop (1998, 2014) who look at the receipt of Aid to Families with Dependent Children (AFDC) in the United States.[1] They compare results from a range of different specifications based on eight waves of four-monthly data from the Survey of Income and Program Participation (SIPP). They find evidence of state dependence in benefit receipt and show that dynamic random-effects and fixed-effects approaches yield comparable results. They moreover document that the failure to appropriately control for the initial outcome of a benefit receipt sequence in the analysis leads to a strong upward bias in the level of estimated state dependence. Chay, Hoynes & Hyslop (1999) use monthly administrative data from California and a 'fixed effects' conditional logit model to study state dependence in receipt sequences of 32 months and find evidence of first- and second-order state dependence. Neither of the two studies however quantifies the fraction of overall 'raw' state dependence that is structural. Gong (2004) presents an analysis of state dependence

[1] A related literature looks at state dependence in unemployment, labour force participation, or low-income / poverty (for references see Chapter 4).

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among female benefit recipients in Australia. He uses a dynamic multinomial logit model estimated on quarterly administrative data to study benefit transitions between Income Support, Family Allowance and off benefits and finds state dependence in both benefit programmes. He however cannot study entries into benefits because his data provide information on recipients only.

More recent studies are based on annual data on benefit receipt. Hansen & Lofstrom (2008, 2011) and Andrén & Andrén (2013) study the native-immigrant gap in benefit receipt using administrative data from Sweden. They report higher structural state dependence for migrants but emphasize the importance of unobserved heterogeneity for explaining differences in receipt rates between natives and migrants. In a study based on the British Panel Household Survey (BHPS), Cappellari & Jenkins (2008a) estimate stronger structural state dependence for lone parents and for recipients with one non-interrupted spell compared to individuals with a spell of work between interview dates. For Canada, Hansen, Lofstrom, Liu & Zhang (2014) report strong variations in state dependence across provinces and suggest that the level of state dependence may be positively related to benefit generosity. In Chapter 4, I discuss the implications of using monthly *vs.* annual data for estimating state dependence in benefit receipt in Norway (see also Bhuller et al. (2014)).[2]

I am aware of only two studies of state dependence in social assistance benefit receipt in Germany.[3] Wunder & Riphahn (2014) compare the benefit dynamics of natives and immigrants in Western Germany for the post-Hartz years 2005-2009. Based on SOEP data, they estimate a dynamic multinomial logit model with three competing states distinguishing between social assis-

[2]A few studies look at the related question of duration dependence in social assistance receipt using event-history models, see for instance Blank (1989) and Sandefur & Cook (1998) for the U.S., Dahl & Lorentzen (2003b) for Norway, Bäckman & Bergmark (2011) and Mood (2013) for Sweden or Schels (2013) for Germany.

[3]Related earlier articles study the determinants of social assistance receipt in Germany without looking at state dependence (Riphahn, 2004; Riphahn, Sander & Wunder, 2013; Schels, 2013). For cross-sectional analyses of the determinants of social assistance benefit receipt in Germany see Voges & Rohwer (1992) or Riphahn & Wunder (2012). Mühleisen & Zimmermann (1994) and Biewen & Steffes (2010) study state dependence in unemployment.

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tance receipt, employment, and 'inactivity' (which they define as including unemployment). They find that persistence in social assistance benefit receipt can mostly be accounted for by observable characteristics, with only limited evidence for structural state dependence. In a recent follow-up paper, Riphahn & Wunder (2013) extend this analysis to cover the years immediately before the Hartz reform. Using the same model, they find evidence for a decline in persistence in welfare and inactivity and for a rise in frequency of transitions from welfare to work but also from work to welfare.

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

A sensitivity check shows that the estimated level of state dependence is affected by sample selection and, more importantly, by the method used for defining the benefit variable. In an attempt to reconcile my results with those presented by Riphahn & Wunder in their two recent papers, I show that using an individual- rather than the more standard household-level definition of the social assistance variable leads to a substantial drop in the estimated level of state dependence. Moreover, findings are sensitive to the source of information used, *i.e.* whether data on benefit receipt are taken from personal or household

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questionnaires. Once these issues and differences in sample selection are accounted for, the results presented in this chapter are consistent with Riphahn & Wunder's findings of only very weak state dependence in Germany.

The remainder of this chapter is structured as follows: Section 2.2 gives an overview of the institutional background in Germany during the observation period and defines the benefit variable. Sections 2.3 and 2.4 describe the SOEP data used in the analysis and present trends in benefit receipt and transition rates, respectively. Section 2.5 introduces the econometric model used for the analysis along with two extensions suited to study time-variation in state dependence. Section 2.6 presents the empirical results on the level and potential time-variation in state dependence. Section 2.7 concludes.

2.2 Institutional background and definition of the benefit variable

During the 1995-2011 observation period, the German social assistance system underwent far-reaching reforms. The so-called 'Hartz reforms', implemented by the left-of-centre coalition of Social Democrats and Greens from 2003 to 2005, resulted, among other things, in a structural change of the groups entitled to different last-resort minimum-income benefits.[4] This section describes some key features of the benefit system in the years before and after the reforms and defines the social assistance variable used in the analysis.

2.2.1 Institutional background

Until 2005, the German income-support system for working-age individuals had a three-tier structure. As the top layer, Unemployment Insurance benefits (UI, *Arbeitslosengeld*) aimed at replacing an individual's income after job loss for a limited amount of time, with eligibility being conditional on a previous

[4]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').

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work and contribution record.[5] Individuals whose entitlements to UI had expired could claim Unemployment Assistance benefits (UA, *Arbeitslosenhilfe*). UA was earnings-related but means-tested on family income and less generous than UI.[6] Unlike UI, UA benefits could in principle be received for an indefinite period of time under the condition that the claimant was looking for and available for work. Finally, Social Assistance (SA, *Sozialhilfe*) served as a benefit of last resort below this primary social safety net.[7] SA was understood as a temporary emergency benefit, and eligibility required from individuals to have exhausted all alternative sources of income in the form of earnings from work, UI or UA benefit payments and financial support from direct family members. While SA had initially been targeted primarily at individuals with special needs and limited employability, a gradual tightening of eligibility criteria for UI and UA over time meant that a growing number of individuals were shifted into SA. Due to the lower benefit amounts of UA compared to UI, recipients of UA benefits moreover often qualified for SA payments as a top-up.

The fourth package of the Hartz reforms, which entered into force in January 2005, abolished this three-tier system with the aim of strengthening labour market services and intensifying the activation of unemployed job seekers. The contribution-based UI was replaced by the new Unemployment Benefit I (UBI, *Arbeitslosengeld I*), with an initially unchanged maximum benefit duration and replacement rate.[8] The more relevant change in the context of this study was

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

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

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

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

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the merger of UA and SA for employable job seekers into the new means-tested Unemployment Benefit II (UBII, *Arbeitslosengeld II*). The computation of UBII benefit levels follows a similar logic as for the former last-resort SA. Compared to the old UA scheme, the new UBII is typically less generous and it no longer depends on the level of previous earnings. SA continues to exist as a separate programme but is now restricted to individuals incapable for work due to sickness, disability, or care duties. The Hartz reforms thus introduced a clearer distinction between the minimum-income support for employable and non-employable individuals.

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

2.2.2 Definition of the benefit variable

In light of the institutional changes just described, it is not obvious what the best choice is for defining a social assistance benefit variable that allows for a consistent analysis of receipt dynamics over the entire observation period. Existing studies of social assistance dynamics in Germany focus on relatively short time periods either before (Voges & Rohwer, 1992; Riphahn, 2004) or after the Hartz reforms (Schels, 2013; Wunder & Riphahn, 2014) and look at receipt of either SA or UBII only. Riphahn & Wunder (2013), who compare pre- and post-reform benefit dynamics for able-bodied individuals, study receipt of UA and SA before 2005 and of UBII thereafter. In this chapter, I choose a slightly different approach by defining a broader benefit variable that takes into account receipt of all means-tested benefits, *i.e* SA, UA (before 2005), UBII (from 2005), and HB (for an overview see Table 2.1).

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The classification of pre- and post-reform SA and of UBII as social assistance programmes is uncontroversial. A categorization of UA by contrast is more difficult: As a contribution-based and earnings-related benefit it does not correspond to the standard definition of a social assistance programme. The reason why it is included in this analysis nonetheless is that treating UA as a social assistance programme makes sense in terms of the implied benefit dynamics. The typical recipient of UA in December 2004 would go on to receive UBII in January 2005. It is not evident why such a transition should bring about a change in the individual's social assistance benefit receipt status for the purpose of this analysis. As the direct precursor to UBII, UA moreover shared a number of key features of the other social assistance programmes. Unlike UI benefits, UA was means-tested and could be claimed for an infinite period of time. Also, it was not paid for by social-security contributions but was tax-funded. These features make it resemble social assistance benefit schemes like SA or UBII. Finally, I also take into account receipt of HB as a means-tested benefit targeted at low-income benefits more broadly. HB receipt rates are however relatively low and excluding HB from the analysis does not affect its main conclusions (see Königs (2013)).[9]

Like most comparable previous studies, I use the individual as the unit of analysis. While eligibility for social assistance benefits is determined at the level of a possibly larger 'need unit', frequent changes in household composition imply that it is not obvious how the benefit dynamics of a household could be studied over time. Since for the means test, the financial status not only of the claimant alone but of other household members matters, I categorize an individual as a benefit recipient if benefit payments are recorded for any individual in the household.[10] I measure benefit receipt using reports of receipt

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

[10]In the estimations, I need to assume independence across individuals. Strictly speaking, this assumption is violated if in each period a household can be represented by several

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Table 2.1: *Principal eligibility conditions of social assistance benefit programmes for working-age individuals in Germany*

before the Hartz reforms	**after the Hartz reforms**
Social Assistance (*Sozialhilfe*) • 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 (*Arbeitslosengeld*) • history of work and social insurance contributions but expired (or lacking) entitlements to Unemployment Insurance benefits	**Unemployment Benefits II (*Arbeitslosengeld II*)** • lacking or expired claims to contributory Unemployment Benefit I and income and assets below a specified minimum level • available for at least 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)

in both the individual and household questionnaires of the SOEP, counting receipt as occurring if there is a report in either source or both sources.[11] To account for the importance of household composition I include partner and household characteristics as explanatory variables in the econometric estimations. The household as defined in the survey will however not always coincide with the benefit unit used by the social assistance office to assess eligibility for income-support payments.

observations that by construction have the same social assistance receipt status. Like earlier authors, I ignore any potential inconsistencies induced by this lack of independence. In an earlier working-paper version of this chapter (Königs, 2013), I however show that results differ little when the sample is split between women and men, a case in which the independence assumption is arguably more credible.

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

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The time interval of analysis is one year during which benefit receipt is measured only once at the moment of the interview. While respondents in the SOEP are requested to provide information on receipt of income-support payments on a monthly basis, corresponding information on personal and household characteristics is lacking that would be required to estimate the model at the monthly level. Earlier research moreover indicates that the quality of monthly data on benefit receipt derived from annual surveys is often poor. In particular, so-called 'seam bias' is observed in months where survey periods adjoin or overlap as respondents have apparent difficulties to answer questions that relate to early parts of the survey year (Pavetti, 1993; Blank & Ruggles, 1994). The approach of modelling benefit transitions from one interview date to the next therefore appears to be the safer option, and this approach has been previously used for the same reason by Cappellari & Jenkins (2008a, 2014) in their analysis of social assistance receipt dynamics in Britain.[12] Chapter 4 provides an assessment of the implications of using annual data for the study of state dependence in benefit receipt.

2.3 Data used

The data for the 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. The last wave currently available is for 2011. Over time, the sample size increased from an initial 6,000 households to around 12,300 households and 22,000 individuals in 2011.

[12] An alternative approach frequently used is to define a 'benefit year' by setting the binary social assistance variable equal to one if any positive amount of benefit receipt is recorded during the calendar year. This method is convenient if data come from annual administrative records where information on the amount of benefits received is available but the exact timing of payments during the year is unknown (see Hansen & Lofstrom (2008, 2011), Andrén & Andrén (2013), or Hansen et al. (2014)). For an empirical comparison of the two approaches see Chapter 4.

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

I construct my data set by merging the first eight samples of the SOEP (officially denoted samples A-H).[14] To separately study the benefit dynamics in Western and Eastern Germany, I split this full sample into two subsamples based on the region of residence of the sample members as explained below.

From the full sample, I use the last 17 waves for the years 1995-2011 prior to which no question on the receipt of income-support benefits at the time of the interview was asked. I restrict the sample to working-age individuals (25-59 years) who are not dependent children and without missing information on benefit receipt and a few other important variables. I further drop observations for individuals with a partner who is not of working-age (*i.e.* below 25 or above 59 years), observations for individuals in a household with a working-age member in full-time education, and all observations after a gap in an individual's interview sequence.[15] Excluding the initial observation in

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

[14]The samples are labelled "Residents in the Federal Republic of Germany" (sample A), "Foreigners in the Federal Republic of Germany" (B), "German Residents in the German Democratic Republic" (C), "Immigrants" (D), "Refreshment" (E), "Innovation" (F), "High Income" (G), and "Refreshment" (H).

[15]The motivation for dropping households with adult full-time students is to avoid having to deal with the labour market entry of these individuals. This criterion however concerns only few observations because it is applied after having removed dependent children and individuals below the age of 25 from the sample.

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each individual's interview sequence for which no lag is available, the resulting estimation sample consists of 17,733 individuals and 100,434 person-year observations. A breakdown of the size of the estimation sample across waves and an overview of the lengths of individuals' observation periods is provided in Appendix B, Tables B.1 and B.2.

I match the sample with annual data on unemployment rates in the individuals' state of residence from the German Federal Statistical Office (Statistisches Bundesamt, 2013). These data are used to control for differences in regional labour market conditions in the econometric analysis.[16]

2.4 Trends in benefit receipt

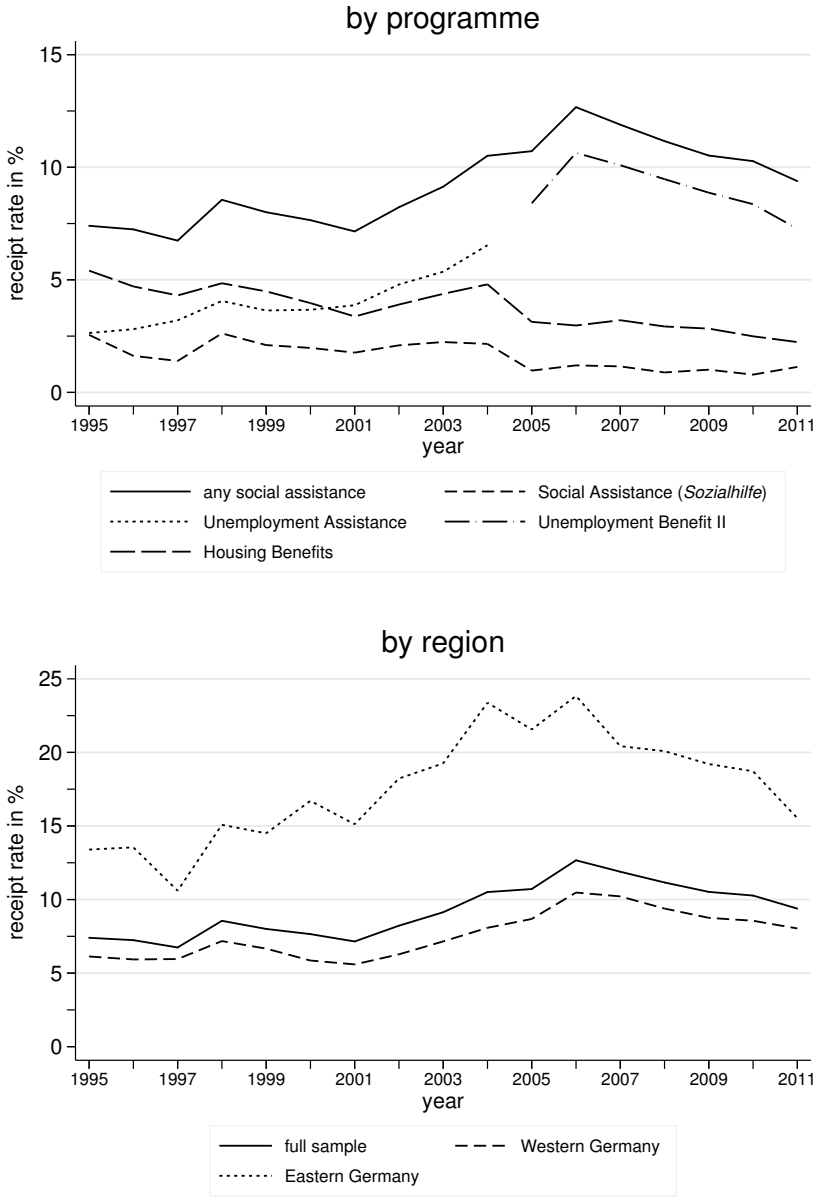
Germany has seen a slight rise in rates of social assistance benefit receipt over the 17 years of the observation period. As illustrated in the top panel of Figure 2.1, the frequency of benefit receipt among working-age individuals is initially relatively stable at around 7-8%. After 2001, rates of benefit receipt start rising strongly to peak at 12.7% in 2006. The beginning of this increase coincides with the start of a period of economic stagnation in Germany in the early 2000s. After 2006, the year after the Hartz reforms, the frequency of benefit receipt declines through the years of the Great Recession and drops to below 10% in 2011.

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

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

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Figure 2.1: *Rates of benefit receipt*



Note: Rates of benefit receipt were calculated using cross-sectional individual sampling weights. The frequency of benefit receipt is the share of working-age individuals who live in a benefit-receiving household at the time of the interview. The final package of the Hartz reforms entered into force in January 2005. *Source*: SOEP, 2011

UBII through the Hartz reforms. Similarly, HB receipt rates fall as recipients who are transferred from UA to UBII lose eligibility to HB. The decline in receipt rates after 2006 is primarily due to a reduced number of UBII recipients.

Patterns of benefit receipt still differ considerably between Eastern and

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Western Germany. As illustrated in the bottom panel of Figure 2.1, receipt rates in Eastern Germany are substantially higher averaging 17.6% compared to 7.6% for Western Germany. This difference is broadly comparable to the disparity in unemployment rates in the two parts of the country.[17] Benefit receipt rates in Eastern Germany show a weak upward trend even in the initial years of the panel and peak already in 2004. Receipt rates for Western Germany closely follow those for Germany overall, which reflects the fact that about 80% of observations in the full sample are for Western Germany.

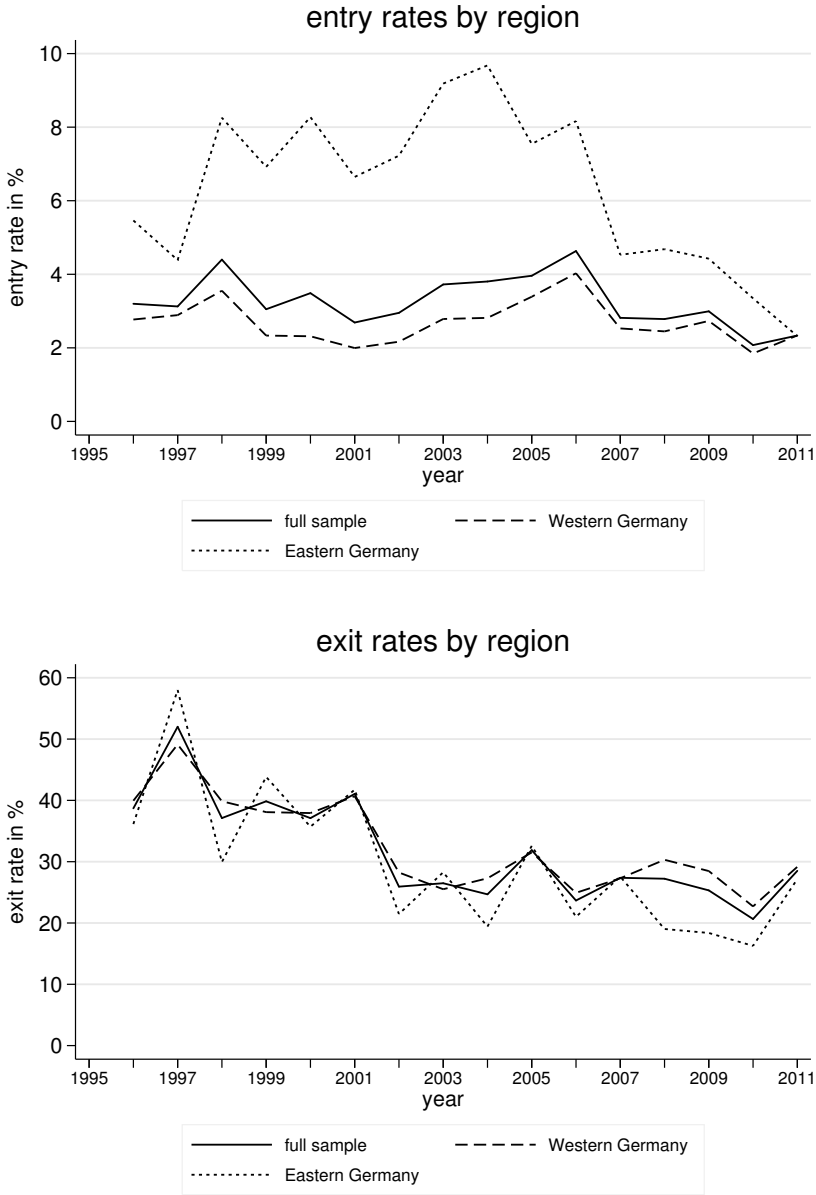
Benefit transition rates plotted in Figure 2.2 show that the rise in the benefit receipt rates observed after 2001 seems to have been primarily due to a permanent drop in exit rates from benefit receipt. The share of individuals who report leaving benefits from one interview to the next falls from around or above 40% until 2001 to below 30% thereafter (bottom panel). This is remarkable, since earlier comparable work for Canada (Finnie & Irvine, 2008) and Britain (Cappellari & Jenkins, 2008a, 2014) shows that declining rates of benefit receipt in these countries where primarily driven by falling entry rates while exit rates remained stable or declined as well. The rise in receipt rates in Eastern Germany during the late 1990s and, more importantly, the decline in receipt rates after 2006 appear to be due to changes in entry rates (top panel).

The breakdown of benefit transition rates by region again shows that patterns differ in the two parts of the country. Exit rates are nearly identical with slightly stronger fluctuations for Eastern Germany due to the much smaller sample size. Entry rates into benefit receipt by contrast are up to four times higher in Eastern Germany than in Western Germany. They also show much more variation in Eastern Germany, rising from around 5% to 8% in 1998 and dropping again by the same amount from 2006 to 2007. The gap in social assistance receipt rates between Eastern and Western Germany shown in Figure 2.1

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

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Figure 2.2: *Benefit transition rates*



Note: Entry rates into benefit receipt are defined as the number of individuals in receipt of social assistance benefits at time *t* but not at time *t-1* divided by the total number of individuals not in social assistance at time *t-1*. Similarly, exit rates from benefit receipt are defined as the number of individuals in receipt at time *t-1* but no longer in receipt at time *t* divided by the total number of individuals in receipt at time *t-1*. Individuals observed for only one of the two waves have not been used in the calculations. Benefit transition rates were calculated using cross-sectional individual sampling weights for period *t*. The final package of the Hartz reforms entered into force in January 2005. *Source*: SOEP, 2011

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is thus due to much higher entry rates in Eastern Germany.

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

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

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

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2.5 Econometric approach

The econometric analysis is based on a dynamic random-effects probit model, the standard model in recent empirical work on state dependence in social assistance benefit receipt. This section introduces the model and proposes two simple extensions that permit studying time-variations in state dependence.

Let y_{it} be a binary outcome variable such that for $y_{it} = 1$ individual i is in receipt of social assistance in period t. A latent variable specification for this outcome can be written as

$$\begin{aligned} y_{it} &= \mathbb{1}\left\{y_{it}^* > 0\right\} \\ &= \mathbb{1}\left\{x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \mu_t + u_{it} > 0\right\} \\ &\quad \text{for} \;\; i = 1, ..., N; \; t = 1, ..., T_i, \end{aligned} \tag{2.1}$$

where y_{it}^* depends linearly on a vector of observable characteristics $x_{i(t-1)}$, the observed receipt status in the previous period $y_{i(t-1)}$, a period-specific effect μ_t and an error term u_{it}.[18] The latent variable y_{it}^* can be interpreted as the potential utility from receiving social assistance, with the individual choosing benefit receipt for $y_{it}^* > 0$.

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

[18] An alternative specification of the model uses current values of the observable characteristics x_{it}. In using lagged values $x_{i(t-1)}$, I follow Cappellari & Jenkins (2014). The difference in results between the two approaches is modest.

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that the covariates $x_{i(t-1)}$ are strictly exogenous.[19]

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

$$P(y_{it}|y_{i0},...,y_{i(t-1)},x_i,\mu_t,\alpha_i) = \Phi(x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \mu_t + \alpha_i), \quad (2.2)$$

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

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

A difficulty for estimation of this model is that the specification suffers from an initial conditions bias. The individual-specific error component α_i induces a correlation between the error term and the lagged dependent variable thus leading to inconsistent estimates. While for linear models, a transformation through first-differencing and subsequent use of instrumental variable techniques can address this problem (Anderson & Hsiao, 1981; Arellano & Bond, 1991), the individual-specific error component in a random-effects probit model needs to be integrated out. This requires specifying its relationship with the outcome in the initial period y_{i0}, and in most cases y_{i0} cannot be treated as exogenous.

The simplest approach for addressing the initial conditions problem has been proposed by Wooldridge (2005).[20] He suggests specifying a density for the

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

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

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individual-specific effect conditional on the outcome in the initial period and the covariates, which permits integrating out α_i. More specifically, Wooldridge sets $\alpha_i = \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i$, with $a_i|y_{i0}, x_i \sim \mathcal{N}(0, \sigma_a^2)$. The vector x_i contains here the values of time-varying covariates for all periods not yet already included in $x_{i(t-1)}$ and allows for a correlation of α_i with the covariates as proposed by Chamberlain (1982, 1984). Under this assumption, the joint density of $y_{i1}, ..., y_{iT_i}|y_{i0}, x_i$ unconditional on α_i simply corresponds to the likelihood of the standard random-effects probit model with the additional explanatory variables y_{i0} and x_i added in each period t, and it can be used for maximum likelihood estimation. In empirical practice, the vector of lags and leads of all time-varying covariates x_i is typically replaced by an individual's longitudinal averages of these covariates $\bar{x}_i$ à la Mundlak (1978) to reduce the number of regressors and thus computation time. This is also what I do in this chapter.[21] Consistency of this model relies on the assumption that unobserved heterogeneity is uncorrelated with the regressors once between-individual differences in observable characteristics are accounted for.

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

$$\frac{1}{N}\sum_{i=1}^{N}\left(\Phi(x_{i(t-1)}'\hat{\beta} + \hat{\lambda}y_{i(t-1)} + \hat{\mu}_t + \hat{\gamma}_1 y_{i0} + \bar{x}_i'\hat{\gamma}_2)(1-\hat{\rho})^{\frac{1}{2}}\right), \tag{2.3}$$

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

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

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

$$APE = \frac{1}{NT_i}\sum_{t=1}^{T_i}\sum_{i=1}^{N}\Big[\hat{P}\left(y_{it}=1|y_{i(t-1)}=1,x_{i(t-1)},\mu_t,y_{i0},\bar{x}_i\right) - \hat{P}\left(y_{it}=1|y_{i(t-1)}=0,x_{i(t-1)},\mu_t,y_{i0},\bar{x}_i\right)\Big]. \quad (2.4)$$

The APE measures structural state dependence in absolute terms. One advantage of looking at the APE as opposed, for instance, to the entire distribution of predicted probabilities is that it does not require predictions of the individual-specific error components a_i.

One can express the degree of state dependence in relative terms by calculating the predicted probability ratio (PPR), *i.e.* the ratio of average predicted probabilities with and without benefit receipt in the previous period:

$$PPR = \frac{\frac{1}{NT_i}\sum_{t=1}^{T_i}\sum_{i=1}^{N}\hat{P}\left(y_{it}=1|y_{i(t-1)}=1,x_{i(t-1)},\mu_t,y_{i0},\bar{x}_i\right)}{\frac{1}{NT_i}\sum_{t=1}^{T_i}\sum_{i=1}^{N}\hat{P}\left(y_{it}=1|y_{i(t-1)}=0,x_{i(t-1)},\mu_t,y_{i0},\bar{x}_i\right)}. \quad (2.5)$$

To study time-variation in the level of state dependence, I estimate two different extensions of the standard model. First, I directly compare the level of state dependence before and after the Hartz reforms by re-writing the model as

$$y_{it} = \mathbb{1}\left\{x'_{i(t-1)}(\beta^0+\beta^1 H_t)+(\lambda^0+\lambda^1 H_t)y_{i(t-1)}+(\gamma_1^0+\gamma_1^1 H_t)y_{i0}+\bar{x}'_i(\gamma_2^0+\gamma_2^1 H_t)+a_i>0\right\} \quad \text{for} \quad i=1,...,N;\ t=1,...,T_i, \quad (2.6)$$

where H_t is a dummy variable for the post-Hartz period that takes the value one in the years 2005-2011 and is zero otherwise. This specification allows for differences in the processes driving benefit receipt before and after the Hartz

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reforms by letting the coefficients of the lagged dependence variable $y_{i(t-1)}$, of the covariates $x_{i(t-1)}$ and of the Wooldridge controls y_{i0} and $\bar{x}_i$ differ between those two periods. Unlike the standard specification, this model no longer includes a set of year dummies μ_t. The time trend is instead captured by an interaction of the post-Hartz dummy with the intercept term included in $x_{i(t-1)}$. Unobserved heterogeneity is assumed not to vary over time.

Second, I analyze the timing of any variations in state dependence by allowing the effect of the lagged dependent variable on the likelihood of benefit receipt to vary on a year-by-year basis. I again extend the standard specification, this time including interactions between the lagged dependent variable and calendar-year dummies. The model can be written as

$$\begin{aligned} y_{it} &= \mathbb{1}\left\{ x'_{i(t-1)}\beta + \lambda y_{i(t-1)} + \sum_{\tau=1997}^{2011} (\mu^\tau + \lambda^\tau y_{i(t-1)}) + \gamma_1 y_{i0} + \bar{x}'_i\gamma_2 + a_i > 0 \right\}, \\ &\quad \text{for } \; i = 1, ..., N; \; t = 1, ..., T_i. \end{aligned} \tag{2.7}$$

where μ^τ and λ^τ are the coefficients of a set of calendar-year dummy variables and of their interactions with the lagged dependent variable, respectively. The effects of all other variables are assumed to be constant over time.[22] This specification is more flexible than the one given in Equation (2.6) in allowing for richer variation in the effect of lagged benefit receipt. It is more restrictive in imposing that the coefficients of other covariates do not change over time.

To quantify variations in the degree of state dependence, I predict entry and persistence rates for each individual in the sample *in each of the periods distinguished in the model* (*i.e.* also for those periods in which the individual is not observed). This is done by 'switching on' the dummy for the respective period and its interaction term (and 'switching off' period dummies and interaction terms of all other periods) for all individuals. I then use the model estimates to predict period-specific entry and persistence rates across all individuals as usual. Time-variation in state dependence is given as the difference in APEs

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

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between two periods, *i.e.* as the 'difference in differences' between the average predicted persistence and entry rates across all individuals in two periods.[23]

The results presented in the next section are obtained using Stata's `xtprobit` command, which employs adaptive quadrature with twelve quadrature points for evaluation of the integrals. As a robustness check, all specifications have been re-estimated using Stata's `gllamm` command that allows for robust standard errors and the use of sampling weights (Rabe-Hesketh, Skrondal & Pickles, 2004, 2005), but for which computation time is substantially higher. I find that the use of sampling weights leads to larger standard errors but does not strongly affect coefficient estimates and the resulting APEs. I therefore use weights only for the calculation of APEs and PPRs but not in the estimation process. For the baseline specifications, I report results from weighted estimation in Appendix D, Tables D.1 and D.2. Results from weighted estimation of other specifications are provided in a working paper version of this chapter (Königs, 2013).

2.6 Estimation results

This section presents estimation results for the full sample and separately for Western and Eastern Germany. Covariates used in the estimation consist of personal characteristics (sex, age, years of education, health status, and migrant status), household characteristics (household type, a dummy for the presence of a child aged six years or younger in the household, and household size), and partner characteristics (age, years of education, health status, and migrant status).[24] In the specification for the full sample, I control for region of residence

[23]More specifically, in the second model extension described by Equation (2.7), the difference between the predicted entry rates in year τ and the initial year 1996 is captured by the time effect μ^τ, while the difference in predicted persistence rates between the two periods is captured by $\mu^\tau + \lambda^\tau$. The magnitude of the time-variation in predicted persistence and entry rates (and the difference between the two) depends on the values of the covariates and thus varies across individuals.

[24]Partner characteristics are set equal to zero if the individual is single. This is equivalent to interacting partner variables with a dummy that indicates whether a person is partnered or not: For a person with partner the interaction takes the value of the partner's variable,

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using a dummy variable for Eastern Germany. I moreover include a variable measuring the annual state-level unemployment rate in all specifications to capture regional and time differences in the economic environment. For an overview of the definitions of these variables, see Appendix A, Table A.1. Some descriptive statistics are provided in Appendix B, Table B.3.

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

The division of the sample into Western and Eastern Germany is based on residence in the initial period in which an individual is observed. This is meant to help avoid possible endogeneities that might arise as sample members move from one part of the country to another, although such moves are infrequent.[25]

2.6.1 The evidence for state dependence

Results from the standard version of the dynamic random-effects probit model described in the previous section indicate that there is considerable state dependence in social assistance benefit receipt in Germany. Column I of Table 2.2, which gives coefficient estimates, shows that the coefficient of the lagged dependent variable is positive and strongly significant for the full sample. Table 2.3 presents the corresponding average predicted transition rates: I calculate an average predicted entry rate of 5.4% and an average predicted persistence rate of 18.4%. The resulting APE is 13.0 percentage points.

for an individual without partner the interaction term is zero.

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

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The result implies that even after controlling for observed and persistent unobserved characteristics, an individual in the sample is on average 13 percentage points more likely to report benefit receipt at the current interview if she already received benefit payments at the last interview. This corresponds to an increase in the probability of benefit receipt by a factor of 3.4 as indicated by the PPR. While an APE of 13 percentage points is substantial, the value is considerably lower than the difference between observed persistence and entry rates of about 65 percentage points shown in Figure 2.2. Most of the 'raw' state dependence is thus due to observed and unobserved heterogeneity across individuals.

Results for Western and Eastern Germany show strong disparities in average predicted transition rates, but relatively similar levels of state dependence in absolute terms. Coefficient estimates for the lagged dependent variable are significantly positive again in both subsamples (Columns II and III of Table 2.2). Average predicted entry and persistence rates for Western Germany are very close to, but lower than, those for the full sample. For Eastern Germany, both predicted entry and persistence rates are substantially higher as one would expect given the much higher regional unemployment rates (Columns II and III of Table 2.3). State dependence in Western and Eastern Germany is comparable when measured in absolute terms at 13.5 percentage points in Western Germany and 15.2 percentage points in Eastern Germany. In relative terms, the effect of past benefit receipt is however much stronger for Western Germany where receipt rates are much lower: The PPR implies that benefit receipt at the time of the last interview raises the likelihood of benefit receipt at the current interview by a factor of 4.2 for Western Germany compared to 2.2 in Eastern Germany.

The choice of not having used sampling weights in the estimations only has very little effect on measured state dependence. When re-running the same three specifications with individual-level weights that correct for differences in sampling probabilities and attrition, the level of estimated state dependence

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drops to 12.8 percentage points (from 13.0) for the full sample, to 13.1 percentage points (from 13.5) for Western Germany and to 14.6 percentage points (from 15.2) for Eastern Germany (Appendix D, Table D.2).

Table 2.2: *Baseline specifications – coefficient estimates*

dependent variable: SA_t	Full sample		Western Germany		Eastern Germany	
SA_{t-1}	1.160***	(0.029)	1.259***	(0.036)	0.977***	(0.048)
individual characteristics						
female	-0.004	(0.029)	-0.010	(0.033)	0.023	(0.057)
age	-0.078***	(0.013)	-0.077***	(0.015)	-0.080***	(0.025)
age^2	0.088***	(0.015)	0.087***	(0.017)	0.089***	(0.029)
years of education	-0.293***	(0.042)	-0.252***	(0.045)	-0.725***	(0.127)
years of education^2	0.007***	(0.002)	0.006***	(0.002)	0.021***	(0.005)
good health	-0.060**	(0.028)	-0.070**	(0.034)	-0.027	(0.051)
poor health	0.080**	(0.035)	0.097**	(0.041)	0.020	(0.066)
migrant	0.268***	(0.045)	0.261***	(0.046)	0.263	(0.186)
household characteristics						
single, with children	0.029	(0.064)	0.100	(0.082)	-0.043	(0.104)
couple, no children	0.061	(0.073)	0.095	(0.089)	0.022	(0.132)
couple, with children	-0.076	(0.075)	-0.061	(0.092)	-0.047	(0.131)
child $\leq$ 6 years	0.096**	(0.039)	0.166***	(0.045)	-0.083	(0.076)
household size	0.062***	(0.022)	0.047*	(0.026)	0.108**	(0.043)
partner characteristics						
age	-0.018***	(0.004)	-0.016***	(0.005)	-0.021***	(0.008)
age^2	0.022***	(0.007)	0.020***	(0.008)	0.026*	(0.013)
years of education	0.056***	(0.012)	0.046***	(0.014)	0.086***	(0.025)
years of education^2	-0.005***	(0.001)	-0.004***	(0.001)	-0.006***	(0.001)
good health	0.003	(0.031)	0.002	(0.038)	0.018	(0.057)
poor health	0.155***	(0.041)	0.175***	(0.048)	0.097	(0.076)
migrant	0.163***	(0.048)	0.163***	(0.049)	0.504**	(0.201)
calendar-year effects						
1997	-0.116**	(0.055)	0.061	(0.064)	-0.525***	(0.108)
1998	0.039	(0.058)	0.097	(0.070)	0.049	(0.114)
1999	-0.100*	(0.057)	-0.016	(0.068)	-0.155	(0.115)
2000	-0.076	(0.058)	-0.100	(0.070)	0.057	(0.111)
2001	-0.083	(0.052)	-0.108*	(0.062)	0.066	(0.104)
2002	-0.081	(0.053)	-0.130**	(0.065)	0.114	(0.106)
2003	0.097*	(0.053)	0.068	(0.063)	0.278***	(0.107)
2004	0.080	(0.055)	0.054	(0.065)	0.316***	(0.115)
2005	0.129**	(0.056)	0.169***	(0.065)	0.229**	(0.116)
2006	0.190***	(0.060)	0.195***	(0.075)	0.410***	(0.118)
2007	-0.018	(0.058)	0.043	(0.071)	-0.005	(0.111)
2008	0.049	(0.057)	0.015	(0.069)	0.134	(0.105)
2009	0.136**	(0.060)	0.158**	(0.072)	0.019	(0.111)
2010	0.155**	(0.063)	0.095	(0.076)	0.208*	(0.115)
2011	0.084	(0.068)	0.108	(0.080)	-0.084	(0.130)
Wooldridge controls						
SA_0	1.268***	(0.048)	1.222***	(0.059)	1.335***	(0.086)
avg: good health	-0.110**	(0.055)	-0.080	(0.066)	-0.192*	(0.103)

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Table 2.2 – continued from previous page –

	Full sample		Western Germany		Eastern Germany	
avg: poor health	0.308***	(0.071)	0.332***	(0.083)	0.217	(0.143)
avg: single, w. children	0.304***	(0.093)	0.245**	(0.113)	0.392**	(0.173)
avg: couple, no children	-0.360***	(0.090)	-0.447***	(0.108)	-0.250	(0.167)
avg: couple, w. children	-0.267***	(0.098)	-0.358***	(0.116)	-0.107	(0.187)
avg: child $\leq$ 6 years	0.267***	(0.064)	0.192***	(0.073)	0.525***	(0.133)
avg: household size	0.059**	(0.028)	0.086***	(0.032)	-0.030	(0.058)
avg: reg. unempl. rate	0.024**	(0.010)	0.042**	(0.018)	0.005	(0.017)
avg: good health (ptnr.)	-0.096*	(0.057)	-0.024	(0.068)	-0.227**	(0.107)
avg: poor health (ptnr.)	0.209***	(0.079)	0.260***	(0.092)	0.115	(0.161)
reg. unemployment rate	0.037***	(0.009)	0.021	(0.017)	0.007	(0.016)
Eastern Germany	0.247***	(0.056)				
constant	0.923**	(0.375)	0.549	(0.423)	5.096***	(0.998)
σ_a	0.831***	(0.025)	0.773***	(0.030)	0.959***	(0.047)
ρ	0.409***	(0.014)	0.374***	(0.018)	0.479***	(0.025)
log Likelihood	-14,538.474		-9,671.444		-4,776.992	
# of observations	100,434		79,790		20,644	
# of individuals	17,733		14,010		3,723	

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

Note: Standard errors are given in parentheses. SA_t, SA_{t-1} and SA_0 are the observed social assistance receipt status in the current, the last period and the initial observed period, respectively. All other covariates with the exception of calendar-year dummies are lagged by one period. 'avg:' denotes an individual's longitudinal average of a variable. Values of age^2 have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011

Table 2.3: *Baseline specifications – predicted transition rates, APE and PPR*

	Full sample	Western Germany	Eastern Germany
Average predicted entry rate in %	5.4 (0.2)	4.2 (0.1)	12.3 (0.6)
Average predicted persistence rate in %	18.4 (0.7)	17.7 (0.9)	27.5 (1.2)
Average partial effect (in ppts)	13.0 (0.7)	13.5 (0.9)	15.2 (1.3)
Predicted probability ratio (in ppts)	3.4 (0.2)	4.2 (0.3)	2.2 (0.1)

Note: Calculations are based on the coefficient estimates presented in Table 2.2. All averages have been calculated using individual cross-sectional sampling weights. Standard errors in parenthesis were obtained by bootstrapping with 100 replications. Results for Western and Eastern Germany are *within-sample* predictions. *Source*: SOEP, 2011

A methodological point worth mentioning is that predicted transition rates for Western and Eastern Germany are *within-sample* predictions in the sense that they have been calculated for the respective subsamples used for estimation

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rather than over all individuals in Western and Eastern Germany combined. A disadvantage of this approach is arguably that results are less comparable, as – due to the non-linearity of the model – they depend on the distributions of observable characteristics in the two subsamples. The reason why I have nonetheless opted for this approach is that results by region can straightforwardly be interpreted as the decomposition of the results for the full sample. By contrast, I found that *out-of-sample* predictions can give very counter-intuitive results. In particular, predicted transition rates calculated over all individuals in the full sample were higher when based on coefficient estimates obtained for either the Western or the Eastern German subsample than when based on estimates from a specification run on the two subsamples combined.

One concern about the results presented in Table 2.2 may be that the panels used for estimation are unbalanced. Indeed, Table B.2 in Appendix B indicates that only 585 persons, or 3% of all individuals, are in the sample for the entire 17-year observation period. The remaining 97% drop out early – by reaching the upper age threshold or through sample attrition –, or join late – in one of the subsamples added after 1995, by reaching the lower age threshold or by moving into the household of a sampled individual. Consistency of the results presented in this paper requires that such movements be uncorrelated with the likelihood of benefit receipt.

Despite the high number of year-on-year sample entries and exits (as documented in Appendix B, Table B.1) and the resulting short observation periods (Appendix B, Table B.2), weighted sample characteristics remain relatively stable (Appendix B, Table B.4). For variables that one would expect to vary little over time – for instance the population's average health status or the share of individuals with Eastern German residence – sample means in the initial and final wave of the 17-year observation period are very similar. Changes are somewhat larger for the share of females in the sample, which may indicate that the weighting procedure does not fully correct for differences in sample attrition between sexes. Time variation is much stronger however for

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the family-type variables, a pattern that likely reflect actual differences in the population studied.

An estimation-based robustness check suggests that the main findings presented in this chapter are not affected heavily by the use of unbalanced panels. As reported in Königs (2013, p. 57), a 'weakly-balanced' panel constructed only from the first ten observations of all individuals observed for a minimum of ten years gives an estimated level of state dependence that is virtually identical to the one reported in Table 2.2 for the full sample. When using a fully-balanced panel, the estimated level of state dependence increases by around 3.6 to 17.7 percentage points; also this effect is relatively modest considering that 97% of all observations have been dropped from the sample. Based on these results, and considering the large loss in precision that would follow from moving to a balanced panel, I believe that the use of unbalanced panels is justified.

The relations between individual-, household-, and partner-level characteristics and the likelihood of benefit receipt tend to have the direction one would assume. Coefficient estimates for Western and Eastern Germany are generally similar with larger standard errors for Eastern Germany as a result of the lower sample size. I therefore limit the discussion to estimates for the full sample.

Starting with the individual-level explanatory variables, a surprising finding is that, *ceteris paribus*, the sex of the respondent does not seem to be related to the risk of benefit receipt as indicated by the insignificant coefficient for the female dummy.[26] The effect of age on the outcome variable is u-shaped with a minimum at age 44 implying that young adults and older individuals have a higher probability of benefit receipt. Education is associated negatively with social assistance receipt at a slightly diminishing rate for higher years of education as suggested by the positive coefficient on the quadratic term (the minimum is at about 21 years of education). As one would expect, poor health is associated with a higher probability of benefit receipt and healthier

[26]This result is largely confirmed when running the model separately for women and men, see Königs (2013).

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individuals are less likely to receive benefits. Even after controlling for personal characteristics, first- or second-generation migrants are significantly more likely to receive social assistance than natives.

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

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

Further down in Table 2.2, the coefficient estimate for the state-level unemployment rate indicates that living in a region with higher unemployment is associated with a higher likelihood of benefit receipt. The positive coefficient

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of the dummy for residence in Eastern Germany implies that even once socio-economic characteristics and the state-level unemployment rate are controlled for, the probability of benefit receipt is higher for individuals living in the east.

The model captures time trends in benefit receipt during the observation period using year dummy variables as covariates (the reference year is 1996). The large majority of year coefficients in the model are insignificant, which suggests that the model does relatively well at explaining the time trends observed in Section 2.4. I obtain significantly positive coefficient estimates for the years 2005 and 2006, which indicates *ceteris paribus* a higher probability of social assistance receipt in the years directly following the Hartz reforms. Yet, this rise in the probability of benefit receipt is transitory as the estimated coefficients for the years 2007 and 2008 are very close to zero again and statistically insignificant. The coefficient estimates for the years 2009 and 2010 are positive and significant suggesting a higher probability of benefit receipt during the economic crisis. While a negative coefficient estimate is observed for the year 1997, there does not appear to be a clear time trend or any systematic difference in the probability of benefit receipt before and after the Hartz reforms.

2.6.2 Time-variation in state dependence

The results presented thus far suggest that social assistance benefit receipt in Germany is characterized by a substantial degree of state dependence. As outlined in Section 2.2 the institutional framework for the provision of minimum-income benefits in Germany underwent a major reform during the observation period. Since one explicit aim of these reforms was to strengthen the activating elements in social assistance, an interesting question is whether based on the models I can find any evidence for changes in the degree of state dependence around the time of the Hartz reforms.

I start my analysis of time-variation in state dependence by explicitly comparing the the periods before and after the Hartz reforms. For this purpose, I

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estimate the model described in Equation (2.6), which interacts the lagged dependent variable and all covariates with a dummy variable for the post-Hartz years. Table 2.4 presents average predicted persistence and entry rates derived from this model for the two periods. Coefficient estimates are reported in Appendix C, Table C.1 but not discussed in detail.

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

The predicted increase in state dependence for the full sample described in Table 2.4 is entirely due to a change in the coefficient estimates of the covariates, in particular the intercept term. Unlike for Eastern Germany, the coefficient estimate of the lagged dependent variable does not vary over time in the specification for the full sample as illustrated by the insignificant interaction term between the lagged dependent variable and the post-Hartz dummy reported in Table C.1. The increase in state dependence in the data is thus entirely captured by the non-linearity of the model. This non-linearity means that changes in the effect of observable characteristics and the intercept term indirectly affect the impact of the lagged dependent variable and thus the estimated degree of state dependence. When using a non-linear model to evaluate changes in state dependence over time by interacting the lagged dependent variable with a time dummy, it is thus not sufficient to focus alone on the significance of the interaction term if the model also includes a time trend.

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Table 2.4: *Pre- vs. post-Hartz variation in state dependence*

- Full sample -

	1996-2004	2005-2011	Δ
Average predicted entry rate in %	5.2 (0.2)	6.2 (0.2)	1.0*** (0.2)
Average predicted persistence rate in %	16.6 (0.7)	20.2 (1.0)	3.6*** (0.9)
Average partial effect	11.5 (0.7)	14.0 (0.9)	2.6*** (0.9)
Predicted probability ratio	3.2 (0.2)	3.3 (0.2)	0.0 (0.2)

- Western Germany -

	1996-2004	2005-2011	Δ
Average predicted entry rate in %	3.9 (0.2)	4.7 (0.2)	0.8*** (0.2)
Average predicted persistence rate in %	16.3 (0.9)	18.8 (1.2)	2.5** (1.2)
Average partial effect	12.4 (0.9)	14.0 (1.2)	1.7 (1.2)
Predicted probability ratio	4.5 (0.3)	4.3 (0.3)	-0.2 (0.3)

- Eastern Germany -

	1996-2004	2005-2011	Δ
Average predicted entry rate in %	12.0 (0.6)	13.8 (0.8)	1.8*** (0.7)
Average predicted persistence rate in %	24.8 (1.3)	30.8 (1.9)	6.1*** (2.0)
Average partial effect	12.7 (1.4)	17.0 (1.8)	4.3** (1.9)
Predicted probability ratio	2.1 (0.1)	2.2 (0.2)	0.2 (0.2)

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

To assess whether the increase in state dependence over the observation period indeed happened around the time of the Hartz reforms, I estimate the alternative specification described by Equation (2.7). This model allows the coefficient of the lagged dependent variable to vary on a year-by-year basis while restricting the coefficients of all covariates to be constant over time. The latter simplification however does not appear too problematic given that in the model with the post-Hartz interactions most of the time-variation in benefit dynamics was due to a change in the intercept term (see the coefficient estimates presented in Table C.1). Figures 2.3-2.5 plot the predicted yearly average entry

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and persistence rates (top panels) and the year-by-year time-variation in the APEs compared to the initial year 1996 (bottom panels). Coefficient estimates for the specification are reported in Appendix C, Table C.2.

Figure 2.3 shows changes in predicted transition rates and state dependence for the full sample. I find that entry rates in benefit receipt are very stable over time at around 5% per year with increases to 6% in 2005-6 and 2010-11. The average predicted persistence rate drops from 19% in 1996 to below 13% in 1997, but then starts rising to reach 20% in 2003 and to peak at 26% in 2010.

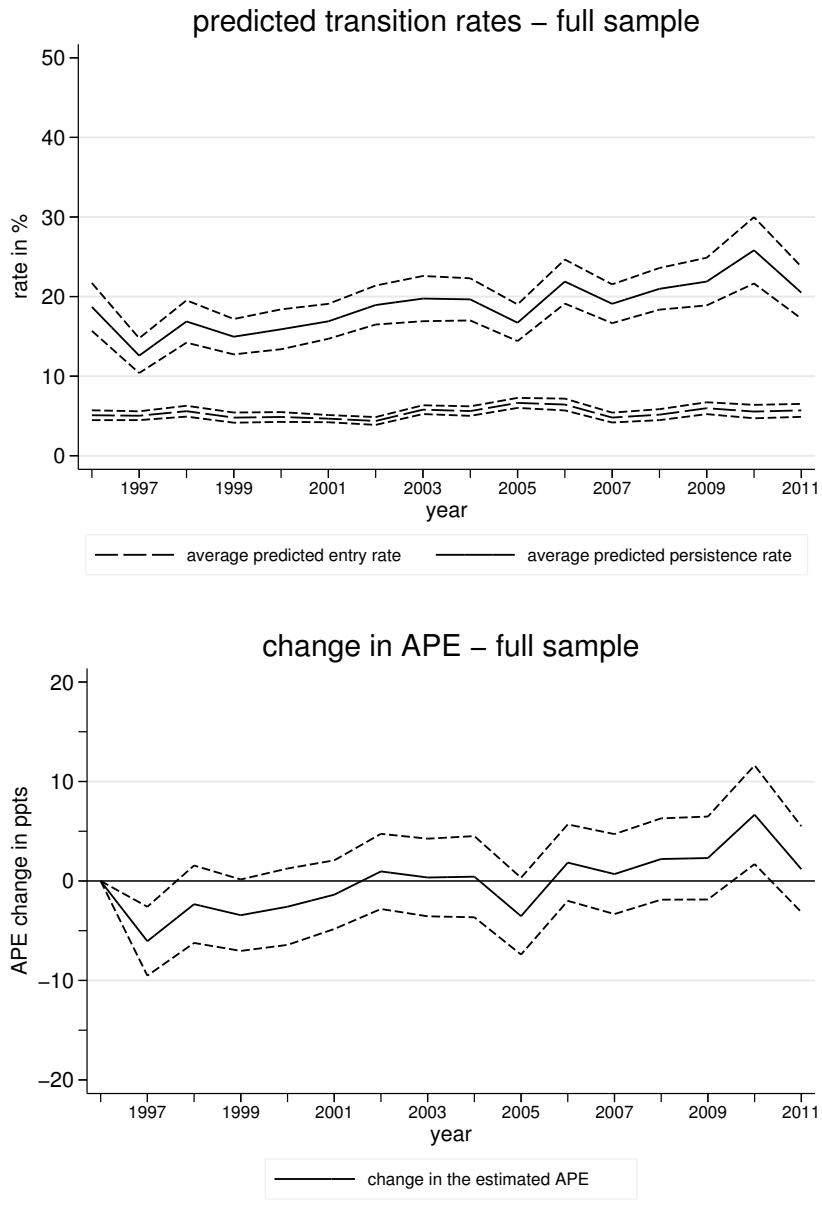
The bottom panel plots the variation in the average partial effect with respect to the reference year 1996, *i.e.* the difference in the vertical distance between average predicted entry and persistence rates in the given year compared to that in the initial year of the observation period. As one would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

The corresponding graphs for Western Germany – shown in Figure 2.4 – offer few additional insights. As observed for other specifications, average predicted persistence and entry rates are very similar to those for the full sample. The same is consequently true also for the year-to-year variations in the average partial effects, now however with none of the calculated annual APEs being significantly different from the one for the initial period.

Time-variation in predicted transition rates appears to be stronger for Eastern Germany (Figure 2.5). Results are however less easy to interpret due to the much smaller sample size and thus much wider confidence bands. Predicted benefit persistence rates are initially only slightly higher than for Western Germany however rise strongly in the early 2000s. They remain relatively stable

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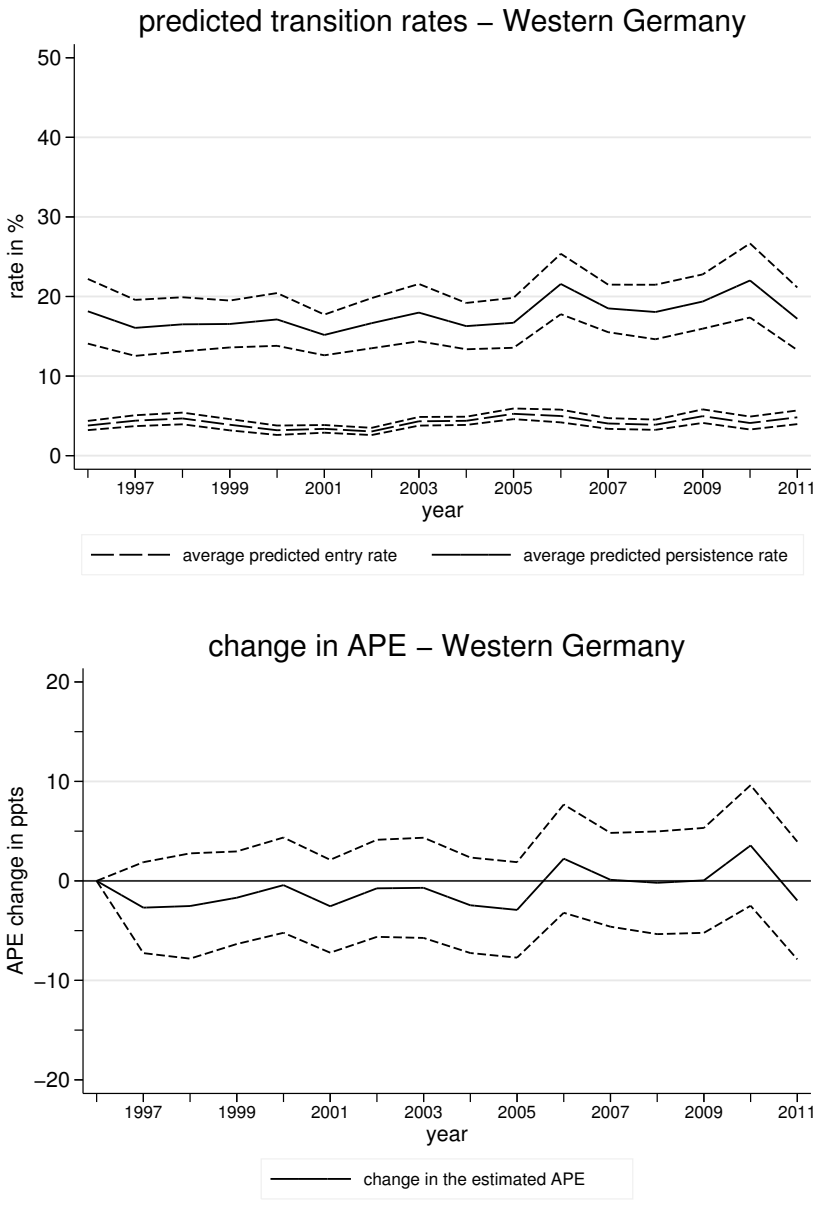
Figure 2.3: *Year-by-year variation in state dependence – full sample*



Note: Calculations are based on the coefficient estimates presented in Appendix C, Table C.2. The top panel shows year-by-year averages of predicted benefit persistence and entry rates. The bottom panel shows the change in the annual average partial effect compared to the one in the initial year 1996. The dotted lines give the 95% confidence intervals. All averages have been calculated using individual cross-sectional sampling weights. Standard errors were obtained by bootstrapping with 100 replications. *Source*: SOEP, 2011

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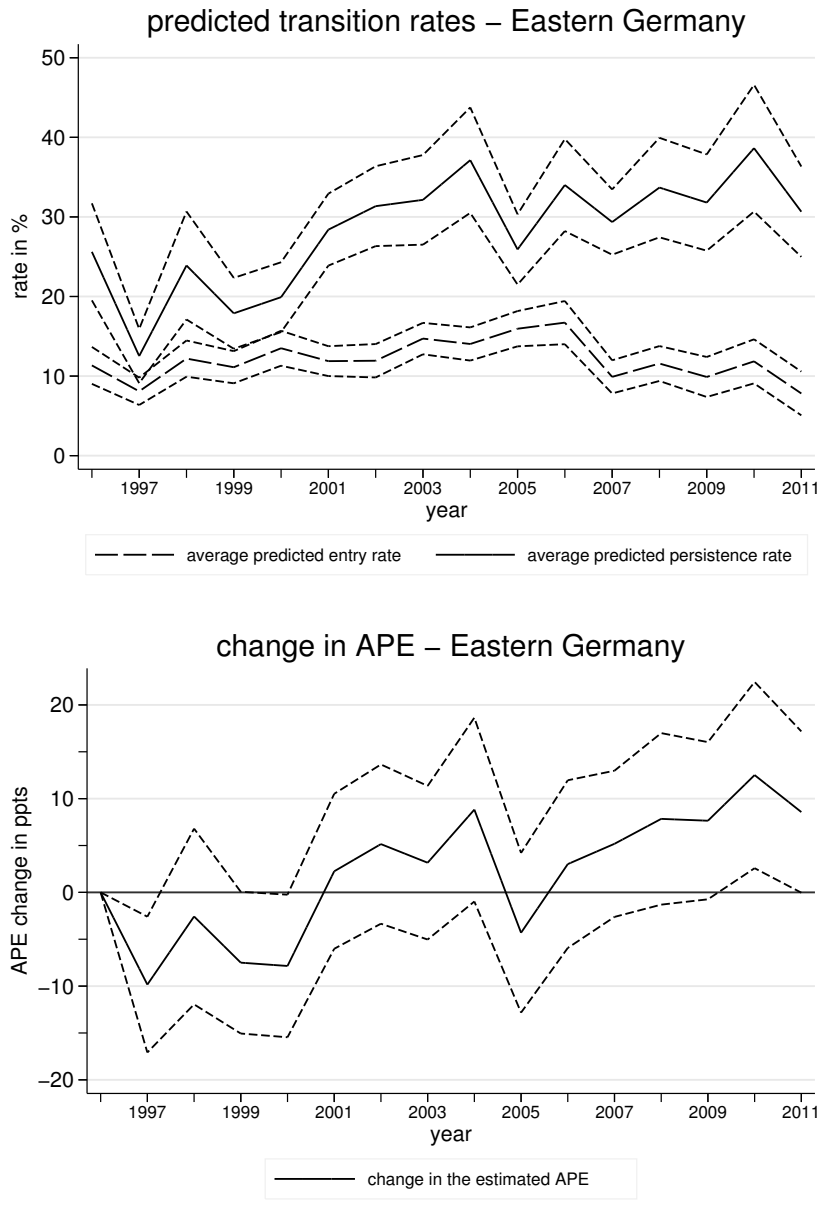
Figure 2.4: *Year-by-year variation in state dependence – Western Germany*



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

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Figure 2.5: *Year-by-year variation in state dependence – Eastern Germany*



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

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

Estimated year-by-year changes in APEs are much more pronounced in Eastern Germany. The APE appears to be around 5 percentage points lower in the late 1990s and around 5 percentage points higher from 2001 than in the initial period. I moreover again find a strong drop in the APE for the year 2005 and a rise up to around 10% for the final years of the observation period. Because of very wide confidence intervals, however, only the drops in the APE for the years 1997 and 2000 and the increase for the year 2010 are statistically significant at the 5% level.

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

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2.6.3 The sensitivity of results to changes in the methodology

The finding of positive state dependence in social assistance receipt confirms similar results from comparable earlier studies for other countries. The APE of 13 percentage points reported in Table 2.3 is for instance relatively close to the value of 14.4 percentage points estimated with the same methodology by Cappellari & Jenkins (2008a) in their analysis for Britain. The findings just reported by contrast differ surprisingly from those presented by Wunder & Riphahn (2014) and Riphahn & Wunder (2013) in two recent papers on state dependence in social assistance receipt for Germany.

Using SOEP data for the years 2005-2009, Wunder & Riphahn (2014) estimate a dynamic multinomial logit model of transitions between receipt of 'welfare' benefits (Unemployment Benefit II), employment and 'inactivity' (which they define as including unemployment).[27] They find evidence of structural state dependence in all three states modelled, the effect size however is much smaller than the one reported in this chapter. For natives, the authors calculate a predicted persistence rate in welfare of 3.1% and predicted entry rates into welfare of 1.6% from inactivity and of 0.5% from employment. For immigrants, the predicted persistence rate is 9.0% compared to an entry rate from inactivity of 3.8% and from employment of 1.8%. The partial effect of past welfare receipt on welfare receipt in the current period lies thus between 1.5 and 2.6 percentage points for natives and between 5.2 and 7.2 percentage points for immigrants (Wunder & Riphahn, 2014, Table 5).[28] Since moreover exit rates to employment are higher from welfare than from inactivity for both natives and immigrants, they conclude that there is no evidence of a 'welfare trap'.

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

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

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

To assess the reason for the difference in findings between the two studies, I use my simpler model described in Equation (2.2) to study the UBII receipt dynamics for the years 2005-2009 only. I then vary the way of defining the benefit variable (household- *vs.* individual-level definition) and the estimation sample (standard sample *vs.* restricted sample of natives in Western Germany without disability).[29] The results of this exercise are presented in Table 2.5.

The level of estimated state dependence in benefit receipt is sensitive to both the approach used for defining the benefit variable and the sample selection criteria. For the standard sample and the household-level definition of the benefit variable, I calculate an average partial effect of 8.3 percentage points. The disparity between this value and the higher 13 percentage points reported for the standard specification in Table 2.3 reflects the lower rate of benefit receipt once Social Assistance and Housing Benefit are no longer considered. Switching from the household-level to the individual-level definition

[29] As in Wunder & Riphahn (2014) 'natives' are defined as sample members who are not first- or second-generation immigrants irrespective of citizenship. I do not follow Wunder & Riphahn's sample selection criteria exactly in that the upper age threshold of 59 years I use is slightly lower than theirs of 65 years.

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of the benefit variable then reduces this APE by nearly half to 4.6 percentage points. The restriction of the sample to natives without disability in Western Germany leads to a further reduction in estimated state dependence. For the household-level definition, the estimated APE declines from 8.3 to 5.2 percentage points; for the specification that uses the individual-level benefit variable, the APE drops from 4.6 to 2.9 percentage points. This APE of 2.9 percentage points is very close to the 1.5 to 2.6 percentage points reported by Wunder & Riphahn for a similar sample and definition of the benefit variable. The APE is moreover no longer significantly different from zero even at the 10% level (t-value of 1.62).

Table 2.5: *The impact of sample selection and the definition of the benefit variable on estimated state dependence*

	Standard sample, 2005-09			Restricted sample, 2005-09		
	Entry rate in %	Persistence rate in %	APE in ppts	Entry rate in %	Persistence rate in %	APE in ppts
Benefit variable defined at the						
household level	6.0 (0.4)	14.2 (1.4)	8.3 (1.5)	3.5 (0.5)	8.7 (1.5)	5.2 (1.7)
individual level	4.5 (0.4)	9.0 (1.1)	4.6 (1.2)	2.7 (0.5)	5.6 (1.6)	2.9 (1.8)
# of individuals		6,769			4,083	
# of observations		22,289			13,249	

Note: The standard sample is defined using the sample selection criteria outlined in Section 2.3. The 'restricted' sample excludes individuals who live in Eastern Germany, disabled individuals, and first- or second-generation migrants. The benefit variable measures receipt of Unemployment Benefit II. For the household-level definition, I categorize an individual as a recipient if benefit payments are recorded for any member of the household; for the individual-level definition, I only use information on benefit receipt reported in the personal questionnaire. Standard errors in parentheses have been obtained from bootstrapping with 100 replications. Coefficient estimates are not reported but available from the author upon request. *Source*: SOEP, 2011

In a recent follow-up study, Riphahn & Wunder (2013) extend their earlier analysis to cover the pre-Hartz years 2001-2004. They again estimate a dynamic multinomial logit model with the same three competing states. For the years prior to the reforms, the state 'welfare' now consists of UA or SA receipt. Unlike in their previous study, where they used information on an individual's benefit receipt from the personal questionnaires only, Riphahn & Wunder now use only the information on benefit receipt from the household questionnaire.

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They then go on to model the benefit transitions of the household head (but not of the other family members). The sample is again restricted to non-disabled individuals in Western Germany.

As in their previous study, they obtain a much lower degree of state dependence, with a partial effect in the post-reform years of 2.5 to 4.4 percentage points for natives and of 3.9 to 7.5 percentage points for immigrants. State dependence in welfare receipt appears to be somewhat higher during the immediate pre-reform years, but it is not obvious whether this effect is statistically significant (Riphahn & Wunder, 2013, Table 5).

Using again my simpler dynamic probit model from Equation (2.2), I find that Riphahn & Wunder's focus on the benefit transitions of the household head alone has little effect on estimated state dependence in the binary case. Instead, lower levels of state dependence reported in their paper may again to be due to sample selection and a subtle difference in the definition of the household-level benefit receipt variable. While Riphahn & Wunder use information on benefit receipt from the household questionnaire only, I also take into account the responses provided by each of the household members in their personal questionnaires (see footnote 11). As it turns out, for about 10% of the households in which UBII receipt is reported by at least one of the household members in their personal questionnaires no benefit receipt is recorded in the household questionnaire. I find that coding these households as non-recipient households and further restricting the sample to non-disabled natives in Western Germany induces a drop in the APE from 9.1 to 2.9 percentage points for the post-Hartz years (*not shown*). This is again much in line with the 2.5 to 4.4 percentage points reported by Riphahn & Wunder.

Overall, these robustness checks confirm that while the conclusions differ, the results presented in this chapter and in the studies by Wunder & Riphahn (2014) and Riphahn & Wunder (2013) can easily be reconciled. In particular, Riphahn & Wunder's finding of only very weak state dependence appears to be due to their focus on able-bodied individuals in Western Germany only

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and the choices they make when defining the social assistance benefit variable. As outlined in Section 2.3, I believe that there are good reasons to define a social assistance benefit variable at the household level. In this case, it also seems justified to take into account all the information provided by the respondents in a household via the household questionnaire and the responses given in the personal questionnaires. Yet, there is no easy way of telling whether a benefit variable that uses information from only one of two sources really underestimates benefit receipt. If benefit receipt is misreported in some of the individual questionnaires, combining data from individual and household questionnaires may lead to an overestimate of receipt rates and hence likely also of state dependence.

For the analysis presented in this chapter, this issue is arguably of lesser importance: UBII is only one of three benefit programmes considered for constructing the social assistance variable, and no information on receipt of SA or HB is provided in the personal questionnaires. Consequently, I find that using Riphahn & Wunder's approach for defining UBII receipt hardly affects the results presented above for a broader definition of the social assistance variable. More generally, the exercise presented in this subsection however highlights the sensitivity of estimation results to the way the benefit variable is defined. Researchers should therefore be very clear in stating what type of information they use for constructing the benefit variable, and potential methodological differences should be taken into account whenever results are compared across studies.

2.7 Conclusion

In this chapter, I have studied state dependence in social assistance benefit receipt in Germany using annual data from the German Socio-Economic Panel (SOEP) for the years 1995 to 2011. Estimating a series of dynamic random-effects probit models that control for observed and persistent unobserved het-

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erogeneity, I found evidence of substantial structural state dependence: Benefit receipt at the last interview is on average associated with a rise in the likelihood of benefit receipt at the current interview by 13 percentage points. This corresponds to an increase by a factor of 3.4. Predicted benefit entry and persistence rates and the resulting absolute level of state dependence are higher in Eastern Germany than in Western Germany. Large disparities between observed and estimated structural state dependence however illustrate that individual characteristics are the most important determinant of benefit transitions.

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

A methodological insight from this chapter is that the magnitude of estimated state dependence can be sensitive to the method used for constructing the benefit variable. For the means-tested Unemployment Benefits II introduced by the Hartz reforms, I showed that defining the benefit variable at the individual rather than at the household level can lead to substantially lower estimated state dependence. Apparent inconsistencies between the information provided in household and personal questionnaires also affect the level of estimated state dependence. An implication of these findings is that my results can be reconciled with those reported in two recent studies by Wunder & Riphahn (2014) and Riphahn & Wunder (2013), who – based on a slightly different methodological approach – find little evidence for state dependence in UBII receipt in post-Hartz Western Germany.

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

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

A different reason for why even a successful policy reform may not translate into a decline in state dependence is that it may lower both benefit entry and persistence rates, for instance as benefit generosity is reduced. Since state dependence is calculated as the difference between predicted persistence and entry rates, such a reform may not at all affect the level of state dependence. In Eastern Germany, average predicted entry rates into social assistance seem to have fallen after the Hartz reforms while average predicted persistence rates remained relatively stable, inducing an increase in state dependence (though

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most of these changes were not statistically significant). A policy reform that works by keeping individuals off benefits rather than by promoting exits may thus even induce greater state dependence.

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Chapter 3

Micro-level Patterns of Social Assistance Receipt – Evidence from 4 European Countries

3.1 Introduction

In reaction to rising social assistance receipt rates and a growing concern about long-term benefit dependence, many OECD countries – notably the United States, the United Kingdom and Germany – implemented structural reforms to their benefit systems since the mid 1990s. These aimed at inducing a transition from a 'passive' to an 'active' provision of benefits by improving the support available to benefit recipients to help them become self-sufficient and by strengthening their incentives to look for and take up work.

Measures to support or 'activate' unemployed benefit recipients – such as job-search assistance, counselling or work experience programmes – tend to work best when they are well-targeted and implemented early-on in a benefit spell (Martin, 2000). To be effective, policy interventions should therefore be designed based on evidence about who receives social assistance, what recipient groups are likely to remain on benefits for longest, and what the chances are of remaining self-sufficient for those who manage to leave benefits. While there has been a long interest in studying the determinants of social assistance benefit receipt, remarkably little evidence exists to date on these 'micro-dynamics' of benefit receipt especially in Europe. The principal reason seems to be that the

high-quality micro-level panel data at short observation intervals required for such analyses are rarely available for research purposes.

In this chapter, I describe the patterns of social assistance benefit receipt in four European countries – Luxembourg, the Netherlands, Norway, and Sweden – based on long panels of monthly administrative data.[1] The chapter adds to the literature on social assistance dynamics by providing comparable evidence on the length of benefit spells in these countries. For Luxembourg, the Netherlands and Sweden, this is first such analysis at the national level that I am aware of; for Norway, the chapter updates and extends an earlier analysis by Dahl & Lorentzen (2003a) for the 1990s. A main contribution of the chapter lies moreover in that it exploits the long observation periods of the data sets by studying the frequency of repeat benefit receipt, an issue on which there exists little evidence to date.

The first part of my analysis provides an overview of the frequency in social assistance benefit receipt in the four countries. There has been no uniform trend in benefit receipt over the last decade or so, with receipt rates having declined in the Netherlands and Norway, having remained stable in Sweden, and having risen in Luxembourg. In the late 2000s, benefit receipt rates varied between 2% in Norway and 4% in Luxembourg and the Netherlands. Comparing rates of benefit receipt and transition rates calculated from monthly and annualized data, I find that, unsurprisingly, annualized rates of benefit receipt substantially overestimate the frequency of benefit receipt in any single month of the year. The magnitude of this effect depends on the degree of turnover in benefit receipt, which is much higher in Norway and Sweden than in Luxembourg and the Netherlands.

In the second part of the chapter, I present evidence on the paths that individuals take into and out of social assistance for the years 2001 to 2008.

[1] This chapter is based on my single-authored contribution to a co-authored OECD working paper (*forthcoming*), which is joint work with Herwig Immervoll and Stephen Jenkins (Immervoll et al., 2014).

There is substantial heterogeneity in benefit spell lengths, both within and across countries. In the two Nordic countries, short-term benefit receipt is the norm with median spell durations of only two months, and with only 6% and 11% of spells in Norway and Sweden lasting longer than one year. In Luxembourg and the Netherlands, long-term benefit receipt is frequent: Median spell durations are 14 and nine months, respectively, and 35% and 24% of all benefit spells last 24 months or longer. Benefit leavers are however much more likely to return into benefits in Norway and Sweden: Over the eight-year period, more than two-thirds of benefit recipients in the Nordic countries have multiple spells compared to less than 30% in Luxembourg and the Netherlands. Time until re-entry into benefits is moreover typically very short in Norway and Sweden with more than half of benefit leavers returning into benefits within 3 months after leaving and only around 25% staying off benefits for the first 24 months after an exit from benefits. The total duration of benefit receipt across spells over the eight-year period however remains shorter for recipients in the two Nordic countries with a median 'net duration' of 7 and 10 months in Norway and Sweden, respectively, compared to 23 and 33 months in the Netherlands and Luxembourg.

The remainder of this chapter is structured as follows: Section 3.2 provides a review of the existing work on individual-level patterns of social assistance receipt. Sections 3.3 and 3.4 give a brief overview of the institutional framework in the four countries and present the data sources used in the analysis. In Section 3.5, I describe the aggregate dynamics of benefit receipt, before presenting in Section 3.6 the results of my analysis of the micro-dynamics of benefit receipt. Section 3.7 concludes.

3.2 Relevant literature

The earliest available evidence on the duration of social assistance benefit receipt is based on annual household survey data on the receipt of Aid to Fami-

lies with Dependent Children (AFDC)[2] in the United States. Bane & Ellwood (1983) and O'Neill, Bassi & Wolf (1987) study the receipt of AFDC in the late 1960s and 1970s using data from the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey of Young Women, respectively. Hoynes & MaCurdy (1994) document developments in the distribution of spell lengths over a 20-year period until 1989 again based on PSID data and relate them to changes in recipient characteristics and in the economic environment. In a seminal study, Bane & Ellwood (1994) use again PSID data for the late 1960s to 1980s to study AFDC spell lengths and the return to benefits of welfare leavers. They raise the important methodological point that when measuring spell durations, it is crucial to distinguish between results obtained from a sample of individuals receiving benefits *at a certain point in time* and those *beginning a spell* in that period. This finding is discussed in more detail further below. One common conclusion of these studies is that while there is clear evidence of long-term benefit receipt, the large majority of spells are relatively short. An obvious limitation of studies based on annual data is however that the measurement of spell durations is quite imprecise. For individuals who remain on benefits for prolonged periods, benefit spell lengths will be overestimated as no distinction can be made between single long spells and a series of recurrent shorter spells.

Later studies of U.S. welfare benefit dynamics that use information at the monthly level are typically also based on survey data. A series of articles use data from the Survey of Income and Programme Participation (SIPP): Fitzgerald (1991, 1995) studies AFDC spell lengths over a period of 32 months, Harris (1993) examines to what extent single mothers combine receipt of AFDC with work, and Blank & Ruggles (1994) and Harris (1996) look at re-entries to AFDC benefit receipt among welfare leavers over periods of up to three and up to six years, respectively. Using data from the National Longitudinal

[2]AFDC was the precursor of Temporary Assistance for Needy Families (TANF), which was introduced in 1996.

Survey of Youth (NLSY) for the years 1979 to 1989, Pavetti (1993) compares AFDC spell lengths obtained from monthly and annualized data. She finds that spell durations are indeed overestimated in the annualized data, though the magnitude of this effect is relatively modest.[3]

One concern about these studies is that survey-based data on benefit receipt may not be very reliable. Even though interviews in the SIPP are conducted at four-monthly intervals and thus relatively frequently, Blank & Ruggles (1994) report a 'seam bias problem' observing a disproportionately large number of benefit transitions where interview periods start and end. Similarly, Pavetti (1993) observes that in the NLSY, 22% of all welfare spells last exactly from January to December, and that December endings account of 47% of all spell endings. This casts doubts on the reliability of these results.

A rare example of an early study of welfare benefit dynamics based on monthly data from administrative sources comes from Blank (1989), who, using six years of data from Denver and Seattle for the early 1970s, finds weak evidence for duration dependence in AFDC receipt. Hoynes (2000) presents evidence on receipt of AFDC in California based on administrative data for the period from 1987 to 1992. The spell lengths she finds are relatively similar to those calculated in previous studies that use survey data, with 28% of spells lasting at most six months and 38% lasting over two years. Among benefit leavers, 41% re-enter within two years of leaving.

From the late 1990s, increased availability of data from administrative records for research purposes primarily outside the U.S. has allowed researchers to produce what are arguably more reliable results on the length of benefit spells. For Canada, Barrett & Cragg (1998) study welfare use in the province of British Columbia using administrative data for the 1980s and early 1990s. They find benefit duration to be typically short, with 75% of spells ending

[3]Based on monthly data, Pavetti estimates that 70% of all starting spells last two years or less and that only 7% of spells last eight years or longer. When annualizing these data, she finds that the share of spells lasting no longer than two years drops to 47% while the proportion of spells with a length of eight years or above rises to 12%.

within six months. Repeat benefit receipt however is frequent, with 25% of benefit leavers returning within three months and half of all benefit leavers returning within a year. Wilson (1999) uses administrative data from New Zealand to study benefit receipt for a cohort of welfare receivers from 1993 over a period of five years. While only 5% of benefit recipients remained on benefits for the entire period, more than one-third were still receiving benefits at the end of the observation period. A challenge therefore appears to be not so much to quickly move recipients off benefits, but rather to ensure that they remain self-sufficient after having left.

A series of articles based on fortnightly administrative data from the Australian Longitudinal Data Set (LDS) illustrate again the high frequency of repeated spells and emphasize the importance of considering transfers across different income-support programmes. For the 1995 inflow sample of recipients of means-tested single parent benefits, Gregory & Klug (2003) show that spell lengths tend to be short with 45% of all spells lasting shorter than one year. However, nearly half of all recipients had spells of other types of income-support benefits during the 5½-year observation period (though this includes receipt of Newstart Allowance, Australia's unemployment benefit). Tseng & Wilkins (2003) show that up to one-third of the Australian working-age population touches on at least one of various income-support benefits in a given year; one-sixth of all recipients continuously receive support over the same 5½-year period. Tseng, Vu & Wilkins (2008) find that 'churning' is a typical feature of income-support receipt, with over half of all recipients leaving and re-entering benefits at least once over a five-year period. Repeated cycling into and out of benefits by contrast is not the norm, and less than one-quarter of 'churners' have four or more spells. Transfers between different programme types during a single benefit spell do not occur very often.

Relatively little evidence exists to date on benefit spell lengths in Europe. A series of studies look at benefit receipt duration at the city level using

monthly data for the German city of Bremen (Buhr & Weber, 1998; Leisering & Leibfried, 1999), Bremen and Gothenburg (Gustafsson & Voges, 1998), and a set of eight different European cities (Gustafsson, Müller, Negri & Voges, 2002). The first three studies conclude that social assistance receipt is mostly a temporary phenomenon, whereas Gustafsson et al. find large differences in benefit spell lengths across cities as discussed further below. With observation periods of five years or less, the authors are however limited in their ability to account for repeat spells in benefit receipt. In a recent study on benefit receipt in the Dutch city of Rotterdam, Snel, Reelick & Groenenboom (2013) challenge Leisering & Leibfried's conclusion (drawn for a different city) that episodes of social assistance receipt are typically short. Based on seven years of administrative data starting in 1999, they calculate that nearly two-thirds of benefit spells last longer than a year. One in four recipients has a total (or 'net') benefit duration of five years or more over the seven-year period. Repeat spells by contrast are found to be the exception, with four out of five benefit recipients in 1999 having had only one single spell. Dahl & Lorentzen (2003a) look at the duration of benefit receipt in Norway for the 1995 cohort of social assistance recipients over an eight-year period from 1992 to 1999. Using the same source of administrative data for Norway as I do, they illustrate the point raised by Bane & Ellwood (1994) that sample selection (above all the distinction between samples of starting *vs.* on-going spells) and spell censoring have a strong influence on measured spell lengths.

This chapter contributes to the summarized literature on social assistance spell lengths by presenting comparable evidence for four European countries. The results for the Netherlands and Sweden complement existing studies at the city level by Snel et al. (2013) for Rotterdam and by Gustafsson & Voges (1998) and Gustafsson et al. (2002) for Gothenberg and Helsingborg. The analysis for Norway updates and extends results presented by Dahl & Lorentzen (2003a) for an earlier time period. The study of benefit dynamics for Luxembourg is the first of its kind.

3.3 Institutional background

All four countries included in this study operate relatively generous minimum-income benefit systems for the working-age population.[4] These are meant to guarantee a basic standard of living to individuals with no or very limited income from other sources. Social assistance benefits may represent a person's sole source of income or top up limited earnings from work, unemployment benefits, or other government transfers. The exception is Sweden, where social assistance is generally not paid to supplement earnings from work but where a tax credit exists to support low-income earners.

A defining characteristic of these social assistance programmes is that they are means-tested at the level of the 'core family': A spouse's or cohabiting partner's income and the couple's assets, typically beyond a certain minimum threshold, are taken into account for determining eligibility; benefits are more generous for recipients with dependent children. Benefit withdrawal with any income from work or other types of government transfers is steep: In the Netherlands, Norway, and Sweden, any income earned in addition to benefit payments is fully deducted from the amount of benefits received. In Luxembourg, total income may reach 130% of the guaranteed minimum income; The implied marginal tax rate on any additional income is 100%.

While social assistance programmes are generally understood as providing temporary relief to individuals during periods of economic hardship, the maximum duration of benefit receipt in all four countries is in principle unlimited. Benefit receipt is however conditional on active job search or participation in training measures. These requirements extend beyond the claimant to other adult members of the family.[5]

[4]More detailed information on the institutional framework in these countries, see Königs (2012) for Luxembourg and Gustafsson (2011) for Sweden.

[5]For an overview of eligibility conditions for social assistance in OECD countries, see Immervoll (2010).

Notable differences exist between the four countries in the degree of centralization of the benefit administration. The Luxembourg Guaranteed Minimum Income (RMG – *revenu minimum garanti*) is administered by the National Solidarity Fund (FNS) at the Ministry of Family Affairs and Integration. Minimum-income thresholds are expressed as a set of fixed base rates, the level of which depend on size and composition of the recipient family. These base rates are then augmented by an index factor that is identical for a range of different social parameters including the national minimum wage and pension, and that is adjusted for inflation at an annual basis. Jobless RMG benefit recipients are required to register with the national public employment service and apply for participation in activation measures.

In the Netherlands, the 2004 Work and Social Assistance Act (WWB) brought about a substantial decentralization of the social assistance (or *bijstand*) benefit administration. Municipalities received large budgetary responsibilities along with incentives to move recipients from social assistance into work quickly. While benefit levels continue to be set by national authorities, the administration of benefit payments and activation programmes takes place entirely at the level of the 415 Dutch municipalities. The central government allocates two separate budgets for reintegration measures and benefit payments to the municipalities. Local authorities are entitled to use any money left in the budget for benefit payments for other purposes, while any excess spending on benefit payments has to be covered out of own funds. The funds in the budget for re-integration measures must be used or paid back. Job-search assistance is provided jointly with the public employment service.

In Norway and Sweden, the administration of social assistance benefits is largely decentralized: The Norwegian Social Services Act of 1991 leaves the generosity of benefit levels of Social Economic Assistance (*Økonomisk sosialhjelp*) and the administration of payments largely to the discretion of the municipalities. Municipal governments determine so-called social assistance benefit

norms as guidelines about the level of monthly payments to be made to a 'standard' recipient. In practice, actual payments then depend on the caseworker's assessment of the applicant's needs. Since 2001, the central government additionally provides national social assistance norms, which are updated annually and aim at equalizing the generosity of social assistance benefit payments across municipalities. These national norms however are not binding.

The Swedish Social Assistance (*Ekonomiskt bistånd*), regulated by the Social Services Act of 1982, is financed and administered by the municipalities. The National Board of Health and Welfare (NBHW) provides national social assistance norms that are meant to harmonize the level of benefits across the country. Unlike for Norway, these national norms have been binding since 1998. As important items like the cost of childcare or electricity are however not covered by these national norms, the level of benefit payouts is in practice determined by municipal social assistance norms and the discretion of the local caseworkers.

To cover the costs of housing, low-income families in all four countries can draw on separate housing allowance programmes. Since 2009, Luxembourg pays its low-income residents a Cost of Living Allowance (*allocation de vie chère*), which replaces the earlier Heating Allowance (*allocation de chauffage*). In the Netherlands, low-income families who spend a substantial part of their income on the costs of housing are entitled to receipt of Housing Allowance (*huurtoeslag*; until 2006: *huursubsidie*). Means-tested housing allowances for low-income families also exist in Norway (*bostøtte*) and Sweden (*bostadsbidrag*).

Additional income-support programmes for specific client groups exist in two of the four countries. In the Netherlands, there are three smaller programmes targeted at older working-age individuals, referred to as IOAW, IOAZ, and IOW.[6] The main difference between these programmes and the standard

[6]Two of these programmes take the form of unemployment assistance programmes: The Act on Income Provision for Older and Partially-Disabled Unemployed Persons (IOAW, *Wet Inkomensvoorziening Oudere en gedeeltelijk Arbeidsongeschikte Werkloze Werknemers*) provides income support to unemployed individuals who were 50 years or older when losing their

bijstand is a more generous means-test, especially in terms of the treatment of the applicants' assets. A means-tested Supplementary Benefit (TW, *Toeslagenwet*) is available for recipients of insurance-based benefits who do not reach a certain minimum-income threshold. In Norway, the Transitional Allowance (*Overgangsstønad for enslige forsørgere*) is a separate minimum-income benefit programme targeted at single parents with insufficient means to cover their living expenses.

Due to data limitations and for reasons of comparability, the analysis presented in this chapter focuses only on a country's main social assistance programme for able-bodied working-age individuals: the RMG for Luxembourg, *bijstand* for the Netherlands, Social Economic Assistance for Norway and Social Assistance for Sweden. Housing Benefits or any of the smaller minimum-income benefit programmes, for which receipt dynamics are likely to be different, are not considered.[7] The same applies to programmes targeted at those unable to work.

The results presented in this chapter should therefore not be interpreted as approximating individuals' receipt patterns of income-support benefits more broadly. Many individuals may receive unemployment benefits before entering social assistance, and many will draw on other types of income-support benefits after leaving. If such moves are frequent, as it is described for Australia by Gregory & Klug (2003) and Tseng et al. (2008), the results presented in this

job and who have exhausted their unemployment insurance benefit entitlements. Individuals who were 60 years or older when becoming unemployed and who have exhausted their unemployment insurance entitlements are eligible to claim benefit payments in the framework of the Income Provision for Older Unemployed Persons (IOW, *Inkomensvoorziening Oudere Werklozen*).

The Act on Income Provision for Older and Partially-Disabled Formerly Self-Employed Persons (IOAZ, *Wet Inkomensvoorziening Oudere en Gedeeltelijk Arbeidsongeschikte Gewezen Zelfstandigen*) supplements incomes of individuals aged 55 years or more who have been self-employed but were forced to shut down their business.

[7]For instance, earlier calculations showed that spell lengths for the Norwegian Transitional Allowance tend to be very long. This probably reflects less stringent work requirements in particular for single parents with young children. Housing Benefit dynamics may show a similar pattern due to the typically higher income thresholds especially for families with children.

chapter will give a lower receipt duration and a higher frequency of repeat spells than would be observed if other income-support benefits were taken into account.

Since benefit receipt in all countries is conditional on the recipient passing a means-test at the 'core family' level, I categorize an individual as a benefit recipient in a given period if a positive amount of benefit payments is registered for any household member in that period. For a discussion of the advantages and limitations of this approach, see Cappellari & Jenkins (2008a). I restrict my analysis to individuals of working age, which I define as the range from 25 to 59 years. This corresponds to the usual practice in the existing work on social assistance benefit dynamics and is meant to make sure that the results are not too strongly affected by individuals who enter the labour market or retire.

3.4 Data used

The analysis presented in this chapter is based on data from four separate national administrative data sets for Luxembourg, the Netherlands, Norway, and Sweden. Each of them provides information on benefit receipt for the entire population of the respective country at monthly intervals. The observation periods vary from 108 months (9 years) for Sweden to 276 months (23 years) for Luxembourg and overlap during a window of 96 months from January 2001 to December 2008.

Results for Luxembourg are based on a data set of social assistance benefit spells collected by the National Solidarity Fund (FNS) that administers the RMG in Luxembourg. Access was granted by the General Inspectorate of Social Security (IGSS). The data come in the form of the starting and end months of all benefit spells between January 1988 and December 2010. They contain records on about 30,400 benefit recipients of working age, which corresponds

to about 1.6 million person-month observations.[8]

The data for the Netherlands come from two separate databases administered by Statistics Netherlands (CBS). The Social Statistical Database (SSB, *Sociaal Statistisch Bestand*) provides precise starting and end dates of all benefit spells for the Dutch resident population over the years 1999 to 2010. I match these records into data from the municipal population registers (GBA, *Gemeentelijke Basisadministratie*) that contain information on the personal and household characteristics of the entire Dutch population (recipients and non-recipients). From the resulting population data set, I use a 0.5% random sample of individuals, which consists of about 96,000 persons (11.7 million person-month observations), 12,000 of which are benefit recipients (527,000 recipient-months).[9] All data work was done on-site at CBS in The Hague.

For Norway, I use data from the Social Security Event-History Database (*FD-Trygd*) that is administered by Statistics Norway. This dataset provides information on benefit receipt for the entire Norwegian population for the years 1993 to 2008. I work with a 10% random sample from the population, which translates into 323,000 individuals (41.9 million person-month observations) of whom 46,000 are benefit recipients (883,000 recipient-month observations).[10] The data analysis was implemented during my employment at Statistics Nor-

[8]The full observation period even extends from November 1986 to February 2012. The data appear to be less reliable however at the beginning and towards the end of the observation period such that I do not use them (for additional details, see Königs (2012)). I am not aware of any earlier research papers that use the FNS data. The IGSS however publishes statistics on trends in benefit receipt derived from FNS data in its annual reports (IGSS, 2012).

[9]To my knowledge, the only existing study of social assistance dynamics using SSB data is from Zorlu (2013), who estimates standard probit models to examine the determinants of benefit receipt for a cross-section of first- and second-generation immigrants in 2005.

[10]*FD-Trygd* has been used extensively to study social assistance benefit receipt: Dahl & Lorentzen (2003a) analyse the length of benefit spells in Norway using monthly data over an eight-year period from 1992 to 1999. Hansen (2009) also looks at social assistance spell durations for the years 1992 to 2002 however annualizes the monthly data on benefit receipt. *FD-Trygd* data have also been used to study exits from benefit receipt (Dahl & Lorentzen, 2003b), state dependence in benefit receipt (Chapter 4 of this dissertation or Bhuller et al. (2014)), the effects of ALMPs on social assistance dynamics (Dahl & Lorentzen, 2005; Lorentzen & Dahl, 2005), social assistance dependency over the life cycle (Hyggen, 2006; Lorentzen, Dahl & Harsløf, 2011) and across generations (Lorentzen, 2010).

way. For a more detailed description of the data set, see Chapter 4.

For Sweden, data come from the Social Assistance register maintained and made available by the National Board of Health and Welfare. This data set provides monthly information on benefit receipt for the years from 2001 to 2009. Over this period, the population of benefit recipients consists of about 530,000 individuals, which translates into about 10.5 million person-month observations.[11]

All four data sets permit linking individuals with other members in the same household based on a household identifier, which allows me to define the benefit receipt variable at the household level. The data also provide limited information on the demographic characteristics of sample members, notably their sex, age, nationality / country of origin, and family status (except for Luxembourg). The Norwegian data can in principle be matched with information on educational attainment, a feature which however I do not make use of since no comparable data are available for the other countries.

Since the data for Luxembourg and Sweden only contain information on individuals for as long as they receive benefits (and no records for non-recipients), I use annual statistics on the size of the working-age population provided by the OECD (2013) for the calculation of receipt rates and entry rates into benefits.[12]

[11]Two earlier studies use the Swedish NBHW database to analyze social assistance benefit receipt: Bergmark & Bäckman (2004) study re-entries into benefit receipt of social assistance leavers, however restrict their analysis to annual data for the years 1990 to 1999. Bäckman & Bergmark (2011) use the monthly data to study duration dependence in social assistance receipt over the period from 2002 to 2004.

[12]To assess the robustness of the results, I have replicated the analysis for Norway using the same annual OECD population statistics (rather than the monthly population numbers constructed from the *FD-Trygd* sample), and I find that this leaves the results virtually unaffected. Over the period from January 2001 to December 2008, the largest difference found in any given month between the benefit receipt rates calculated using those two sources of population numbers is 0.03 percentage points; entry rates derived using the two sources of population numbers differ by less than 0.01 percentage points in each month. It thus seems reasonable to use aggregate population statistics to calculate benefit receipt rates and entry rates for Luxembourg and Sweden where no micro-based population numbers are available.

3.5 Trends in aggregate benefit dynamics

Due to the lack of suitable panel data with monthly observations, much of the existing work on social assistance benefit dynamics is based on annual data. As described in Chapter 1, such data are taken either from household surveys (Cappellari & Jenkins (2014), Königs (2014) and Chapter 2) or administrative records (Finnie & Irvine (2008); Andrén & Andrén (2013); Hansen et al. (2014)).

While analyses based on annual data can provide important insights about the frequency of benefit receipt, its trends over time, and the most important recipient groups, the minimum period of benefit entitlement is in reality shorter than a year in most countries.[13] Annual data on benefit receipt can therefore only give an approximate representation of the underlying dynamic processes, and trends in benefit receipt observed in annual data may not always adequately reflect the benefit dynamics at the monthly level.

This section provides a short overview of the aggregate benefit receipt dynamics in Luxembourg, the Netherlands, Norway and Sweden. Its main purpose is to provide some background to the analysis of individual-level receipt dynamics presented in the following section. By presenting comparable results for both monthly and annualized data, it also permits determining to what extent the conclusions drawn from data at different levels of time aggregation are consistent.

3.5.1 Rates of benefit receipt

Rates of social assistance benefit receipt differ across countries in both levels and trends over the observation period (Figure 3.1). The share of working-age individuals receiving benefits declines in two of the four countries: In the

[13] In Norway and Sweden for instance, individuals are usually required to re-confirm the circumstances that justify benefit receipt every month. Ayala & Rodriguez (2007a,b) by contrast report that the benefits of Madrid's Minimum Income programme (IMI) are granted for one year, renewable automatically.

Netherlands, the receipt rate falls from over 5% in the late 1990s to around 4% in the late 2000s, in Norway it decreases from around 2.5% in the early 1990s to below 2% in the late 2000s.[14] Benefit receipt rates are relatively stable for Sweden, where they fluctuate around 2% to 2.5%. In Luxembourg, receipt rates increase from around 1% in the late 1980s to over 4% in 2011.

The declining benefit receipt rates for the Netherlands and Norway match similar downward trends for other countries. Cappellari & Jenkins (2014) and Hansen et al. (2014) report a decline in benefit receipt rates in Britain and Canada, respectively. In both cases, receipt rates peaked at around 12% the mid-1990s to fall to about 6% in Britain and 8% in Canada in the mid-2000s.[15] For the United States, Scholz, Moffitt & Cowan (2009) find a 50% drop in caseload numbers of Temporary Assistance for Needy Families (TANF) from its introduction in 1996 to 2007.[16]

These downward trends likely reflect a combination of different factors. One possible driver are policy reforms, notably stricter eligibility requirements and a greater focus on the activation of employable benefit recipients. At least in part, the fall in receipt rates however certainly also reflects an improvement in the general economic environment after the economic downturn of the early 1990s with negative GDP growth rates in Canada, Norway, Sweden, and the United Kingdom.

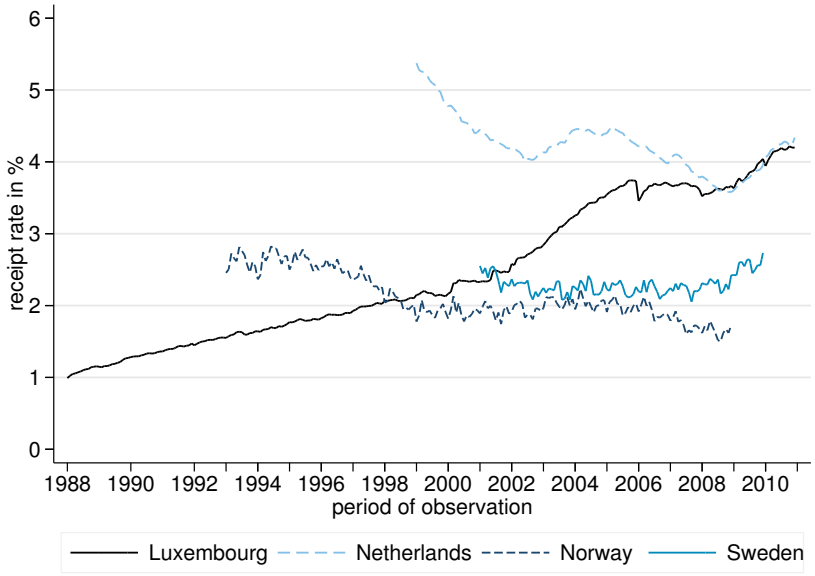
The rising rates of benefit receipt in Luxembourg can be understood in the context of the gradual expansion of the RMG after its introduction in 1986, with increasing take-up rates, looser eligibility conditions, and greater benefit generosity (Königs, 2012).

[14]In the Netherlands, the decline in receipt rates was much stronger in the late 1990s, for which however no monthly data are available (see Immervoll et al. (2014)).

[15]The benefit programmes taken into account are Social Assistance in Canada and Income Support or Unemployment Benefits / Jobseeker's Allowance in Britain.

[16]TANF is the successor of the AFDC programme mentioned in Section 3.2 and was introduced in 1996 by the Clinton administration through the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA).

Figure 3.1: *Rates of benefit receipt – monthly data*



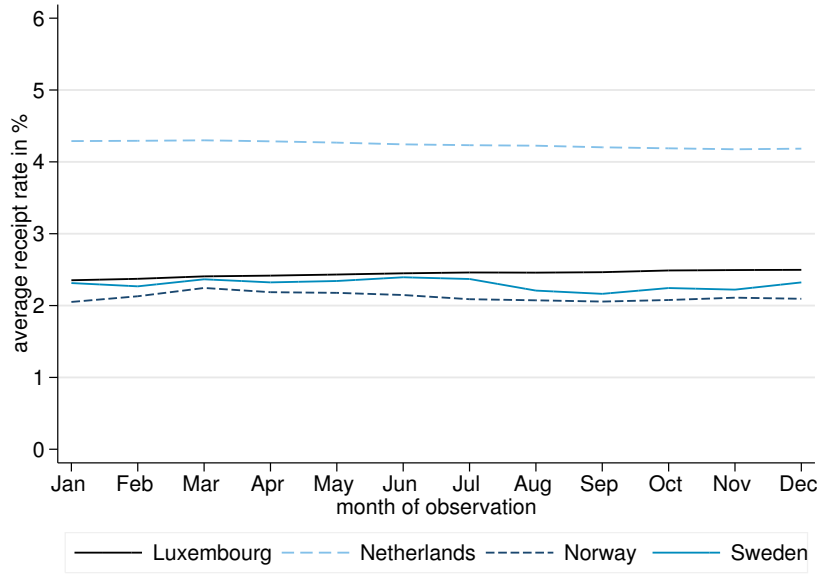
Note: Receipt rates measure the share of working-age individuals (25-59 years) who receive benefits in a given period. Calculations are based on administrative data from Luxembourg (FNS database, 1988-2010), the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). For Luxembourg and Sweden, I calculate receipt rates by combining micro data on recipient numbers with annual aggregate statistics on the size of the working-age population in these countries (OECD, 2013).

There is no evidence of seasonality in benefit receipt for working-age individuals in any of the four countries despite relatively strong month-to-month fluctuations in receipt rates especially in Norway and Sweden.[17] Figure 3.2 plots the average rate of benefit receipt in each calendar month over all years of the observation period. The lines for all four countries are essentially flat, which implies that there is very little systematic variation in rates of benefit receipt between calendar months. Any month-to-month fluctuations in benefit receipt observed in monthly receipt rates for Sweden and Norway thus represent 'noise' rather than seasonal effects.

Figure 3.3 presents annualized benefit receipt rates. To construct these rates from the monthly data, I have employed the 'benefit year' approach used

[17]For Norway and Sweden, this result changes if the bottom age threshold is lowered from 25 years to also include individuals in their early 20s. In this case, rates of benefit receipt rise considerably over the summer months as young adults flow into social assistance to bridge gaps in their educational schedules.

Figure 3.2: *Rates of benefit receipt – seasonality*



Note: See note to Figure 3.1. Seasonality is measured as the receipt rate for a given calendar month averaged over all years of the respective observation period.

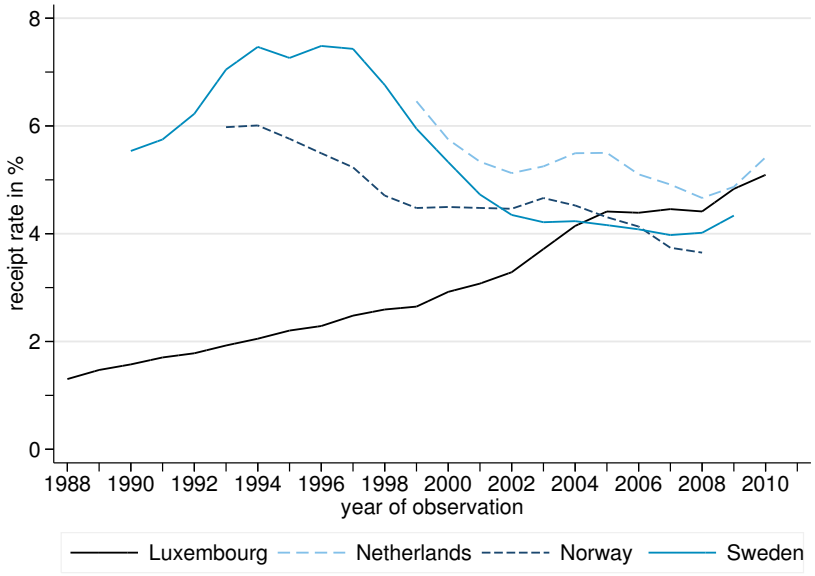
in much of the existing literature classifying an individual as a recipient in a given calendar year if any benefit payments are recorded during that year. For Sweden, I moreover present results for the years 1990 to 2000 for which only annual data are available. The numbers can be compared directly for instance to those presented by Finnie & Irvine (2008) or Hansen et al. (2014) for Canada or by Andrén & Andrén (2013) for Sweden.[18]

Unsurprisingly, annualized receipt rates are much higher than their corresponding monthly counterparts. This reflects the fact that the share of individuals who receive benefit payments at any time during the year must be greater than the 'instantaneous' receipt rate in any month of that year. The ratio between the annualized and monthly receipt rates provides a measure of the 'turnover' in benefit receipt during the calendar year. This result is summarized in Table 3.1.

Turnover in benefit receipt is much lower in Luxembourg and the Nether-

[18]Meanwhile, the numbers are not directly comparable to those reported by Cappellari & Jenkins (2014) for the United Kingdom or Chapter 2 and Königs (2014) for Germany, who use survey data and measure benefit receipt rates at the interview date only.

Figure 3.3: *Rates of benefit receipt – annual(ized) data*



Note: Annual receipt rates measure the share of working-age individuals (25-59 years) who receive benefits at any time in a given year. Calculations are based on administrative data from Luxembourg (FNS database, 1988-2010), the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 1990-2009). To calculate receipt rates for Luxembourg and Sweden, I combine micro data on recipient numbers with annual aggregate statistics on the size of the working-age population in these countries (OECD, 2013).

lands that in Norway or Sweden. In the former two countries, the annual rate of benefit receipt is about 25% higher than the average monthly rate of benefit receipt (column IV of Table 3.1). Over the course of a year, and for a stable annual rate of benefit receipt, about 25% of all Luxembourg and Dutch recipients at the beginning of a year thus leave benefits and are 'replaced' by new benefit entrants. The corresponding figures are substantially higher for the two Nordic countries: In Sweden, the share of individuals who receive benefits at any time during the year is 73% higher than the average monthly rate of benefit receipt. In Norway, the recipient population on average completely changes more than once during a calendar year.

The results demonstrate that relatively similar average monthly rates of benefit receipt in Luxembourg and Norway can go hand in hand with very different degrees of turnover and, hence, benefit durations (an aspect studied

Table 3.1: *Monthly and annualized rates of social assistance benefit receipt*

country	average monthly rate of benefit receipt in %	average annual rate of benefit receipt in %	ratio of columns III and II
Luxembourg	2.4	3.0	1.22
Netherlands	4.2	5.3	1.26
Norway	2.1	4.7	2.23
Sweden	3.2	5.5	1.73

Note: Calculations are based on administrative data from Luxembourg (FNS database, 1988-2010), the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). For Luxembourg and Sweden, I calculate receipt rates by combining micro data on recipient numbers with annual aggregate statistics on the size of the working-age population in these countries (OECD, 2013).

in more detail in Section 3.6). Annual rates of benefit receipt calculated using the 'benefit year' approach (as presented in Figure 3.3) tend to give a much better measure of the frequency of benefit receipt at any point in time if the month-to-month turnover in benefit receipt is low.

The numbers can also be used to illustrate that especially where turnover is high, a greater share of the population may be in social assistance for at least a short period at some point during their lives. For instance, 10.2% of working-age individuals in Norway who were in the sample for the eight-year period from 2001 to 2008 received benefits at least once during that time (*not shown*). This share is more than five times the average monthly rate of benefit receipt of 1.9% among the same individuals. In the Netherlands, the average monthly receipt rate among individuals observed for the same eight years is 3.9%, but 9.8% of individuals receive benefits at some point during this period. In both countries, the number of individuals who draw on social assistance at some point is thus much higher than the low monthly receipt rate may suggest. At least for some individuals, benefit receipt thus tends to be a transitory phenomenon. 'Recipients' and 'non-recipients' should thus not necessarily be considered as two very distinct groups.

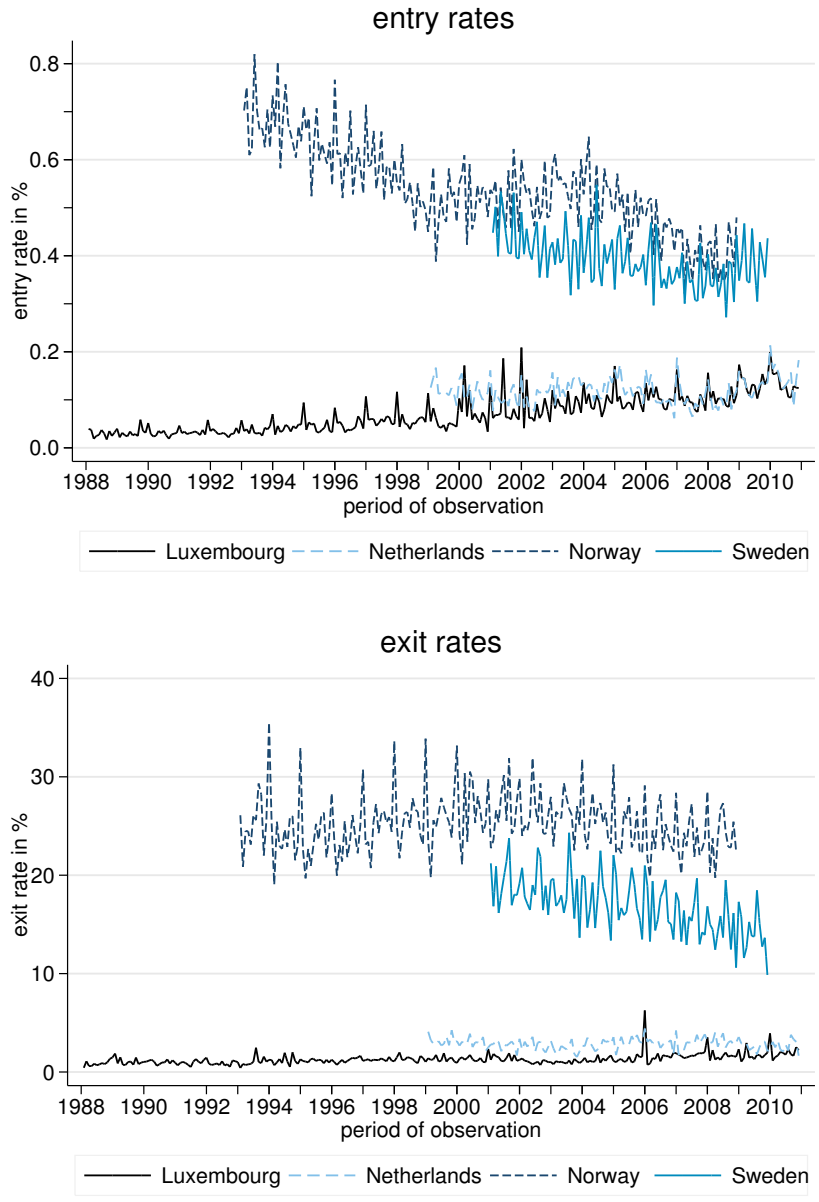
3.5.2 Benefit transition rates

The trends in benefit receipt rates can be linked to transition rates into and out of benefits. For this purpose, I define an entry rate as the number of individuals who receive benefits at time t and who did not receive benefits in *t-1* as a percentage of the non-recipient population in period *t-1*. Analogously, I define an exit rate as the number of individuals who do not receive benefits in period t but who received benefits in *t-1*, as a share of all benefit recipients in *t-1*. As the data for Sweden and Luxembourg do not include non-recipients, I use again OECD statistics on the size of the working-age population in these countries for the calculation of entry rates (OECD, 2013). Figures 3.4-3.6 plot month-to-month benefit transition rates, seasonal variations in transition rates, and annualized transition rates calculated from the monthly data, respectively.

The larger turnover in benefit receipt in the Nordic countries observed in Table 3.1 is reflected in much higher benefit transition rates. In Norway and Sweden, around 0.5% and 0.4% of non-recipients, respectively, enter benefits from one month to the next (upper panel of Figure 3.4). In Luxembourg and the Netherlands, the corresponding average monthly entry rates are much lower at around 0.1%. Similar disparities are observed for exit rates: In Norway and Sweden, around 25% and 15% of recipients in a given month leave benefits in the next month, compared to below 3% of Dutch recipients and not even 1.5% of recipients in Luxembourg (bottom panel).

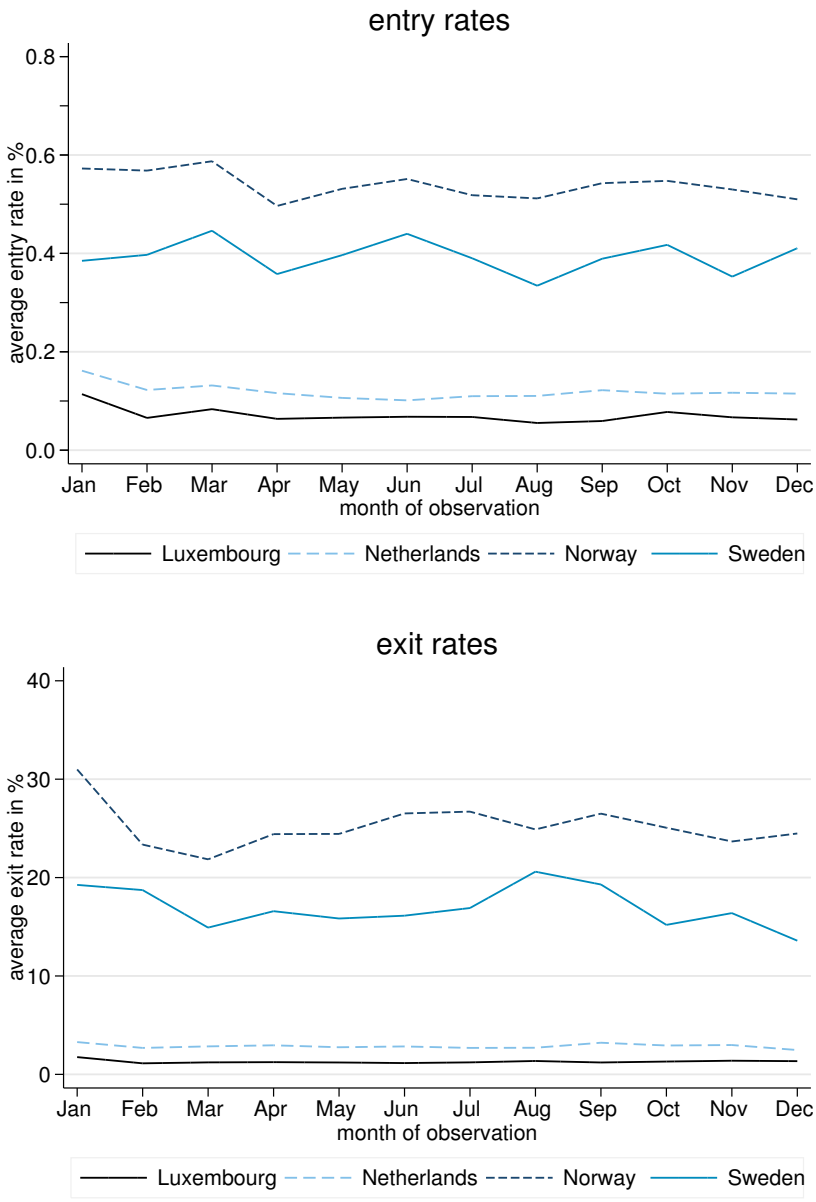
Trends in benefit receipt rates observed in Figure 3.1 appear to be driven primarily by changes in entry rates. In Norway, the decline in the receipt rate coincides with a fall in the entry rate while the exit rate remains relatively stable. In Luxembourg, where the receipt rate increased, a similar rise can be observed in the entry rate again at stable exit rates. These results again confirm earlier findings for Canada (Finnie & Irvine, 2008) and Britain (Cappellari & Jenkins, 2014), who report that declining benefit receipt rates coincide with falling entry rates into benefit receipt while exit rates remain stable or even

Figure 3.4: *Benefit transition rates – monthly data*



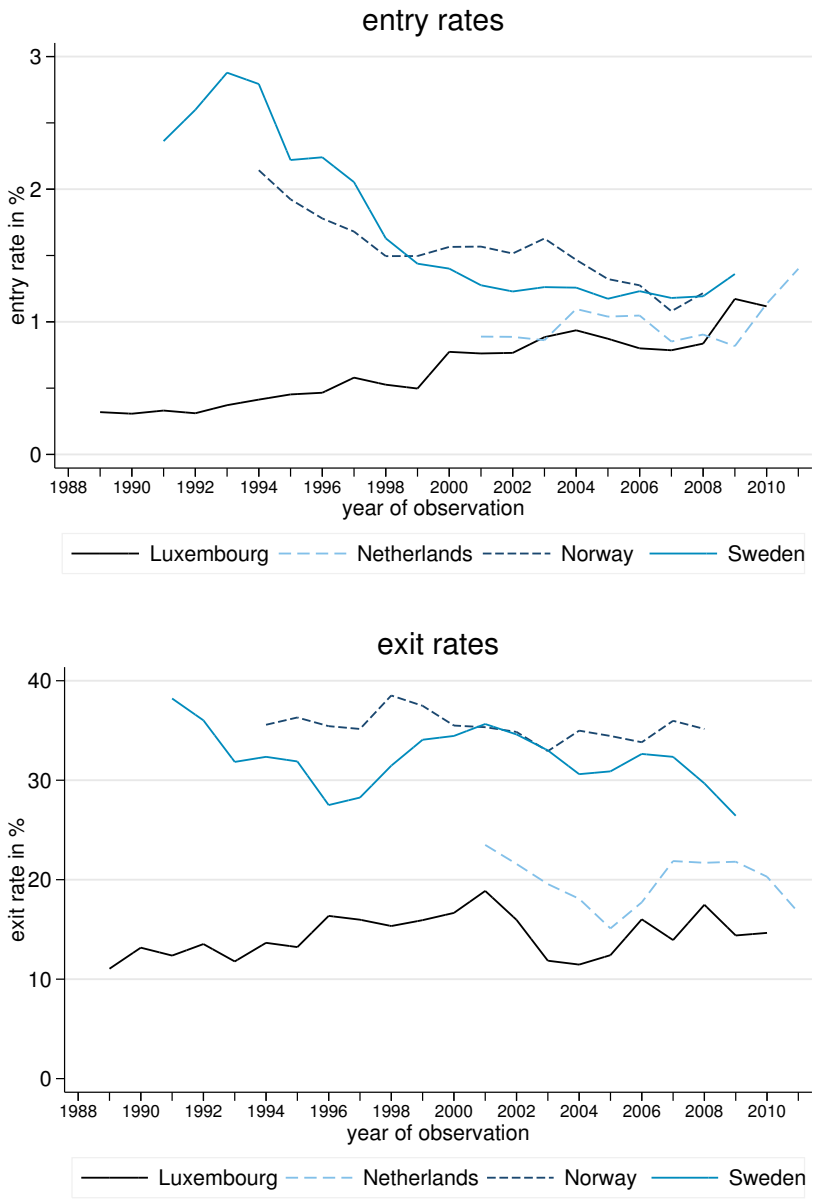
Note: Entry rates are defined as the number of individuals who receive benefits in period *t* and who did not receive benefits in *t-1* divided by the total number of non-recipients in *t-1*. Exit rates are defined accordingly as the number of individuals who do not receive benefits in period *t* but who received benefits in *t-1* as a fraction of all benefit recipients in *t-1*. Calculations are based on administrative data from Luxembourg (FNS database, 1988-2010), the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). For Luxembourg and Sweden, I calculate receipt rates by combining micro data on recipient numbers with annual aggregate statistics on the size of the working-age population in these countries (OECD, 2013).

Figure 3.5: *Benefit transition rates – seasonality*



Note: See note to Figure 3.4. Seasonality is measured as the transition rate for a given calendar month averaged over all years of the respective observation period.

Figure 3.6: *Benefit transition rates – annual(ized) data*



Note: See note to Figure 3.4.

drop. The pattern differs however in the two other countries: Figure 3.1 shows a decline in the receipt rate for the Netherlands, yet both entry and exit rates remained relatively stable over the observation period. The decline in benefit receipt thus represents a move towards a new 'equilibrium receipt rate'.[19] For Sweden, a stable receipt rate is associated with declines in both entry and receipt rates.

Like the benefit receipt rates, also benefit transition rates display a perhaps unexpected lack of seasonality (Figure 3.5). While there are some fluctuations across calendar months, the only systematic patterns appear to be the somewhat higher exit rates from benefits in January and in the summer months for the two Nordic countries. For Luxembourg and the Netherlands, the low observed exit rates and the lack of seasonality in entries and exits hint at long benefit spell lengths. Overall, the large fluctuations in month-to month benefit transition rates observed for all four countries in Figure 3.4 thus do not reflect seasonality in benefit receipt.

The patterns in benefit transition rates are remarkably well preserved when the data are aggregated to the annual level (Figure 3.6). Annual entry rates show similar trends as their monthly counterparts, declining for Norway and Sweden, remaining stable for the Netherlands, and rising for Luxembourg. Similarly, exit rates calculated from the annualized data display the same lack of trend observed for monthly exit rates.

Annual transition rates can be thought of describing the benefit dynamics purged of any short-term turnover: Annual entry rates consider benefit receipt of those individuals who did not receive any payments last year, which means that they do take into account direct re-entries of benefit leavers. Similarly, annual exit rates do not reflect short-term departures from benefits but only consider exits that last for one year at least. The much lower cross-country

[19] At an average monthly entry rate of 0.12% and an average exit rate of 2.86%, the receipt rate at which entries equal exits is at 4%. This roughly corresponds to the receipt rate attained towards the end of the observation period.

disparities in the level of annual transition rates compared to the monthly rates thus imply that a substantial share of the month-to-month transitions observed for the two Nordics are only temporary.

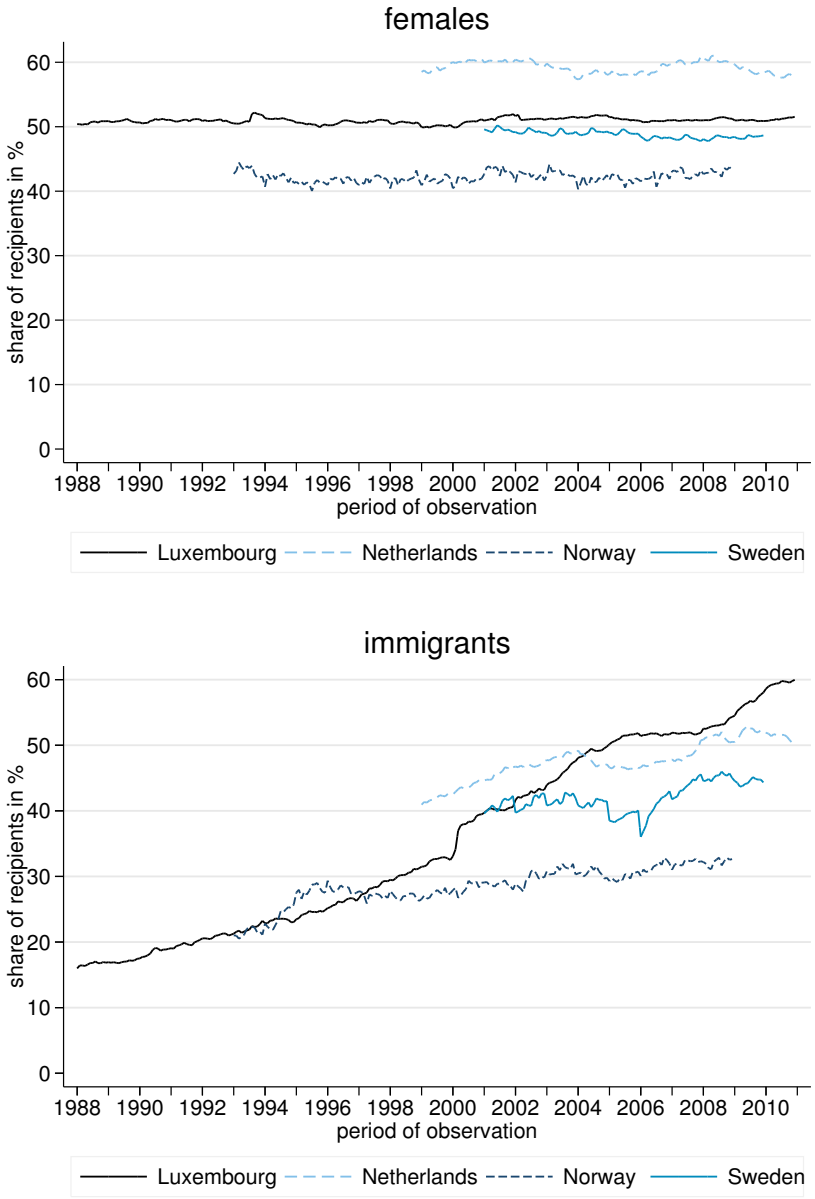
An implication of these benefit transition rates is that observed state dependence in benefit receipt is strong: Even for Norway, where benefit transition rates are highest, monthly-to-month persistence rates of around 75% are considerably higher than the corresponding entry rates of about 0.5%. While most of this gap between benefit persistence and entry rates likely reflects differences in characteristics between recipients and non-recipients, it raises concerns about possible 'scarring' effects and motivates research on the drivers of state dependence in benefit receipt (for a discussion, see Chapters 2 and 4).

3.5.3 The composition of the recipient population

As a last-resort safety net, social assistance tends to support a highly heterogeneous population. Important recipient groups are usually young adults with little work experience, those with low education, single parents with young children, immigrants, individuals with health problems, and other groups with limited incomes and no or little support through other benefit programmes. To the extent that these groups find it more difficult to become self-sufficient than less disadvantaged individuals, the composition of the recipient population will be reflected in the dynamics of benefit receipt, *e.g.* through long benefit durations.

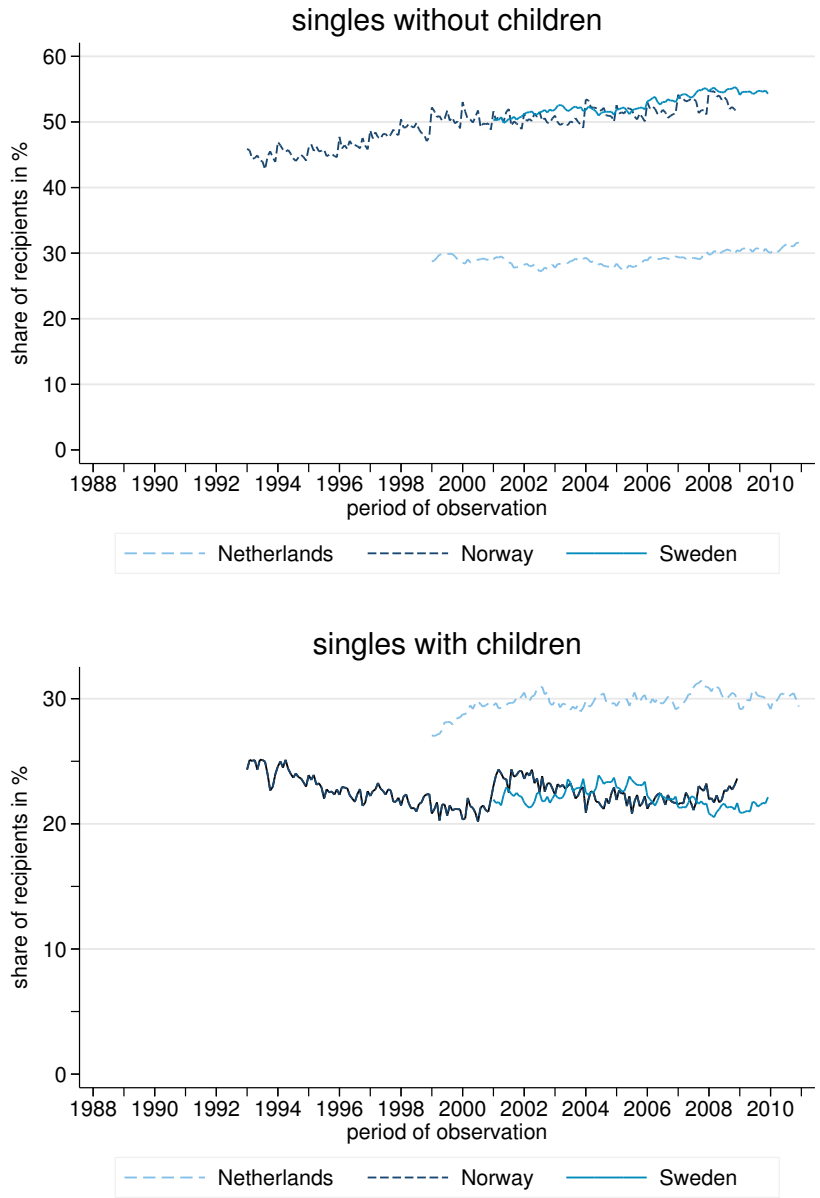
One shortcoming of the data sets used for this analysis is that – with the exception of the Norwegian *FD-Trygd* – they do not provide very rich information on individuals' socio-economic characteristics. While all four data sets include information on an individual's age, sex, and origin or nationality, only the data for the Netherlands, Norway, and Sweden give reliable information on family status, and only *FD-Trygd* includes educational attainment. Figures 3.7-3.9 give a breakdown of recipients into the most important client groups that can

Figure 3.7: *Composition of the recipient population – females and immigrants*



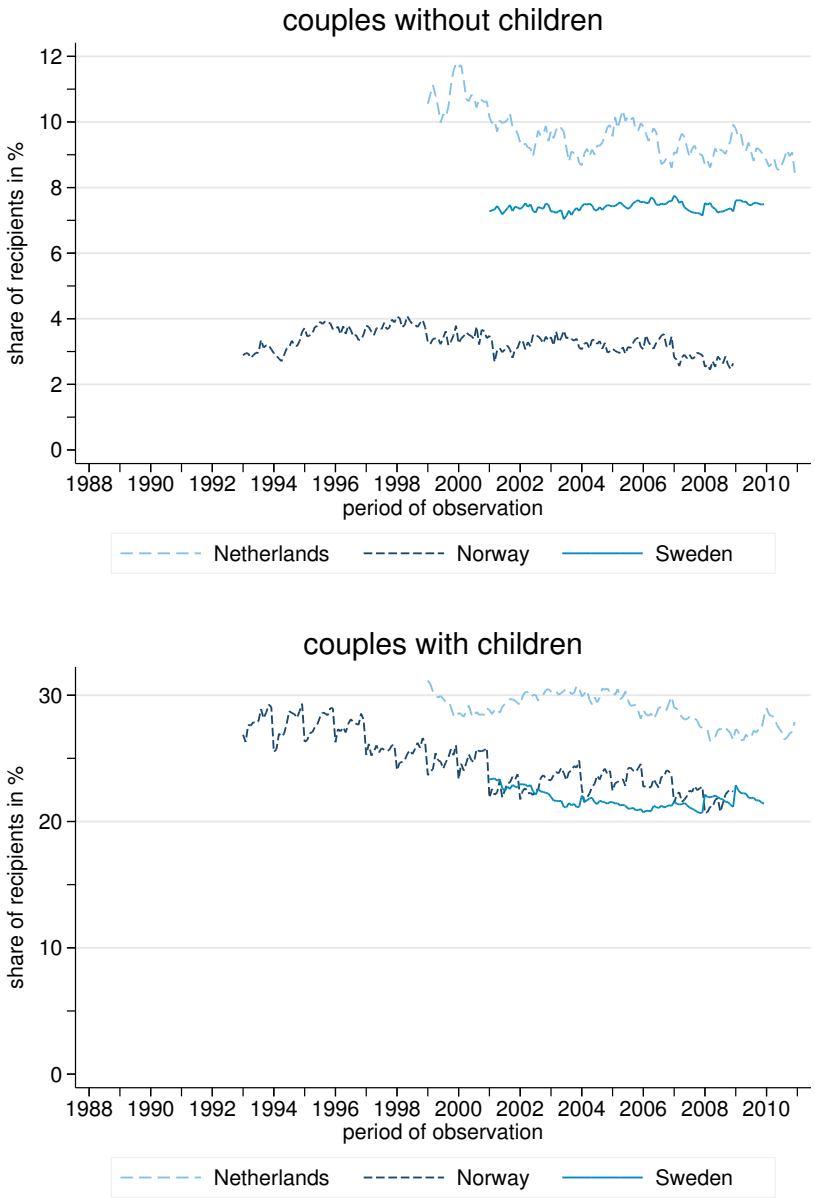
Note: Calculations are based on monthly administrative data from Luxembourg (FNS database, 1988-2010), the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). The reported numbers are for individuals aged 25-59 years, with social assistance benefit receipt being defined at the household level. For Luxembourg, the lower panel gives the proportion of individuals without Luxembourg nationality; for Sweden, it plots the proportion of recipients living in a household in which at least one of the adults was born abroad.

Figure 3.8: *Composition of the recipient population – singles*



Note: Calculations are based on monthly administrative data from the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). No information on family status is available in the Luxembourg FNS data. The reported numbers are for individuals aged 25-59 years, with social assistance benefit receipt being defined at the household level.

Figure 3.9: *Composition of the recipient population – couples*



Note: Calculations are based on monthly administrative data from the Netherlands (SSB, 1999-2010), Norway (FD-Trygd, 1993-2008), and Sweden (NBHW database, 2001-2009). No information on family status is available in the Luxembourg FNS data. The reported numbers are for individuals aged 25-59 years, with social assistance benefit receipt being defined at the household level.

be identified in the data and show changes in the composition of the recipient population over time. For definitions of these variables see Appendix E, Table E.1.

The share of women among recipients varies significantly across countries (upper panel of Figure 3.7). It is lowest at around 40% in Norway, where there exists a separate minimum-income benefit (the so-called 'Transitional Allowance') to support single parents most of whom are women. In the Netherlands, where the share of single parents among recipient households is highest, close to 60% of recipients are women. In Luxembourg and Sweden, the recipient population consists to about equal shares of women and men. In none of the four countries is there substantial time-variation in the share of women among benefit recipients.

The fraction of immigrants among recipients is high and has been rising considerably in all countries over the observation period (bottom panel of Figure 3.7). This effect is most distinct for Luxembourg, where the share of benefit recipients without Luxembourg nationality more than tripled over the observation period from below 20% of recipients in 1988 to close to 60% at the end of 2010. Part of this rise reflects a general increase in the number of migrants in the country. The FNS data unfortunately do not permit calculation of the immigrant share in the overall population. Information from other administrative sources however suggests that over the period from 2001 to 2009 alone, the share of working-age Luxembourg residents without Luxembourg nationality rose from 41% to 46%.[20] The more important factor however is certainly the step-wise relaxation of residence requirements for eligibility to the RMG.[21]

[20]These numbers are based on calculations using data from the annual Social Policy Analysis File on Income in Luxembourg (SPAFIL), see Königs (2012).

[21]Eligibility for RMG was initially conditional on residence in Luxembourg for the ten years prior to the application for benefits, a requirement that was relaxed sequentially to five out of the last twenty years in 1999. The current rules, introduced in 2001, do no longer impose a mimimum residence requirement on EU citizens; non-refugee immigrants from outside of the EU should have lived in the country for at least five out of the last 20 years. For further information see again Königs (2012).

The result that non-natives are strongly overrepresented among benefit recipients is a standard finding in the literature on social assistance benefit receipt dynamics and motivates a series of papers on the 'immigrant-native gap' in benefit receipt (see for instance Hansen & Lofstrom (2003, 2008) for evidence on Sweden or Riphahn & Wunder (2012) for results on Germany).

A breakdown of recipient households by family type in Figures 3.8 and 3.9 shows relatively similar patterns across countries. In Norway and Sweden, singles without children constitute the by far the largest recipient group accounting for about half of all recipient households. Singles and couples with children each account for 20-25% of recipient households, with less than 10% of recipient households being couples without children. In the Netherlands, the shares of recipient households with children (singles or couples) is slightly higher at around 30%, and singles without children constitute only around 30% of recipient households. These numbers are comparable to those reported for a larger range of countries by Immervoll et al. (2014). Changes in the frequency of the different household types are modest, though there has been a steady rise in the share of singles without children and a decline of the share of couples with children in the Nordic countries.

3.6 The micro-dynamics of benefit receipt

The aggregate benefit receipt rates and transition rates presented in the previous section suggest that the degree of 'turnover' in benefits – and thus the length of benefit spells – differs across the four countries studied. In this section, I take a spell-based perspective to show how these aggregate-level differences are reflected in individual-level benefit receipt dynamics. To ensure that the results are comparable across countries, I limit my analysis to the eight years from January 2001 to December 2008 during which the observation periods of all four data sets overlap. The samples are restricted to benefit spells of individuals who are of working age (25-59 years) at the beginning of their spell.

3.6.1 The definition of a benefit spell

The advantage of using monthly data for an analysis of benefit dynamics is that spell lengths can be measured with precision, and that even very short spells can be easily identified in the data. A contentious issue in the existing literature on the duration of benefit receipt has been, however, how short interruptions in receipt should be dealt with when spells are defined. Short periods without benefit receipt may not represent actual exits from benefits but may result from 'administrative churning', for instance caused by delays in benefit pay-outs or errors in data entry. In this case, they should arguably be corrected for.

Even if data have been recorded correctly, a case can be made for ignoring short exits from benefit receipt because they may not represent genuine departures from a situation of dependency. Kazepov (1999) suggests that one should really be concerned with *dependence episodes*, which he defines as periods of benefit receipt that might span multiple *cash episodes* (*i.e.* benefit spells) interrupted by short times without benefit receipt. Blank (1989) ignores interruptions in benefit receipt of up to three months unless she can link them to changes in employment status or income, and a similar approach is taken in most of the later work on the topic.

I choose to employ a different approach by defining a benefit spell as a period during which a positive amount of benefit payments is observed for every single month. The spell is thus coded as having ended as soon as no benefit payment is recorded. The justification for this approach is that – as we will see – a large share of the observed benefit spells in the two Nordic countries are of very short duration, and that it is not obvious why short spells on and off benefits should be treated asymmetrically. To illustrate the robustness of my findings, I also report results obtained when ignoring interruptions in benefit receipt of two months or less. Unlike it is done by previous authors, I however do not count these interruptions as contributing towards the length of the benefit spell.[22]

[22]For instance, an individual might be observed as receiving benefits for two times four months interrupted by a period without payments of two months. In my analysis, such an

3.6.2 Hazard rates

A plot of the hazard rates of exits from and re-entries into benefit receipt can provide a first overview of the benefit transition dynamics at the individual level. The upper panel of Figure 3.10 presents the hazard rate out of benefits among recipients who started their spell during the observation period. The lower panel shows the corresponding hazard rate from non-receipt into benefits for those who have ended a spell of benefit receipt.[23]

In all four countries, hazard rates from benefit receipt show the typical declining pattern indicating that the probability of leaving benefits in a given period (conditional on not yet having left in any of the previous periods) falls with increased duration of benefit receipt. Exit probabilities however differ substantially across the four countries: In Norway and Sweden, the period-specific exit rate from benefits is above 30% per month in the beginning of a benefit spell but strongly declines to around 10% after 12 months and further to around 5% after 36 months. In Luxembourg and the Netherlands, exit probabilities are much lower to start with and consequently decline less strongly.

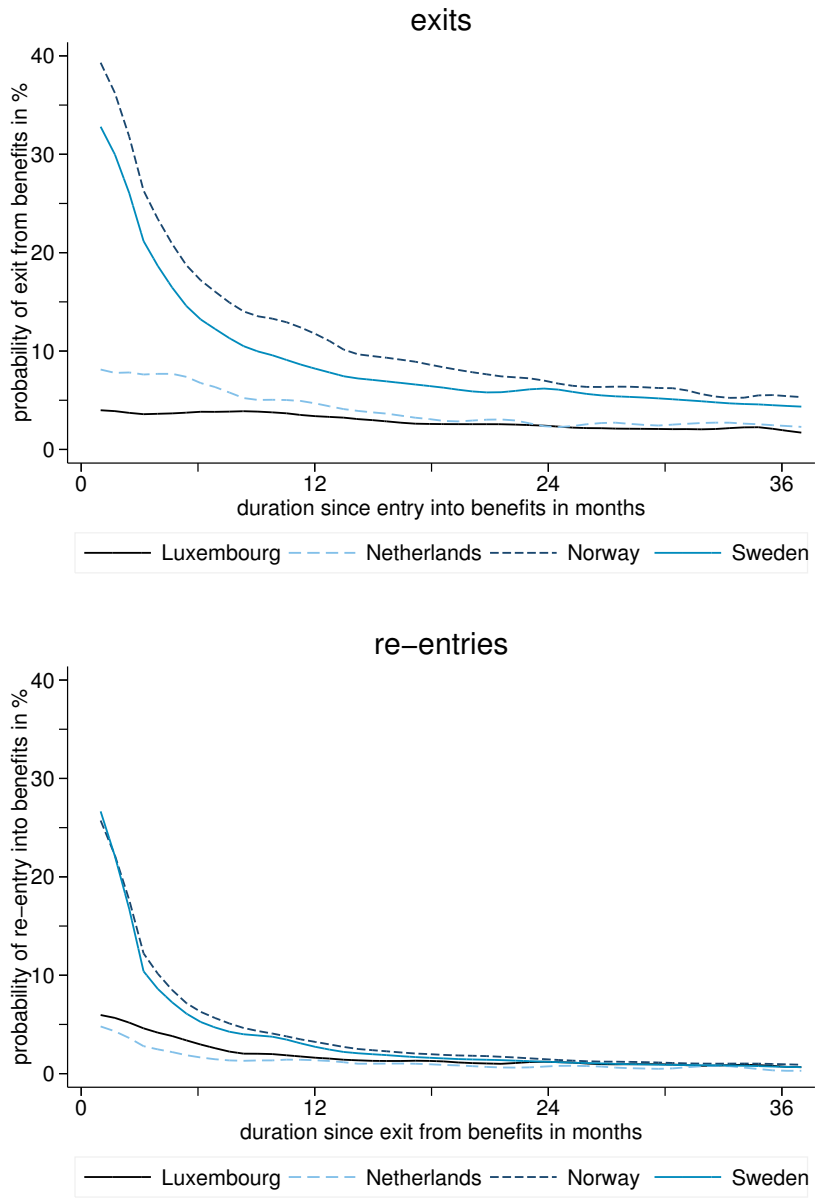
Re-entry hazards, *i.e.* the exit rates from self-sufficiency among those with a previous benefit spell, tend to be lower but show a very similar pattern. In the two Nordic countries, re-entry rates are much higher in the early periods of a spell off benefits declining from around 25% in the initial months to 2-3% at 12 months and 1% at 24 months and thereafter. In Luxembourg and the Netherlands, re-entry rates of benefit leavers are only around 6% per month already at the beginning of a spell off benefits and decline less strongly. 18 months after the end of a benefit spell, the re-entry hazard rates in all four countries have practically converged.

The hazard rates' declining patterns of course cannot be interpreted as evidence for duration dependence. The fall in exit rates with increased spell dura-

episode would be classified as either two separate spells of four months or as a single spell of eight months. Gustafsson et al. (2002) for instance would record a single spell of ten months.

[23]For consistency, I restrict the sample to spells of non-receipt that follow a benefit spell which started during the observation period.

Figure 3.10: *Hazard rates*



Note: Calculations are based on monthly administrative data for the period January 2001 to December 2008 from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd), and Sweden (NBHW database). The exit hazard rate gives the probability of leaving benefits at a given spell duration conditional on not having left in any of the earlier periods. Analogously, the re-entry hazard rate gives the probability of re-entering among welfare leavers after a given duration off benefits conditional on not having re-entered in an earlier period.

tion likely reflects primarily compositional effects as individuals with favourable characteristics leave social assistance more quickly. Rapidly falling hazard rates from self-sufficiency back into benefits indicate that a large share of departures from social assistance only represent short interruptions in benefit receipt, potentially as individuals seize short-term employment opportunities, or indeed as a result of administrative churning.

The larger exit and re-entry probabilities in the two Nordic countries, especially at the beginning of a spell, reflect the higher aggregate month-to-month benefit transition rates observed in Figure 3.4. As shown below, they also imply much shorter spell lengths and a higher share of individuals with multiple benefit spells.

3.6.3 The length of benefit spells – evidence from long panels

To assess the incidence of long-term benefit dependence, I study the distribution of spell lengths over the time from January 2001 to December 2008. Table 3.2 presents an overview of the lengths of all benefit spells that start during the eight-year observation period. Spells that are ongoing in December 2008 are counted as right-censored but included in the calculations. Left-censoring is not an issue for inflow samples.

Observed spell lengths differ substantially across countries. Benefit spells are generally relatively short in Norway and Sweden but much longer in the Netherlands and Luxembourg (Table 3.2, panel A). The average length of a benefit spell in the sample varies from below 4 months in Norway to 23 months, *i.e.* nearly 2 years, in Luxembourg. Median durations indicate that short-term benefit receipt is the norm in Norway and Sweden, where 50% of all spells are of duration 2 months or shorter. By contrast, for the Netherlands and Luxembourg, half of all benefit spells last at least 9 and 14 months, respectively.

The duration of benefit receipt is also heterogeneous across spells within countries. In all four countries, at least a small proportion of recorded spells

last very long. In Luxembourg, 57% of all spells are longer than 12 months and 35% last two years or longer. In the Netherlands, close to one out of four spells lasts longer than 24 months. In Norway and Sweden, where mean and median spell lengths are short, only about 2% and 4% of all spells, respectively, last for 24 months or longer.

Despite the long observation period, a substantial fraction of spells in Luxembourg and the Netherlands are right-censored. The observed spell duration until the end of the observation period will therefore generally be lower than the true spell length. Since I include right-censored spells in my calculations, the already long average spell durations calculated for these countries still underestimate true spell lengths. Similarly, the differences in spell durations between the Nordics and the Netherlands and Luxembourg is likely to be greater still than the numbers in Table 3.2 suggest.

Ignoring short exits from benefits in the calculation of spell lengths does not strongly affect results. Median spell durations change little, remaining stable for the Netherlands and Norway, increasing by 1 month for Sweden and by 2 months for Luxembourg (Table 3.2, panel B). By contrast, measured average spell durations rise by about 75% for the two Nordic countries from 3.8 to 6.7 months in Norway and from 5.0 to 8.8 months in Sweden. This increase is reflected in the measured durations of long benefit spells. The proportion of spells that last 24 months or longer rises from 3.8% to 10.8% in Sweden and more than triples from 2.0% to 6.4% in Norway. For the Netherlands and Luxembourg, ignoring short exits from benefit receipt does not have a major effect on average spell durations. This reflects the finding from Figure 3.10 that benefit leavers in these countries are much less likely to return to benefits quickly.

It is interesting to compare the presented spell durations with those found in earlier studies. Blank (1989) reports in her analysis of AFDC-receipt in Denver and Seattle that 62% of the completed spells for a household ended within a year, with an average duration of benefit payments of 13 months. She

Table 3.2: *The length of benefit spells – inflow sample for 2001-2008*

Panel A – standard sample

country	# of spells	duration in months		share of spells in % with a duration of at least				share of censored spells in %
		median	mean	3 months	6 months	12 months	24 months	
Luxembourg	20,987	14	22.6	90.0	77.5	57.0	34.8	36.7
Netherlands	4,408	9	17.0	82.7	63.2	42.3	24.1	22.6
Norway	164,486	2	3.5	34.4	15.2	5.6	1.6	3.6
Sweden	1,551,454	2	5.0	42.5	22.3	10.5	3.8	6.5

Panel B – ignoring short-term exits

country	# of spells	duration in months		share of spells in % with a duration of at least				share of censored spells in %
		median	mean	3 months	6 months	12 months	24 months	
Luxembourg	19,335	16	24.6	91.6	80.2	60.8	38.4	39.8
Netherlands	4,112	9	18.2	83.5	64.9	44.4	26.4	24.2
Norway	94,436	2	6.1	46.6	26.4	13.3	5.8	6.3
Sweden	873,441	3	8.8	52.3	34.4	21.4	10.8	11.5

Note: Calculations are based on monthly administrative data for the period January 2001 to December 2008 from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd), and Sweden (NBHW database). Panel A uses the standard sample while Panel B ignores exits from social assistance benefit receipt of up to two months. More specifically, spells that are interrupted by a period of non-receipt of 1-2 months are counted as continuous, but the interjacent period of 1-2 months off benefits is not counted towards the duration of the benefit spell. Spells are counted as censored if they are ongoing in December 2008. The sample consists of all spells that start during the observation period. It is restricted to individuals of working age (25-59 years) at the beginning of their benefit spell.

however does not include the 36% of right-censored spells in these calculations, which generally have a longer duration until censoring. Fitzgerald (1991) calculates a much longer median AFDC benefit spell length of 20 months over a shorter observation period (32 months), but calculates a median spell duration of 11-12 months for receipt of AFDC and Food Stamps in a follow-up study (Fitzgerald, 1995). Also Hoynes (2000) reports longer AFDC spell lengths for California, where 46% of spells end within 12 months and 62% end within 24 months. The finding of relatively long AFDC spell lengths, at least compared to spell lengths in Norway and Sweden, is maybe not so surprising since the U.S. AFDC primarily targeted highly disadvantaged single mothers, whom we would generally expect to remain on welfare for longer.

More recent and comparable results are presented by Gustafsson et al. (2002) in their study of social assistance dynamics in eight European cities. For a 42-month period starting in the late 1980s or early 1990s, they report median spell durations of around 4 months for an individual's first observed spell in Gothenburg and Helsingborg and of 5-6 months for Bremen, Milan and Turin. By contrast, median spell durations are 1 year for Vitoria, over 2 years for Barcelona and nearly 3 years for Lisbon. The results for Helsingborg and Gothenburg are relatively close to the ones reported in Table 3.2 for Sweden. The median durations for Barcelona and Lisbon are much longer, which, as Gustafsson et al. (2002) suggest, may reflect stricter means-tests and thus more disadvantaged recipient populations in these cities.

My results are remarkably similar to those reported by Dahl & Lorentzen (2003a) who study benefit spell durations in Norway using the same data set for an earlier period. For their sample of spells that start in 1995, they calculate median and mean spell durations of 2 and 4 months, respectively, and a 94%-share of spells that last 12 months or shorter (p. 295, Table 6). These numbers are nearly identical to those that I present in Table 3.2 for a later and longer time period.

My findings for the Netherlands by contrast differ from those reported by Snel et al. (2013) for *bijstand* receivers in the Dutch city of Rotterdam. For the 1999 inflow cohort, they calculate a median spell duration of 23 months (p.184, Table 5), which is more than double the 9 months that I report in Table 3.2 for the country as a whole. Similarly, they calculate that only 35% of benefits spells last one year or less, compared to around 58% of spells with a duration below 12 months in my analysis.

Even though the benefit receipt rate in urban Rotterdam is two to three times as high as the one for the country as a whole, heterogeneity in spell durations within the country does not appear to be responsible for the differences in findings. I use the postcode information in the SSB data to restrict my sample to recipients who live in city of Rotterdam at the beginning of their spell. Over the eight-year period, I calculate median and mean spell lengths of 10 months and 21 months, respectively. 53% of all spells last longer than 12 months. These numbers are only a little higher than those reported in Table 3.2 for the entire country and thus still much lower than those reported by Snel et al.. Since there are no apparent differences in methodology or sample selection between the two studies, the much higher spell durations calculated by Snel et al. might simply be due to different observation periods. More specifically, the 1999 inflows sample used by Snel et al. was drawn before the start of my observation period and at a time when benefit receipt rates were still much higher.[24] The differences in spell durations in the two studies might thus hint at a positive relation between spell durations and the receipt rate at the start of a spell. Unfortunately, it was until now beyond the scope of this project to study this aspect in more detail.

[24]Based on the SSB data, I calculate a benefit receipt rate for the city of Rotterdam of 15.5% in 1999, compared to 11.9% over the years 2001 to 2008 which I look at.

3.6.4 The length of benefit spells – a cross-sectional perspective

The analysis of benefit spell lengths just presented is based on a sample of all inflows over an eight-year observation period. Data required for such an analysis are however rarely available. Policy debates about benefit dependence therefore commonly refer to the duration of *on-going* spells measured at a single point in time. In particular, discussions are often concerned with the proportion of long-term recipients among those *currently* in receipt of benefits. Conclusions based on such cross-sectional samples can be highly misleading.

In their seminal study on the duration of welfare benefit spells in the U.S., Bane & Ellwood (1994) emphasize that in a cross-sectional sample, a snapshot of all on-going spells (a sample of 'stocks') will yield very different spell durations than a sample of all spells that start or end in that period (samples of 'flows'). To illustrate the importance of this point, I re-produce part of the analysis done by Bane & Ellwood to provide evidence on the gap in measured spell durations when comparing stock and flow samples for the four countries studied in this chapter.[25]

Table 3.3 shows the duration of benefit spells for each country measured at a single point in time calculated for *(i)* a sample of on-going spells, *(ii)* a sample of starting spells, and *(iii)* a sample of ending spells. To minimize the impact of censoring, I use for my calculations the wave in the middle of the observation period, *i.e.* January 2005. In all three samples, I account for short exits from benefits (as in the 'standard sample' used for panel A of Table 3.2). The results are however robust to choosing different sampling waves and to ignoring short interruptions in benefit receipt.

Both median and mean spell durations in the stock samples of on-going

[25]Bane & Ellwood use annual data from the PSID to calculate the benefit spell duration in years for an inflow sample, and compare these numbers to *estimated* spell durations for a 'point-in-time' sample under the assumption that benefit dynamics are in steady state. For an earlier empirical illustration of this point, see Dahl & Lorentzen (2003a)

Table 3.3: *The length of benefit spells – stock vs. flow samples*

country	sample	# of spells	duration in months		share of censored spells in %
			median	mean	
Luxembourg	stock	8,516	60	60.4	59.1
	inflows	288	13	21.8	21.6
	outflows	138	13	18.6	9.4
Netherlands	stock	1,869	67	63.2	64.1
	inflows	67	12	20.5	23.9
	outflows	39	9	18.8	23.1
Norway	stock	6,739	9	16.7	6.4
	inflows	1,747	2	4.3	0.6
	outflows	1,541	2	3.7	0.3
Sweden	stock	92,607	17	26.2	15.1
	inflows	13,681	2	5.5	1.6
	outflows	18,289	3	6.2	1.6

Note: Calculations are based on monthly administrative data from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd), and Sweden (NBHW database) over the period from January 2001 to December 2008. I use the term stock sample to refer to a sample of spells that are ongoing in January 2005. Inflow and outflow samples are the samples of spells starting and ending in January 2005, respectively.

spells are much longer than for samples of either inflows or outflows.[26] The magnitude of this effect varies, but is considerable for each of the four countries. The gap between mean durations is largest in Sweden, where the spells that are on-going in January 2005 last 26 months on average, while the average duration of spells starting in that same month is nearly one fifth of this (5.5 months). The gap between median durations is larger still: For the Netherlands, 50% of all on-going spells in January 2005 lasted 67 months or longer while the median duration of spells that start and end in that same month are only 12 and 9 months, respectively. These numbers still underestimate the true size of the gap because a much larger fraction of spells in the stock samples are censored.[27]

The explanation for this striking result is that longer spells are overrep-

[26]Table 3.3 shows that in Luxembourg, the Netherlands and Norway, where the number of recipients has risen in January 2005, spell durations are higher in the inflow than in the outflow sample. The opposite is true for Sweden, where the number of outflows surpasses the number of inflows, and where spell durations are lower in the inflow sample. In equilibrium, the measured spell durations in inflow and outflow samples are equal on expectation.

[27]The stock sample suffers from both a right- and left-censoring problem, while the spells in the inflow or outflow sample can only be either right- or left-censored.

resented in samples of on-going spells. Any non-censored spell is included in exactly one inflow and one outflow sample, while possibly being part of a multitude of separate stock samples for different waves (depending on the spell's duration). This is equivalent to saying that each stock sample includes a disproportionately large number of long spells.

Any inference on spell durations that is based on a sample of on-going spells will therefore overestimate the issue of long-term benefit dependence. The large share of long benefit spells in stock samples by contrast correctly indicates that long-term recipients are responsible for the most significant part of social assistance caseloads and thus of the expenditures for benefit payments in a given period (Bane & Ellwood, 1994).

3.6.5 Repeat spells and time until re-entry

An important aspect related to the length of benefit spells is whether individuals remain self-sufficient once they have stopped receiving benefits, or how long it takes until they return to benefit receipt. As seen, individual benefit spells in the Nordic countries tend to be much shorter than in Luxembourg or the Netherlands. However, Figure 3.10 indicated that individuals in Norway and Sweden return to benefits more quickly after having left the benefit rolls. This subsection quantifies the size of this effect by studying the number of benefit spells per individual and the time until re-entry.

One difficulty that arises in such an analysis is that the panel used previously for the calculation of benefit spell lengths is unbalanced. Since the analysis is based on a sample from the working-age population, individuals enter the sample late (by turning 25 years old after January 2001) or leave early (by turning 60 before December 2008). The magnitude of this effect is non-negligible: For Norway, I for instance calculate that over the period from 2001 to 2008 less than two-thirds of individuals in the sample are observed for the full 96 waves. The average observation period is 76 waves, *i.e.* a bit above six out of the eight years. As a result, the number of spells per individual counted

in the data will be an underestimate of what would be observed if the sample were balanced.

To address this problem, I restrict the sample to individuals who are of working age during the entire observation period. Individuals who join the panel late or drop out early for age-related reasons are excluded. The resulting sample is balanced except for individuals who leave the data set due to migration or death.[28] Results from the analysis are presented in Table 3.4. To test the sensitivity of the results to the sample selection criteria, I replicate my calculations for the standard sample and present the results in Appendix F, Table F.1.

The number of benefit spells per individual is inversely related to the duration of individual spells across the four countries. For Luxembourg and the Netherlands, countries with a significant proportion of long benefit spells, repeat spells are infrequent. Less than one-third of all recipients in Luxembourg have more than a single spell during the eight-year period and only 0.5% of recipients have five spells or more. In the Netherlands, 25% of recipients are observed as having multiple spells. This number is comparable to the one presented by Snel et al. (2013), who report that 80% of recipients in Rotterdam only have one single spell over the seven-year observation period. In the two Nordic countries by contrast, re-entries into social assistance are relatively frequent. More than two-thirds of benefit recipients in Norway and Sweden have multiple spells and around 30% even have five spells or more.

Especially in the Nordic countries, the number of repeat spells drops considerably if short exits from benefit receipt are ignored. Even then, however, about half of all benefit recipients have at least two spells over the eight-year period, and a sizeable minority of 11% of recipients in Norway and 7% in Sweden have five spells or more.

[28]The reason why I do not construct a truly balanced sample is that for Luxembourg and Sweden, I cannot observe whether an individual who does not receive benefits remains in the sample.

Table 3.4: *The number of benefit spells per individual*

Panel A – standard sample					
	# of	# of spells		share of individuals in %	
	of recipients	median	mean	with 2 spells	with 5 spells
Luxembourg	12,056	1	1.4	29.0	0.5
Netherlands	2,572	1	1.4	25.9	0.5
Norway	19,900	3	4.2	70.6	33.8
Sweden	348,806	2	3.6	67.1	27.1
Panel B – ignoring short exits					
	# of	# of spells		share of individuals in %	
	of recipients	median	mean	with 2 spells	with 5 spells
Luxembourg	12,056	1	1.3	23.1	0.1
Netherlands	2,572	1	1.3	21.2	0.1
Norway	19,990	2	2.3	56.3	11.4
Sweden	348,824	1	2.0	47.1	6.9

Note: Calculations are for the period from January 2001 to December 2008 and based on monthly administrative data from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd) and Sweden (NBHW database). The sample is limited to individuals aged at least 25 years at the beginning of the observation period and below 60 years at the end.
Panel A uses the standard sample while Panel B ignores exits from social assistance benefit receipt that last a maximum of two month. More specifically, spells that are interrupted by a period of non-receipt of 1-2 months are counted as continuous, but the interjacent period of 1-2 months off benefits is not counted towards the duration of the benefit spell. For example, two benefit spells of 4 months each that are interrupted by 2 months of non-receipt are thus counted as a single spell of 8 months.

These findings are robust to using an unbalanced panel for the calculations. The spell count indeed falls when age-related sample drop-out is not accounted for, the size of this effect however is small (Appendix F, Table F.1).

The negative relation that I find at the country level between benefit spell lengths and the number of spells per individual could of course be purely mechanical. As individuals in the Netherlands and Luxembourg remain on benefits for longer, they will by construction have less opportunity to return to benefit receipt over a limited observation period. Gustafsson et al. (2002), who obtain a similar result as the one presented, are hesitant to attribute much relevance to it since their observation period of 48 months is very short.

The greater spell numbers for recipients in Norway and Sweden reported

in Table 3.4 are however consistent with the higher re-entry hazards for these countries presented in Figure 3.10. These hazard rates account for the right-censoring of spells and thus for the fact that individuals with very long spells may have less time remaining until the end of the observation period to re-enter.

To quantify differences in re-entry rates across countries, I present in Table 3.5 information on the share of benefit leavers who return to benefits. More specifically, I present the proportion of social assistance leavers (with or without a repeat spell) who have re-entered benefits within a certain interval after leaving *conditional on not yet having reached the end of the observation period.*[29]

In Norway and Sweden, nearly half of all benefit leavers return to benefits within three months. This explains the earlier finding that ignoring short interruptions in benefit receipt has a much stronger impact on both measured spell durations and numbers in the Nordic countries. Only about 25% of benefit leavers in these countries remain self-sufficient for at least the first two years after their exit from benefits. By contrast, I find that less than 10% of benefit leavers in the Netherlands return to benefits within three months and two-thirds of those who are observed for at least two years after leaving remain off benefits during that time. The numbers for Luxembourg finally turn out to be very similar to those calculated by Hoynes (2000) for California, who reports re-entry rates into AFDC receipt of 23% within the first 6 months, of 33% within 12 months, and of 41% within 24 months of leaving (page 355, Table 1).

In summary, the shorter benefit spell durations in Norway and Sweden coincide with a higher propensity to return into benefits. Moreover, a substantial share of re-entries into benefits in Sweden and Norway happen relatively quickly. Without monthly data on benefit amounts, it is difficult to tell however whether these short interruptions in benefit receipt in the Nordic countries

[29] A recipient who leaves social assistance 10 months before the end of the observation period will thus be included in the sample used to calculate re-entry with in 3 or 6 months (columns II and III of Table 3.5) but not in the sample for re-entry within 12 or 24 months (columns IV and V).

Table 3.5: *The time until re-entry*

country	share of benefit leavers in % who re-enter benefits within			
	3 months	6 months	12 months	24 months
Luxembourg	12.0	22.3	32.0	42.0
Netherlands	9.6	16.1	23.1	32.6
Norway	46.7	60.8	70.9	77.8
Sweden	48.6	60.3	69.2	75.2

Note: Calculations are based on monthly administrative data for the period from January 2001 to December 2008 from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd), and Sweden (NBHW database). The table gives the share of benefit leavers who return to benefit receipt within a certain time among those who have not yet reached the end of the panel at that time.

are the result of 'administrative churning', or whether individuals indeed gain self-sufficiency for short periods, for instance by finding temporary work. The number of spells per individual is higher however in the Nordic countries even if short interruptions in benefit receipt of one or two months are ignored.

3.6.6 The total duration of benefit receipt

The findings presented thus far indicate a much higher turnover in benefit receipt in Norway and Sweden, with benefit spells being substantially shorter but repeat spells more frequent than in the Netherlands and Luxembourg. These results can be combined to give what is referred to as the 'total time on welfare' (Gottschalk & Moffitt, 1994) or the 'net duration' of benefit receipt (Leisering & Leibfried, 1999), *i.e.* the cumulative time individuals spend in benefits over the observation period. For better comparability across countries, I again restrict my analysis to a balanced panel of the years 2001 to 2008. Results are presented in Table 3.6, with Table F.2 in Appendix F providing the robustness check for an unbalanced panel.

For the majority of benefit recipients in the two Nordic countries, total time spent in benefits falls well short of one year over the eight-year observation period. Median net duration on benefits is 7 months in Norway and 10 months in Sweden. The share of recipients who receive benefits for more than two years

Table 3.6: *The total duration of benefit receipt across spells*

country	duration in months		share of recipients in % with a duration		
	median	mean	≥ 6 months	≥ 12 months	≥ 24 months
Luxembourg	32	39.8	88.7	76.3	59.5
Netherlands	23	36.3	79.9	65.6	49.6
Norway	7	16.5	56.0	39.2	23.6
Sweden	10	20.7	62.3	47.5	30.8

Note: Calculations are for the period from January 2001 to December 2008 and based on monthly administrative data from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd) and Sweden (NBHW database). The sample is limited to individuals aged at least 25 years at the beginning of the observation period and below 60 years at the end. The total period of benefit receipt is the cumulative time spent in social assistance across all spells that are observed to start during the years 2001 - 2008.

out of the eight-year period is 24% in Norway and 31% in Sweden.

In Luxembourg and the Netherlands, recipients generally depend on benefit payments for much longer: the median time spent on benefits is 32 months and 23 months, respectively. 60% of recipients in Luxembourg and half of recipients in the Netherlands remain on benefits for at least two years. The number for the Netherlands is again slightly lower than the one calculated by Snel et al. (2013), who report a median net benefit duration of 30 months over a period of seven years for Rotterdam.

In spite of the fact that recipients in Norway and Sweden are more likely to have multiple benefit spells, the total time spent on benefits is hence considerably shorter in these countries.

3.6.7 Characteristics of short- and long-term recipients

To be able to specifically tailor policies at long-term recipients early in their benefit spells, benefit administrations and employment services need to identify the risk factors for long-term benefit receipt. As mentioned earlier, a shortcoming of the monthly data used in this chapter is unfortunately that the available information on the characteristics of benefit recipients is not very rich. In this subsection, I break down groups of recipients with the longest and shortest benefit spells by individual characteristics in an attempt to characterize groups

Table 3.7: *Characteristics of recipients with short and long benefit spells*

	Luxembourg	Netherlands	Norway	Sweden
share of females in % among recipients with...				
short spells	46.4	34.1	49.5	51.9
long spells	54.6	66.2	42.6	47.9
share of single parents in % among recipients with...				
short spells	n/a	11.5	32.2	25.6
long spells	n/a	29.9	25.9	16.9
share of over-55 year-olds in % among recipients with...				
short spells	6.5	7.8	11.1	7.0
long spells	11.9	13.9	9.3	7.6
share of immigrants in % among recipients with...				
short spells	61.6*	35.9	18.8	22.8**
long spells	52.0*	48.9	36.3	52.3**

Note: Calculations are based on monthly administrative data for the period from January 2001 to December 2008 from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd), and Sweden (NBHW database). Short spells are defined as those with a spell duration in the bottom decile in the given country; long spells are those with a spell duration in the top decile. Recipient characteristics are measured in the first period of the spell. * share of recipients without Luxembourg nationality; ** proportion of recipients living in households in which one of the adults was born abroad.

that are at greater risk of long-term benefit receipt. Specifically, I consider recipients with spells in the bottom and top decile of the distribution in the respective country, and describe them in terms of available information on personal characteristics. The results of this analysis are presented in Table 3.7.

In the Netherlands and to a lesser degree in Luxembourg, women are more strongly represented among recipients who start spells with long durations. This result might be driven in part by a large share of single parents among long-term recipients. Interestingly, the gender pattern is reversed in Norway and Sweden, where, perhaps surprisingly, single parents are over-represented in the 'short spells' group. In Norway, this finding might again be linked to the fact that low-income single parents will typically receive Transitional Allowance rather than Social Economic Assistance. For Sweden, I do not have a policy-

related explanation for this finding.

In Luxembourg and the Netherlands, more senior individuals tend to remain on benefits for longer. In both countries, the share of over-55 year-olds is nearly twice as high among recipients in the top decile of spell durations compared to those in the bottom decile. If anything, the opposite is true however in Norway, and there is no obvious relation between higher age and the length of benefit spells in Sweden.

Immigrants or individuals with a foreign nationality are represented more strongly among recipients with long benefit spells. In Norway, the share of immigrants among recipients with spell lengths in the top decile is about twice as high than among those with spell lengths in the bottom decile. Similarly the share of recipients living in the household with an immigrant in Sweden is more than twice as high among recipients with long spells than among those with short spells. The gap for the Netherlands is smaller but still sizable. The fact that I find the opposite result for Luxembourg may again be driven by strong residence requirements for the Luxembourg RMG. While these restrictions were relaxed at the beginning of the observation period in 2001, the share of non-Luxembourg benefit recipients has strongly risen over the observation period. This implies that most non-Luxembourg recipients have been on benefits for a shorter (and more recent) time period.

3.7 Conclusion

In this chapter, I have studied and compared the dynamics of social assistance benefit receipt in four European countries: Luxembourg, the Netherlands, Norway, and Sweden. The analysis was based on monthly data from separate administrative registers for the four countries covering periods of 9 to 23 years until the late 2000s. In the first part of the empirical analysis, I have examined aggregate trends in benefit receipt and transition rates and related the findings derived from monthly and annualized data. In the second part, I then followed

individuals' paths into and out of benefits in all four countries for the period from January 2001 to December 2008 to describe the distribution of benefit spell lengths, the frequency of repeat spells and the time until re-entry into benefits, and the total duration of benefit receipt across spells.

Benefit receipt rates of working-age individuals do not follow a uniform trend in the four countries. Receipt rates declined since the 1990s in the Netherlands and Norway, remained stable in Sweden, and steadily increased since the late 1980s in Luxembourg. In the late 2000s, monthly receipt rates vary between less than 2% of the working-age population in Norway to over 4% in Luxembourg and the Netherlands. Annualized data of the type used in many existing studies of social assistance benefit receipt provide a good approximation of the monthly receipt rates only for Luxembourg and the Netherlands, where turnover in benefit receipt is low. In Norway and Sweden, short-term fluctuations in benefit receipt are strong, and annualized data on benefit receipt overestimate the share of the population that receives benefits in any given time by about a factor of two.

The patterns of individuals' benefit receipt are very heterogeneous across countries. In Norway and Sweden, social assistance spells are typically very short with a median duration of only two months and only about 5% to 10% of spells lasting 12 months or longer. The majority of recipients however have multiple spells, and re-entry tends to occur quickly after an exit from benefits. In Luxembourg and the Netherlands, by contrast, a substantial share of recipients are long-term dependent. Around 40% of spells in the Netherlands and 60% in Luxembourg last for at least 12 months, and 23% and 37% of spells, respectively, have a duration of 24 months or longer. Those who leave benefits are however much less likely than in the Nordic countries to return to benefits quickly, and less than one-quarter of recipients have more than one spell over an eight-year period. The median benefit duration across all spells of an individual over the eight-year period is well below one year in Norway and Sweden, and two years or above in the Netherlands and Luxembourg.

The reasons for the lower spell durations and higher entry rates in Norway and Sweden are difficult to determine. Differences in benefit dynamics are likely driven by the institutional features of the benefit system, for instance the strictness of eligibility criteria, the generosity of benefit levels, or the availability of active labour-market programmes. Unfortunately, there exists little recent cross-country evidence on the design of social assistance policies (for an exception, see Immervoll (2010)). Where social assistance is administered at the local level, as it is the case for instance in the Netherlands, Norway and Sweden, the within-country policy variation can moreover be large. Immervoll et al. (2014) provide up-to-date information on the generosity of mimimum-income benefits. We show that social assistance benefit levels in Luxembourg and the Netherlands tend to be among the highest among OECD countries reaching typically up to around 40% of national median household income. In Norway and Sweden, the income provided by social assistance is much lower corresponding to about 20% of median household income.[30] Recipients' incentives to take up work might thus be lower in Luxembourg or the Netherlands.

Also the design of the social safety net more generally might affect the dynamics of social assistance benefit receipt. As a last resort-benefit, social assistance targets individuals who do not qualify (anymore) for higher-tier unemployment insurance or assistance benefits. The design of the unemployment benefit system, *e.g.* the maximum duration of unemployment benefit receipt or the availability of unemployment assistance programmes, will therefore have a direct effect on transitions into social assistance. The same applies for the possible availability of alternative income-support programmes targeted at low-income individuals. In Norway, low benefit rates and short spell durations for Social Economic Assistance certainly reflect the fact that low-income single parents typically qualify for the more attractive Transitional Allowance. Similarly, the Netherlands, Norway and Sweden all operate comprehensive disability

[30]These numbers are derived from the OECD tax-benefit model for singles, single parents with two children or married coupled with two children.

benefit programmes, such that the number of social assistance recipients who suffer from problems of physical or mental health might be lower than in Luxembourg. Unfortunately, the data currently available do not permit extending the analysis to account for the interaction of social assistance with other benefit programmes.

Chapter 4

Time Aggregation and State Dependence in Social Assistance Receipt

4.1 Introduction

Chapters 2 and 3 of this dissertation illustrated that the individual-level dynamics of social assistance benefit receipt are characterised by a high degree of persistence over time. A similar pattern can be observed for other labour market processes, for instance the dynamics of employment or unemployment.

Two related approaches have been employed in the empirical literature to study the sources of this persistence. Duration models analyse the processes leading up to an exit from a labour market state, for instance social assistance benefit receipt. Persistence is typically introduced by allowing the exit rate from the labour market state to depend not only on individual characteristics but also on the length of current or past spells in that state. Applications of such models in labour economics typically use data collected at short, discrete time intervals such as weeks or months and thus require access to detailed event-history data. Dynamic discrete-choice models have been a popular alternative in cases where weekly or even monthly data on benefit receipt are not available. These models employ a single specification for both entry and persistence in the labour market state, where the probability of being in that state is allowed to depend on the individual's state in the previous period. Estimation of such

models requires data to also extend to periods in which the individual is outside the state being modelled. The presence of unobserved heterogeneity poses a formidable threat to identification in both duration models and dynamic discrete-choice models. Econometric methods have been devised however to account for persistent unobserved heterogeneity and thereby obtain consistent estimates of structural state dependence (Van den Berg, 2001; Heckman & Navarro, 2007).

For reasons of data availability, the recent literature on social assistance benefit dynamics has primarily relied on estimation of dynamic discrete-choice models to study state dependence in benefit receipt as described in Chapter 2. Similar models have been estimated to study the dynamics of employment (Heckman, 1981a; Hyslop, 1999), unemployment (Arulampalam, Booth & Taylor, 2000; Gregg, 2001; Biewen & Steffes, 2010) and low-income or poverty (Stewart & Swaffield, 1999; Cappellari & Jenkins, 2004; Stewart, 2007; Cappellari & Jenkins, 2008b; Biewen, 2009). More recent studies have moved to analysing the transitions between multiple labour market states using dynamic multinomial-choice models (Uhlendorff, 2006; Prowse, 2012).

A key assumption typically made in such dynamic discrete-choice models is that benefit dynamics follow a Markov process: conditional on observed and unobserved individual characteristics the first lag of the dependent variable is sufficient for predicting the outcome. Higher-order lags are assumed not to add any predictive power to the model.[1]

This conditional Markov property has important implications for the aggregation of dynamic processes over time. If a model exhibits the Markov property at the monthly level, this property carries through to the annual level when the dynamic process is aggregated over time. There will hence be a one-to-one correspondence between the benefit transition probabilities in these two speci-

[1]Two exceptions are the studies by Chay et al. (1999), who specify a second-order Markov process using monthly administrative data for California, and Andrén & Andrén (2013), who specify a third-order Markov process using annual administrative data for Sweden.

fications. If the assumed Markov property does not hold, the results obtained from the analysis – notably the estimated degree of state dependence – will be affected by the choice of the observation interval.

As indicated above, the choice of the level of aggregation in previous analyses of social assistance benefit dynamics appears in practice to be determined primarily by the availability of suitable micro-level panel data rather than to be justified by theoretical considerations. To our knowledge, there exists no study however that systematically tests the validity of the Markov assumption in an analysis of benefit receipt dynamics, and that assesses the implications of a violation of this assumption on the level of estimated structural state dependence.[2]

In this chapter, we develop a framework for evaluating whether the conditional Markov assumption is reasonable in such models of labour market dynamics. We exploit the property that if a model satisfies the conditional Markov assumption at a given level of time aggregation this characteristic will hold also at higher levels of aggregation. A well-specified model of monthly labour market dynamics for instance should thus give 12-month-ahead predictions that are comparable to the year-to-year predictions derived from a corresponding model of annual labour market dynamics. The virtue of this exercise is that it provides a test of substance rather than of statistical significance as could easily be obtained by including higher-order lags in a Markov model. Any failure of the test indicates a non-robustness of results to the choice

[2]The only analysis of time aggregation problems in dynamic discrete-choice models that we are aware of is provided by Chay et al. (1999). They estimate dynamic conditional logit models at the monthly, quarterly and biennial level to study welfare receipt dynamics in the U.S. Comparing the size of coefficient estimates and the implied predicted shares of receipt sequences with state dependence across specifications they conclude that aggregating data leads to an attenuation in the estimated level of state dependence. It is not obvious however whether estimates of state dependence from such models can be expected to be the same in receipt sequences measured at different observation intervals (see discussion in Section 4.3). A related literature discusses problems of time aggregation in dynamic time-series models (see *e.g.* Engle & Liu (1972); Tiao & Wei (1976); Mercenier & Michel (1994)) or the estimation of continuous time event-history models with discrete data (see *e.g.* Petersen & Koput (1992); Røed & Zhang (2002) for Monte Carlo evidence and Bergström & Edin (1992) using actual data).

of time unit and is therefore evidence of model misspecification.

We apply this test to assess the validity of the conditional Markov assumption for a standard dynamic random-effects probit model of social assistance benefit receipt in Norway. Using administrative data for seven cohorts of young adults over the years 1993-2008, we find that estimates of state dependence derived from a standard first-order Markov model at the monthly, annual and biennial level fail to relate the way that we should expect if the model were well-specified. The degree of estimated state dependence is found to increase with the level of time aggregation. This is the case irrespective of whether we measure annual benefit receipt once during the calendar year ('point-in-time' definition) or whether we account for benefit receipt at any time during the year ('benefit year' approach). Both approaches have been frequently used in the existing work on social assistance benefit dynamics. The magnitude of estimated state dependence at the annual level moreover differs substantially between these two approaches.

In the second part of our analysis, we extend the dynamic random-effects probit model to permit richer types of benefit dynamics. We specify a more general random-coefficients first-order Markov model that allows the processes driving entries into and persistence in benefit receipt to vary with observable and unobservable characteristics. This more flexible specification brings about a substantial rise in the estimated degree of state dependence. However, we again fail to reproduce the weak structural state dependence calculated from a monthly model over a twelve-month period when re-estimating the same model at the annual level. We further extend this specification to allow for duration and occurrence dependence along the lines proposed by Heckman & Borjas (1980). There is evidence of sizeable duration dependence in both entries into and persistence in benefit receipt as well as a significant effect of previous episodes of benefit receipt on social assistance entry rates. Predicted persistence rates for individuals who first entered benefit receipt in the last period are 37 percentage points higher than entry rates for individuals who

never touched benefits. This translates into a predicted average partial effect of benefit receipt 12 months ago on the likelihood of benefit receipt today of 5 percentage points. A simple specification test indicates that the model provides a substantial improvement over the Markov models in terms of its time aggregation properties. However, even our most complex model of benefit dynamics does not fully succeed at replicating the true data-generating process.

The remainder of this chapter is structured as follows: In the next section, we provide a short introduction to the institutional framework of social assistance benefits in Norway and present the data used in our study. Section 4.3 gives a brief outline of the standard Markov models employed in the existing literature and relates the implied transition probabilities for different levels of time aggregation. Section 4.4 then provides empirical evidence on the performance of a first-order Markov model of social assistance benefit dynamics in Norway. In Section 4.5, we present results from two random-coefficient models that allow for heterogeneity in entries and persistence in benefit receipt, first without and then with duration and occurrence dependence, and examine the time aggregation properties of these models. Section 4.6 concludes.

4.2 Institutional background and data

The primary social assistance benefit in Norway is Social Economic Assistance (SEA, *Økonomisk sosialhjelp*), which is regulated by the Social Services Act (*Loven om sosiale tjenester*).[3] It guarantees a minimum income to all individuals who are unable to fully cover living expenses through own means in the form of earnings, savings, social insurance benefits, or payments from other minimum-income benefit programmes. SEA is means-tested and income

[3]There exist other means-tested income-support programmes in Norway like the Transitional Allowance (*Overgangsstønad for enslige forsørgere*) paid to needy single parents and the Housing Allowance (*Bostøtte*) for low-income households as described in Chapter 3. In this analysis, we restrict our attention to the dynamics of SEA benefits. Earlier analyses of social assistance dynamics in Norway are presented by Dahl & Lorentzen (2003a,b), Lorentzen & Dahl (2005), Hansen (2009), and Lorentzen (2010).

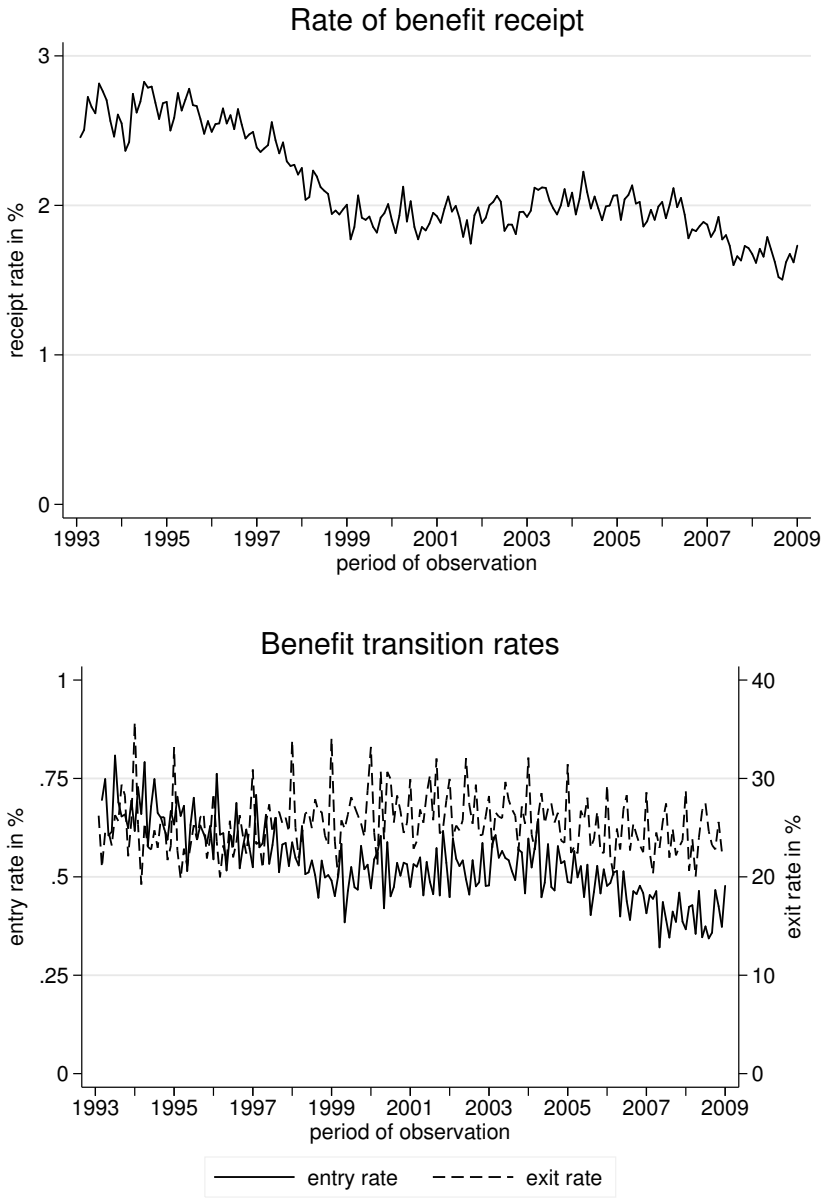
of other family members is taken into consideration when eligibility is determined.[4] While SEA benefits are explicitly designed to provide temporary income support, the maximum possible duration of benefit payments is in principle unlimited. The minimum period of benefit receipt is typically one calendar month.

Figure 4.1 presents first descriptive evidence on the dynamics of SEA benefit receipt in Norway for the period from January 1993 to December 2008. The upper panel plots the development of the monthly rate of social assistance benefit receipt over the observation period. We see that the frequency of benefit receipt in Norway is low and declines from around 2.5% in 1993 to just above 1.5% in 2008. The lower panel of Figure 4.1 suggests that this fall results primarily from a slight decline in entry rates into benefit receipt from just above 0.5% in the mid-1990s to below 0.5% in the 2000s. Month-to-month exit rates from benefit receipt are relatively stable over time around 20-30%. At an implied persistence rate in benefit receipt from one month to the next of about 70-80%, 'raw' state dependence in benefit receipt – *i.e.* the difference between observed persistence and entry rates – is substantial.

For our analysis we use data from a range of administrative registers maintained by Statistics Norway that are matched to form a monthly panel for the years 1993 to 2008. The primary source of data is the social security event-history database FD-Trygd (*ForløpsDatabasen-Trygd*). It is a collection of longitudinal data sets that provide detailed information on spells of employment and benefit receipt from unemployment insurance and income-support programmes for the entire Norwegian population. The data were drawn

[4]Generosity of SEA benefits as well as the administration of payments is largely left to the discretion of the 430 Norwegian municipalities. The municipal governments determine so-called social assistance benefit norms as guidelines for the level of monthly payments to be made to a 'standard' recipient. In practice, actual payments then depend on the caseworker's assessment of the applicant's needs. Since 2001, the central government additionally provides national social assistance norms that are updated annually and aim at equalizing benefit payments across municipalities. These norms however are not binding. The municipal benefit administration may impose additional conditions on the claimant that reflect the need for support and promote self-sufficiency.

Figure 4.1: *Monthly aggregate benefit dynamics*



Note: Calculations are based on a 10% random sample from the working-age population (age 25-59 years). The benefit receipt rate gives the number of individuals in a benefit-receiving household divided by the total working-age population. The entry rate is calculated as the number of individuals who receive benefits in period t but not in period *t-1* divided by the number of benefit recipients in period *t-1*. Similarly, the exit rate is calculated as the number of individuals who do not receive benefits in period t but received benefits in *t-1* as a share of all benefit recipients in *t-1*. Source: FD-Trygd, January 1993 to December 2008.

from various registers maintained by the Norwegian Labour and Welfare Service (NAV, *Nye Arbeids- og Velferdsetaten*) and the Norwegian Tax Authority (*Skatteetaten*). The socio-demographic data used in our study come from additional administrative registers maintained by Statistics Norway. These provide individual-level data on sex, age, marital status, immigrant status, and information on the municipality of residence. Information on household composition comes from the Central Population Register, data on educational attainment are taken from the Norwegian National Education Database.

The resulting panel data set has a few distinct advantages: First, the number of observations is extremely large. The data pertain to all legal residents in Norway irrespective of their employment or social security status. Every child born in Norway during the observation period is automatically added to the register and becomes part of our data set. Similarly, every individual who migrates to Norway to live or work in the country is added to the data set. Individuals only disappear from the data in case of emigration or death. Second, the length of our panel is exceptional. Since the records are updated monthly our observation period of 16 years implies that we can observe individuals for up to 192 monthly waves. Third, the quality of the data is high. For every individual, a large number of personal characteristics are observed on a monthly basis. Personal and household identifiers allow us to link children to their parents to construct household-level variables. Being register-based, the data set moreover does not suffer from any unnatural attrition or non-response.

We construct the sample for our analysis by restricting the population data set to individuals who match a certain set of selection criteria: We limit our sample to individuals who turn 18 years of age in the month of January of the years 1993 to 1999. All individuals belonging to one of these seven cohorts are then followed from the month in which they turn 18 for a period of up to ten years. After those ten years, they are dropped from the sample. Individuals may leave the sample before through emigration or death. We use the first twelve months of an individual's 120-month observation period to construct a

benefit receipt history that we use for conditioning in the more complex models presented in Section 4.5. Each individual thus stays in the estimation sample for 9 years (108 months) from the month of January when turning 19 to the December just before turning 28 years of age.

The main motivation for using these non-standard sample selection criteria is to guarantee that we observe all individuals from the beginning of their 'social assistance careers'. This will be the case as individuals in Norway are generally not entitled to receive social assistance benefits before reaching the age of 18 years. From that age, needy individuals claim benefits for themselves even if they continue to live with their parents. By restricting the sample to individuals who turn 18 years in January during the first seven years of our observation period, we make sure that we observe the first period of potential benefit receipt for each individual.[5] By dropping individuals after ten years we make sure that the resulting sample is weakly balanced.[6]

The population that results from these selection criteria consists of 30,899 individuals and 3,279,708 person-month observations. We estimate both the monthly and annual models for this population of individuals. The disadvantage of our selection procedure is that the resulting sample is clearly no longer representative of the Norwegian working-age population. To illustrate that our main results hold for a more standard sample, we have replicated the first part of our analysis using a 5% random sample of all 25-59 year-olds and present the corresponding results in the working paper version of this chapter (Bhuller et al., 2014, Appendix A.3). These results are not included in this dissertation

[5]This is important because to test for occurrence dependence in benefit receipt in Section 4.5.2, we need to be able to count the total number of benefit spells an individual has had. Since we lack information about any benefit receipt an individual might have had prior to the year 1993, we restrict the sample to individuals whom we observe since the beginning of their social assistance careers.

[6]Consistency of our estimations – in particular of the approach we use to control for the endogeneity of initial conditions – requires that an individual's participation in the sample be unrelated to the outcome variable. This is arguably unproblematic in the case of administrative data. By dropping individuals after 10 years, we however avoid that earlier cohorts are observed for longer and thus at higher ages than the following cohorts. In constructing our weakly balanced panel we follow an approach used by Prowse (2012).

for the sake of brevity.

Table I.1 in Appendix I gives an overview of the sample sizes and attrition rates for the different cohorts. The number of individuals in the sample is very even across years varying from 4,168 young people in the 1996 cohort to 4,883 individuals in the 1993 cohort. Sample dropout before the end of the ten-year observation period is low: In all seven cohorts, the average observation period per individual is 118 months, and only around 4% of individuals leave the sample before reaching the age of 28 years. This confirms that the panel can indeed be considered as weakly balanced. Table I.2 in Appendix I presents changes in sample characteristics over the observation period. As expected, sample members have become ten years older over the observation period, and have increased their level of education by on average 3.5 years. The family type variables illustrate that a lot of the young people leave their parents' home during the observation period: In the final wave, they are much more likely to live single and much less likely to live in a single-parent household or a household of type 'couple with children' (in which they were typically coded as the children). The average local unemployment rate dropped by more than 50% from 6.7% in the initial waves to 3.1% in the final waves of the observation period.

Another choice required for any analysis of social assistance benefit dynamics concerns the appropriate period and unit of observation. As outlined, existing analyses have typically been carried out based on annual data. Depending on the nature of these data, the approach typically used for defining the annual social assistance benefit variable differs. Survey data usually provide information on benefit receipt *at the time of interview*. Where this is the case, researchers have usually opted for a 'point-in-time' approach by modelling benefit transitions from one annual interview to the next making little or no assumptions on whether an individual received any payments in between those dates (see *e.g.* Cappellari & Jenkins (2009), Chapter 2 of this dissertation, Riphahn & Wunder (2013) and Wunder & Riphahn (2014)). Administrative

records by contrast often contain data on the total amount of benefits received *during the calendar year* without however providing any more detailed information on the timing of receipt. Researchers working with such data have usually employed a 'benefit year' approach classifying an individual as a recipient if a positive amount of benefits was received over the entire year (Hansen & Lofstrom, 2008, 2011; Andrén & Andrén, 2013; Hansen et al., 2014). We replicate both of these approaches and compare the results. Finally, as in other countries, social assistance benefits in Norway are paid at the family level. We therefore follow the standard approach of setting the binary benefit receipt variable equal to one for an individual if any member living in the same household in the given period receives benefits, to then model benefit receipt dynamics at the individual level.

4.3 Time aggregation in a conditional Markov model

Dynamic discrete-choice models have been the primary tool for the analysis of state dependence in social assistance benefit receipt. These models rely on the assumption that benefit receipt dynamics follow a conditional Markov process. An important implication of this Markov property is that if a model satisfies the property at the monthly level, the property carries through to the annual level if the process is aggregated over time. If the assumed Markov property is not valid, estimation results and in particular the degree of estimated state dependence will depend on the choice of time unit.

In this section, we show how the conditional transition probabilities estimated from a standard dynamic random-effects probit (DREP) model can be aggregated over time. This method is then used in the following section to assess the time aggregation properties of a model of social assistance benefit dynamics based on data from Norway. For a more extensive discussion of the DREP model, see Chapter 2.

4.3.1 The dynamic random-effects probit model

We define a binary outcome variable y_{it} such that for $y_{it} = 1$ individual i is in social assistance receipt in period t. We then specify the model as

$$\begin{aligned} y_{it} &= \mathbb{1}\left\{\lambda y_{it-1} + x'_{it-1}\beta + \alpha_i + \varepsilon_{it} > 0\right\} \\ &\quad \text{for } i = 1, ..., N;\ t = 1, ..., T_i, \end{aligned} \tag{4.1}$$

where individual i's benefit receipt status in period t depends on the benefit receipt status in the previous period y_{it-1}, a vector of observable individual and household characteristics characteristics x_{it-1}, a time-invariant individual-specific term α_i and a transitory error term ε_{it}. As earlier, the transitory error term ε_{it} is assumed to be distributed standard normal, to be uncorrelated with both α_i and the regressors y_{it-1} and x_{it-1} and to be serially uncorrelated.[7]

For this specification, the conditional probability of social assistance benefit receipt is given as

$$P(y_{it} = 1|y_{i0}, ..., y_{it-1}, x_i, \alpha_i) = \Phi(\lambda y_{it-1} + x'_{it-1}\beta + \alpha_i), \tag{4.2}$$

where $x_i = (x'_{i0}\ ...\ x'_{iT})'$ is a $k \times (T+1)$ vector of all k covariates over the T_i time periods plus the initial period, and $\Phi(\cdot)$ is the standard normal cumulative distribution function.

The model described by Equations (4.1) and (4.2) rests on two crucial assumptions. First, it assumes that the social assistance dynamics are correctly described by a first-order Markov model. Conditional on observed and unobserved characteristics and the first lag of the outcome variable y_{it-1}, higher-order lags do not provide any additional explanatory power. Second, we require that the observed characteristics x_i be strictly exogenous. Once we condition on the individual-specific unobserved effect α_i only the observable characteristics in period $t-1$ matter for determining y_{it}. An impact of earlier values of x_i

[7] The inter-temporal correlation of the composite error $\alpha_i + \varepsilon_{it}$ is thus assumed to be constant at $\rho = \frac{\sigma^2_\alpha}{1+\sigma^2_\alpha}$ for any two periods.

on the dependent variable or feedback effects between the dependent variable and current or future values of x_i are ruled out.[8] Under these conditions, we can interpret the coefficient of the lagged dependent variable λ as measuring structural state dependence. Spurious state dependence induced by persistent unobserved heterogeneity is captured by the individual-specific effect α_i.

As in Chapter 2, we address the initial conditions problem using the method proposed by Wooldridge (2005). We assume that the individual-specific term can be decomposed as $\alpha_i = \gamma_0 + \gamma_1 y_{i0} + \bar{x}_i'\gamma_2 + a_i$ and let the remaining error term a_i be distributed as $(a_i|y_{i0}, x_i) \sim \mathcal{N}(0, \sigma_a^2)$. Using this distributional assumption, we can integrate out the persistent error component and write the joint density of $y_{i1}, ..., y_{iT}|y_{i0}, x_i$ as

$$\int \prod_{t=1}^{T} \Big\{ \big[\Phi(\lambda y_{it-1} + x_{it-1}'\beta + \gamma_1 y_{i0} + \bar{x}_i'\gamma_2 + a_i)\big]^{y_{it}} \big[1 - \Phi(\lambda y_{it-1} + x_{it-1}'\beta + \gamma_1 y_{i0} + \bar{x}_i'\gamma_2 + a_i)\big]^{(1-y_{it})} \Big\} \left(\frac{1}{\sigma_a}\right) \phi\left(\frac{a}{\sigma_a}\right) da. \tag{4.3}$$

where $\bar{x}_i$ is again a vector of individual longitudinal averages of all time-varying observables. As will become clearer below, an advantage of using the Wooldridge approach – next to it being computationally simple – is that it extends in a relatively straightforward way to settings with richer state dependence (see the further discussion in Appendix H and an earlier application by Stewart (2007, p. 527)).

Since the likelihood function does not have an analytical solution we estimate all specifications using a mean- and variance-adaptive Gauss-Hermite quadrature approximation routine for likelihood evaluation and use a Newton-Raphson algorithm for maximization (Rabe-Hesketh et al., 2005). Results for the DREP model that are presented in Section 4.4 have been obtained using Stata's `xtprobit` command, but a robustness check using Matlab gave nearly

[8] Using German data, Biewen (2009) provides evidence on such feedback effects in a joint model of poverty, employment, and household composition, where employment status and household composition are allowed to depend on past poverty status. In our analysis we follow the standard approach in the literature on social assistance benefit receipt dynamics in assuming no such feedback effects exist.

identical results. The more complex random-coefficients models presented in Section 4.5 have only been estimated with Matlab, because they cannot be estimated using Stata's `xtprobit` and computation time was prohibitively high for Stata's `gllamm` command.

4.3.2 Time aggregation in a dynamic random-effects probit model

A key assumption made in the above model – as in nearly all of the existing literature on social assistance dynamics – is that sequences of benefit receipt satisfy the conditional Markov property at the annual level. In reality, the minimum period of social assistance benefit receipt varies between countries but is usually much shorter than a year. Neither for the 'point-in-time' approach nor for the 'benefit year' approach it is therefore obvious from a theoretical point of view why the conditional Markov property should be fulfilled at the annual level.

One sufficient (though not necessary) condition for why the conditional Markov property may hold at the annual level is that benefit dynamics follow a conditional Markov process at the *monthly* level. For instance, Taylor & Karlin (1998, Section 3.1) show that for a Markov process with

$$P(y_t = 1|y_0, \ldots, y_{t-1}) = P(y_t = 1|y_{t-1}) \tag{4.4}$$

it must also hold that

$$P(y_{t+k} = 1|y_0, \ldots, y_{t-1}) = P(y_{t+k} = 1|y_{t-1}) \tag{4.5}$$

for any positive integer k. For a monthly observation interval, this implies that only the outcome in the current period is relevant for making a 12-month-ahead prediction. A month-to-month Markov process thus translates into a year-to-year Markov process where benefit receipt is measured every 12 months, and monthly benefit transitions can be aggregated up to the annual level. If monthly benefit dynamics follow a more complex pattern it is unlikely that the Markov

property would hold once the data are aggregated to the annual level. Note that regardless of the underlying dynamic process at the monthly level, the Markov assumption at the annual level moreover implies that benefit receipt dynamics should continue to satisfy the Markov property if we further aggregate up the data to the biennial level.

Aggregating transition probabilities over time

Monthly transition probabilities in a Markov model of social assistance receipt dynamics can relatively easily be aggregated up to the annual level. Using the results from Section 4.3.1, we define the conditional probability of entry into benefit receipt in a Wooldridge-type dynamic random-effects probit model as

$$\begin{aligned} p_{it}^{01}(y_{it-1}, y_{i0}, x_i, a_i) &= P(y_{it} = 1 | y_{it-1} = 0, y_{i0}, x_i, a_i) \\ &= \Phi(x'_{it-1}\beta + \gamma_1 y_{i0} + \bar{x}'_i\gamma_2 + a_i) \end{aligned} \tag{4.6}$$

and the corresponding persistence probability as

$$\begin{aligned} p_{it}^{11}(y_{it-1}, y_{i0}, x_i, a_i) &= P(y_{it} = 1 | y_{it-1} = 1, y_{i0}, x_i, a_i) \\ &= \Phi(\lambda + x'_{it-1}\beta + \gamma_1 y_{i0} + \bar{x}'_i\gamma_2 + a_i). \end{aligned} \tag{4.7}$$

We construct a transition matrix based on these expressions as

$$\mathcal{A}_{it}(y_{it-1}, y_{i0}, x_i, a_i) = \begin{bmatrix} 1 - p_{it}^{01} & p_{it}^{01} \\ 1 - p_{it}^{11} & p_{it}^{11} \end{bmatrix}, \tag{4.8}$$

where the dependence on covariates and the random effect of the four transition probabilities is suppressed for notational ease. For a column vector of possible states in the previous period z_{it-1}, we can write

$$\underbrace{E\begin{bmatrix} 1 - y_{it} \\ y_{it} \end{bmatrix}}_{E(z_{it}|z_{it-1}, y_{i0}, x_i, a_i)} = \underbrace{\begin{bmatrix} 1 - p_{it}^{01} & 1 - p_{it}^{11} \\ p_{it}^{01} & p_{it}^{11} \end{bmatrix}}_{\mathcal{A}'_{it}(y_{it-1}, y_{i0}, x_i, a_i)} \underbrace{\begin{bmatrix} 1 - y_{it-1} \\ y_{it-1} \end{bmatrix}}_{z_{it-1}}, \tag{4.9}$$

where the vector of expected outcomes in the current period $E(z_{it}|z_{it-1}, y_{i0}, x_i, a_i)$ contains the conditional probabilities of benefit non-receipt $E(1 - y_{it}|y_{it-1}, y_{i0}, x_i, a_i)$ and benefit receipt $E(y_{it}|y_{it-1}, y_{i0}, x_i, a_i)$. At

any time t, only one of the two potential outcomes in each of these two vectors is realized for a given individual i. Iterating Equation (4.9), we can write the vector of expected outcomes s periods ahead as

$$E(z_{it}|z_{it-s}, y_{i0}, x_i, a_i) = \left(\prod_{j=0}^{s-1} \mathcal{A}'_{it-j}\right) z_{it-s}. \tag{4.10}$$

These derivations illustrate how we are able to simply derive annual transition probabilities from their monthly counterparts if benefit receipt dynamics from period t to $t+1$ follow a Markov process.

As noted, the dynamic random-effects probit model is only assumed to satisfy the Markov property conditional on the covariates and the individual-specific effect. The transition probabilities that we would predict directly based alone on the coefficient estimates and the values of the covariates however are population-averaged, because we have integrated out the individual-specific effect for the construction of the likelihood function. To be able to assess the validity of the Markov assumption and aggregate up estimated transition matrices over time we thus need to reconstruct subject-specific transition matrices from our unconditional predictions.

Denoting the 'unconditional' transition probability matrix as $A_{it} \equiv A_{it}(y_{it-1}, y_{i0}, x_i)$ and the conditional transition probability matrix as $\mathcal{A}_{it} \equiv \mathcal{A}_{it}(y_{it-1}, y_{i0}, x_i, a_i)$, we can relate transition probabilities at the monthly level by integrating over the random effect as

$$A^m_{it} = \int \mathcal{A}^m_{it} \frac{1}{\sigma_a} \phi\left(\frac{a}{\sigma_a}\right) da, \tag{4.11}$$

where we use the superscript m to denote the monthly transition matrix. Similarly, we can write the unconditional transition probabilities at the annual level using the superscript y as

$$A^y_{it} = \int \left(\prod_{s=0}^{11} \mathcal{A}^m_{it-s}\right) \frac{1}{\sigma_a} \phi\left(\frac{a}{\sigma_a}\right) da. \tag{4.12}$$

Under the assumption of a Markov process at the monthly level, there is thus a one-to-one mapping of monthly into annual transition probabilities.

From a computational point of view, it is easier to generate predictions of the conditional probabilities by sampling from the estimated distribution of the random effect $\mathcal{N}(0, \hat{\sigma}_a^2)$ than by integrating over this distribution. To produce precise estimates of individual-specific transition rates, we would need multiple simulations for each individual. Since however we are not interested in precise estimates of individual-specific transition rates but only in the population averages, the large size of our sample implies that a single simulation per individual is sufficient.

To assess the degree of state dependence in our model, we construct predicted conditional transition probabilities $\widehat{\mathcal{A}_{it}^m}$ from our estimated unconditional probabilities $\widehat{A_{it}^m}$ using random draws of a_i. Specifically, we predict the entry probability into benefits for an individual in a given period as

$$\widehat{P}(y_{it} = 1 | y_{it-1} = 0, x_i, y_{i0}, \tilde{a}_i) = \Phi\left(x'_{it-1}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2 + \tilde{a}_i\right), \qquad (4.13)$$

and the corresponding persistence probability for the same individual as

$$\widehat{P}(y_{it} = 1 | y_{it-1} = 1, x_i, y_{i0}, \tilde{a}_i) = \Phi\left(\hat{\lambda} + x'_{it-1}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2 + \tilde{a}_i\right), \quad (4.14)$$

where $\tilde{a}_i$ is a random draw from the estimated distribution of the random effect. One of these two predicted transition probabilities will always describe the counterfactual.

The difference between the average predicted transition probabilities over all individuals and time periods gives us the estimated average partial effect (APE) as

$$APE = \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} \Big[\widehat{P}(y_{it} = 1 | y_{it-1} = 1, x_i, y_{i0}, \tilde{a}_i) - \widehat{P}(y_{it} = 1 | y_{it-1} = 0, x_i, y_{i0}, \tilde{a}_i)\Big]. \qquad (4.15)$$

This APE describes the estimated structural effect of benefit receipt last month on the probability of social assistance benefit receipt in the current month.

The two monthly transition probabilities for each individual and period given by Equations (4.13) and (4.14) can also be aggregated up to the annual

level as described. During this aggregation process, we keep all observable characteristics fixed at their values in period $t-12$, which corresponds to the period in which they are measured in the annual model. Taking the difference between the two aggregated counterfactual probabilities averaged over individuals and years we consequently obtain the *annual* average partial effect implied by the estimates from the *monthly* model. This effect can be readily compared to the average partial effect derived from the transition matrix constructed directly from the estimates of the annual model.

As for the monthly-to-annual aggregation, we can also aggregate the annual transition probabilities to the biennial level using an expression analogue to that in Equation (4.12) and compare these to the transition probabilities estimated directly from a biennial model. This allows us to test the assumption commonly made in the literature that social assistance benefit receipt dynamics follow the conditional Markov property at the annual level. Note that the validity of this time aggregation exercise does not depend on whether we use the 'point-in-time' approach or the 'benefit year' approach to define the annual benefit receipt variable.

In practice, rather than to aggregate transition probabilities over time, we directly simulate each individual's benefit receipt path over the twelve-month period. Specifically, we predict an individual's benefit receipt status one period ahead as

$$\hat{y}_{it+1} \quad = \quad \mathbb{1}\left\{\hat{\lambda} y_{it} + x'_{it}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2 + \tilde{a}_i + \tilde{e}_{it+1} > 0\right\}. \qquad (4.16)$$

where now both $\tilde{a}_i$ and $\tilde{e}_{it+1}$ are random draws from the respective estimated error distributions. We then use the value for $\hat{y}_{it+1}$ to again predict the outcome in period *t+2* and so forth. By iterating this process twelve times, we can construct an individual's predicted benefit receipt path over an entire year. The result of this aggregation exercise will be the predicted value of the binary outcome variable $\hat{y}_{i12}$ rather than a predicted transition rate from period t to *t+12*. To calculate the degree of annual state dependence implied by the

monthly model, we simply compare the simulated rate of benefit receipt across individuals twelve periods ahead obtained by setting the current benefit receipt variable y_{it} equal to zero and equal to one, respectively, for for all individuals and years. Again, we fix the covariates during this process at their values at time t.

The advantage of making period-by-period predictions of a benefit receipt path over aggregating transition matrices is that the approach just described extends easily to the more complex models with duration and occurrence dependence discussed in Section 4.5.2. For all Markov models that we estimate, we however verified that the two approaches indeed yield identical results.

Comparing transition probabilities at different levels of time aggregation

We have illustrated above that a model with the conditional Markov property at the monthly level will carry over that property to higher levels of time aggregation. In other words, if our monthly benefit dynamics follow a Markov process, there exists a model describing the annualized data that also satisfies the conditional Markov property, with the transition probabilities from that model being known functions of the monthly transition probabilities.

Our conjecture is now that for a monthly process that can be adequately described by a dynamic random-effects probit model at the monthly level, the process observed in the annualized data can also be approximated by a random-effects probit structure at the annual level. We hence test the monthly model by comparing predictions obtained from the same dynamic random-effects probit model estimated on monthly and annualized data, aggregating the predictions from the monthly to the annual level as explained above.

This approach comes with a subtle qualification: unlike for a linear autoregressive model, for which the time aggregation is exact, a dynamic random-effects probit model at the monthly level does not aggregate exactly to a dynamic random-effects probit model at the annual level. While the aggregated

annual conditional transition probabilities will be functions of the same covariates and random effects that enter the baseline monthly model, they cannot necessarily be approximated by the same probit functional form. Our approach for testing the Markov property therefore relies on the assumption that not only the Markov property but also the probit structure carries over from the monthly to the annual level.

Fortunately, there is an easy way of testing the validity of our approach. If the probit approximation is appropriate, an annual dynamic random-effects probit model should give results identical to those aggregated from a monthly dynamic probit model *if the annual model is estimated on data that have been generated by this monthly model.* As a robustness check for our time aggregation exercise, we therefore (i) simulate data based on each monthly model we estimate, (ii) estimate the corresponding annual model on an annualized version of these simulated data, and (iii) compare the predictions from the annual model estimated on simulated data with the aggregated predictions from the monthly model that was used to construct the simulated data. Any substantial difference between the two predictions will indicate that the dynamic probit structure does not carry over to higher levels of time aggregation. The simulation exercise thus provides a safeguard against falsely rejecting the conditional Markov property only because the probit structure we assume is a bad approximation at higher levels of time aggregation.

4.4 An empirical test of the Markov model for social assistance transitions

In this section, we test the assumption that the dynamics of social assistance entry and persistence can be jointly described by a Markov process. We estimate a series of dynamic random-effects probit models based on monthly, annual and biennial data and calculate the degree of estimated state dependence for each of these models. We then use the model estimates to simulate benefit transitions

from the monthly to the annual level and from the annual to the biennial level. The comparison of estimated and aggregated transition matrices at the annual and biennial level serves as our test of the Markov property. We also compare results based on the 'point-in-time' approach and the 'benefit year' approach for defining the annual benefit receipt variable and assess the time aggregation properties of these two approaches.

In all our models, we control for the usual set of individual characteristics (sex, age, years of education, and immigrant status), household characteristics (family type, household size, and having a child aged 0-5 years), and partner characteristics (age, years of education, and immigrant status) for married or cohabiting individuals. All our specifications moreover include year dummies and a control for the municipal unemployment rate. For an overview of how these variables are defined, see Appendix G, Tables G.1 and G.2. Some descriptive statistics are provided in Appendix I, Table I.3.

Since the focus of our chapter is on the analysis of state dependence in social assistance benefit receipt, we limit ourselves to reporting average predicted transition rates and the corresponding average partial effect of past benefit receipt for each specification. Coefficient estimates for the various specifications are reported in Appendix J.

4.4.1 Time aggregation for the 'point-in-time' benefit variable

We begin our empirical analysis by looking at the case of the standard dynamic random-effects probit specification with an outcome variable that measures benefit receipt at one point in time. As outlined in Section 4.2, our data provide monthly information on the receipt of social assistance benefits in Norway, which we use to estimate this model at the monthly level. To construct a data set of annual 'point-in-time' observations comparable to the ones derived from household surveys, we discard eleven of the twelve monthly observations for an

individual in each year and only keep the benefit receipt status in December.[9] We then compare the effect of benefit receipt in December of year t on the probability of benefit receipt in year $t+1$ implied by the monthly and the annual specification.

Table 4.1 provides evidence on the time aggregation properties of the monthly model. In columns (1) and (2), we report the average predicted transition rates and the resulting average partial effect of the lagged dependent variable obtained directly from estimating our model using monthly and annualized data, respectively. In column (3), we then give the benefit transition rates and APE implied by our monthly model when aggregated to the annual level. The results in column (4) were obtained by estimating the annual model on data simulated from the monthly model as a robustness check of our functional-form assumption in the annual model.

The results presented in columns (1) and (2) suggest that benefit dynamics are characterized by a considerable degree of state dependence both at the monthly and at the annual level. From our monthly model, we estimate that on average sample members have a 14.0% probability of receiving social assistance benefits this period if they already received benefits last month. This compares to a probability of entering benefits from last month to the current month of only 1.4%. The resulting average partial effect, *i.e.* the effect of benefit receipt last month on the probability of benefit receipt this month, is 12.6 percentage

[9]The advantage of using December as the month for our annual observation is that we observe individuals for an entire year after they enter the sample in January. The approach thus resembles the one used for the 'benefit year' definition, where benefit receipt is recorded over the entire year but household composition and observable characteristics are measured in December. For reasons that will become clearer later, our approach is advantageous moreover for the models of duration and occurrence dependence that we present in Section 4.5. As mentioned in Chapter 3, there is some seasonality in benefit receipt for individuals in their low 20s, who are more likely to access social assistance benefits during the summer months to bridge gaps in their educational schedules. When including calendar month dummies in our standard specification, we however find that in spite of the large sample size, only two of the eleven dummies are estimated to have a coefficients statistically different from zero. The APEs of these two calendar month dummies on benefit receipt are just above 0.1 percentage points. We therefore conclude that the choice of December as the month for our annual observation is unlikely to affect our results.

Table 4.1: *Time aggregation: monthly to annual ('point-in-time')*

	(1)	(2)	(3)	(4)
	Monthly model	Annual model	Monthly-to-annual time aggregation	Simulation-based specification check
Average predicted persistence rate (in %)	14.0 (0.2)	7.0 (0.2)	3.1 (0.1)	2.9 (0.0)
Average predicted entry rate (in %)	1.4 (0.0)	2.1 (0.1)	3.1 (0.1)	2.9 (0.0)
Average partial effect (in ppts)	12.6 (0.1)	4.9 (0.2)	0.0 (0.0)	0.0 (0.0)

Note: The monthly model in column (1) and the annual model in column (2) are the dynamic random-effects probit specifications estimated on monthly and annualized data, respectively. The annualized data for model (2) were obtained by keeping the observations for December of each year only. The monthly-to-annual time aggregation results presented in column (3) were obtained by aggregating the transitions of the monthly model to the annual level using the simulation-based approach described in Section 4.3.2. Column (4) gives the results from a dynamic random-effects probit model estimated on annualized data simulated from a monthly dynamic random-effects probit model. Standard errors in parentheses of columns (1) to (3) were calculated using 10 bootstrap replications; predicted transition rates and standard errors in parentheses of columns (4) were calculated by running the annual model on 10 simulated data sets. Coefficient estimates for the two specifications presented in columns (1) and (2) are reported in Appendix J, Table J.1.

points. Estimates from the annual model imply a somewhat lower average partial effect at the annual level. Conditional on benefit receipt in December of last year, the probability of benefit receipt in December of this year is 7.0%, compared to a 2.1% probability of receiving benefits today conditional on non-receipt twelve months ago. The resulting average partial effect is 4.9 percentage points.

Our main interest however lies in the comparison of the results from the annual model with the aggregated results presented in column (3). As discussed, the assumption of a Markov process at the monthly level implies that benefit dynamics should also display the Markov property at the annual level. Thus, we should be able to aggregate up monthly transition probabilities to the annual level and obtain results comparable to those estimated from an annual model directly.

The results in column (3) of Table 4.1 however indicate that based on the estimates of our monthly model, the probability of benefit receipt in the cur-

rent period is virtually independent of whether an individual did or did not receive social assistance benefits twelve months ago. Our simulation-based results give an annual average partial effect of zero, which is moreover very precisely estimated. Intuitively, the relatively low average predicted month-to-month persistence probability of 14% implies that very few individuals remain on social assistance for the entire year. The predicted probability of benefit receipt thus quickly converges to the equilibrium rate of 3.1% even for those individuals who received benefits twelve months ago.

Our dynamic random-effects probit model estimated at the monthly level thus predicts that benefit receipt twelve months ago does not have an impact on the probability of benefit receipt today once we control for observed and unobserved heterogeneity. The finding of no year-to-year state dependence on benefits based on the monthly model clearly differs from the one obtained from our annual model directly, where we estimated an average partial effect of 4.9 percentage points. This leads us to conclude that the Markov assumption is not satisfied at the monthly level.

As discussed in Section 4.3.2, the described exercise is valid only if the benefit receipt process observed in the annualized data can be approximated by a similar type of dynamic random-effects probit model as the probit model we estimated at the monthly level. To check the validity of this assumption, we report results from a simulation exercise in column (4) of Table 4.1. We constructed ten data sets based on the estimates from the monthly model, each of which we used to extract annual data. We then estimate an annual dynamic random-effects probit model on these data and calculate predicted transition rates and average partial effects. Column (4) presents the mean and standard deviation of these transition rates and the average partial effect over the ten replications.

The results obtained from estimating the annual model based on simulated data are nearly identical to those derived from aggregating monthly predicted transition rates to the annual level as presented in column (3). This suggests

that it is indeed reasonable to assume that our dynamic random-effects probit assumption carries through from the monthly to the annual level. The difference in results between the two annual models estimated on the true data (column (2)) and the simulated data (column (4)) moreover highlight again that the true data-generating process is very different from the monthly dynamic random-effects probit model that we have used for simulation.

The same exercise shows that the time aggregation properties implied by the Markov assumption also fail to hold at the annual level. This time, we aggregate up results from an annual model to the biennial level and compare the level of state dependence with that obtained from a specification estimated on biennial data directly. These biennial data were constructed by keeping the observation for December of every second year only while dropping all remaining 23 months.

In analogy to above, we report in columns (1) and (2) of Table 4.2 the estimates obtained directly from the annual and biennial models, while column (3) gives the average predicted transition probabilities and APEs obtained from aggregating annual benefit transitions to the biennial level. As before, we find significant state dependence both in the annual and biennial model of social assistance dynamics with APEs of 5.2 and 3.5 percentage points, respectively. Aggregated to the biennial level, the transition probabilities from the annual model however imply an average partial effect of only 1.5 percentage points. The state dependence over two years implied by a Markov model at the annual level is thus substantially lower than the APE we obtain from estimating the model directly with biennial data. For a point-in-time measure of benefit receipt, the assumption that benefit dynamics follow a Markov process must thus be rejected also at the annual level.

4.4.2 Time aggregation for the 'benefit year' variable

As discussed in Section 4.3.2, the second widely-used approach for defining an annual social assistance benefit receipt variable has been to look at receipt at

Table 4.2: *Time aggregation: annual to biennial ('point-in-time')*

	(1)	(2)	(3)
	Annual model	Biennial model	Annual-to-biennial time aggregation
Avg. predicted persistence rate (in %)	7.3 (0.3)	5.7 (0.3)	3.9 (0.1)
Avg. predicted entry rate (in %)	2.0 (0.1)	2.2 (0.1)	2.5 (0.1)
Average partial effect (in ppts)	5.2 (0.3)	3.5 (0.3)	1.5 (0.1)

Note: The annual model in column (1) and the biennial model in column (2) are the dynamic random-effects probit specifications estimated on annualized and biennialized data, respectively. The annualized data for model (1) were obtained by keeping the observations for December of each year only, while in the biennialized data for model (2) we keep observations for December of every second year only. The annual-to-biennial time aggregation results presented in column (3) were obtained by aggregating the transitions of the annual model to the biennial level using the simulation-based approach described in Section 4.3.2. Standard errors in parentheses were calculated using 10 bootstrap replications. Coefficient estimates of the two specifications presented in columns (1) and (2) are reported in Appendix J, Table J.2.

any time during the calendar year. Arguably, the time aggregation problem for such a variable is even more severe because this approach makes no assumption on the timing of benefit receipt during the year. For instance, we might think of a case in which a single spell of benefit receipt extends from December of one year to January of the next, and a second case with two separate on-month spells in January and December of two subsequent years separated by 22 months without benefit receipt. These two cases are observationally equivalent under the 'benefit year' approach, whereas we would expect them to have very different implications for the degree of annual state dependence.[10]

In this subsection, we assess the validity of the Markov assumption for a model of benefit dynamics in which annual benefit variable is defined using the 'benefit year' approach. Specifically, we construct an annual data set of benefit receipt coding an individual as a benefit recipient if any benefit payments were recorded during the calendar year. In the corresponding data set of biennial observations, we only keep the observations for odd calendar years. All observable characteristics are recorded in December of the respective year. The results of this analysis are presented in Table 4.3.

[10]For a discussion of what we call the 'benefit year' approach and the advantages of point-in-time sampling, see Ellwood (1982).

Table 4.3: *Time aggregation: annual to biennial ('benefit year')*

	(1)	(2)	(3)
	Annual model	Biennial model	Annual-to-biennial time aggregation
Average persistence rate (in %)	20.1 (0.4)	12.0 (0.5)	10.6 (0.2)
Average entry rate (in %)	4.4 (0.1)	5.1 (0.1)	6.2 (0.1)
Average partial effect (in ppts)	15.7 (0.4)	6.8 (0.6)	4.5 (0.2)

Note: The annual model (1) and the biennial model (2) are the dynamic random-effects probit specifications estimated on annualized and biennialized data, respectively. The annualized data for model (1) measure benefit receipt at any time during the calendar year, while in the biennialized data for model (2) we measure benefit receipt at any time during every second calender year. The annual-to-biennial time aggregation results presented in column (3) were obtained by aggregating the transitions of the annual model to the biennial level using the simulation-based approach described in Section 4.3.2. Standard errors in parentheses were calculated using 10 bootstrap replications. Coefficient estimates of the two specifications presented in columns (1) and (2) are reported in Appendix J, Table J.3.

Both rates of benefit receipt and state dependence are substantially higher for the 'benefit year' approach than for the 'point-in-time' approach presented in Table 4.2. We calculate a predicted persistence rate into benefit receipt of 20.1% and an entry rate of 4.4%, which translates into an APE of benefit receipt in the past year on benefit receipt this year of 15.7 percentage points. This effect is more than three times as large as the one for the point-in-time measure of benefit receipt reported in column (1) of Table 4.2.

Again, we find that the model fails our time aggregation test: When estimating our model on biennial data, we obtain an APE of 6.8 percentage points; once we aggregate the results from the annual model to the biennial level, the APE drops to only 4.5 percentage points.

The Markov assumption thus seems to be violated at the annual level also when we define the benefit variable using the 'benefit year' approach. The two approaches to defining the benefit variable moreover yield very different estimates of predicted transition rates and state dependence at the annual level.

4.4.3 A model with higher-order lags

Our findings thus far show that predicted transitions of standard models of social assistance benefit dynamics do not aggregate over time as one should expect if the Markov assumption were true either at the monthly or at the annual level. An alternative, more illustrative test of the Markov specification at the monthly level is to add higher-order lags to the standard specification and check for their significance.

Consistent estimation of such a higher-order dynamic probit model is a little more complicated than it may appear at first, because like the first lag, also the higher-order lags will be endogenous due to their positive correlation with the unobserved individual-specific random effect α_i. Unless this endogeneity problem is appropriately addressed, one should expect to obtain non-zero coefficients for the higher-order terms even if these terms do not indeed matter for determining benefit transition rates.

To allow for higher-order state dependence, we modify the standard model presented in Equation (4.1) as

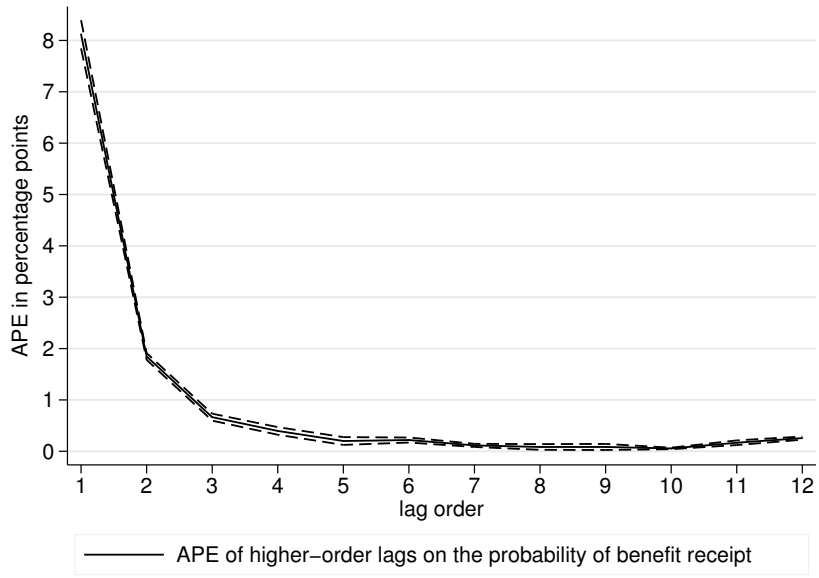
$$\begin{aligned} y_{it} &= \mathbb{1}\left\{\sum_{s=1}^{12}\lambda_s y_{it-s} + x'_{it-1}\beta + \alpha_i + \varepsilon_{it} > 0\right\} \\ &\quad \text{for } \; i = 1, ..., N; \; t = 12, ..., T_i, \end{aligned} \tag{4.17}$$

where we have included twelve lags of the dependent variable with the coefficients λ_1 to λ_{12}. We extend our controls for the initial conditions by now including the values of the outcome variable in the initial *twelve* periods of the panel in addition to the individual longitudinal averages of the covariates $\bar{x}_i$, writing

$$\alpha_i = \gamma_0 + \sum_{s=0}^{11}\gamma_{1,s} y_{is} + \bar{x}'_i\gamma_2 + a_i. \tag{4.18}$$

Under distributional assumptions similar to those discussed in Section 4.3.1, we can then integrate out α_i and estimate this model using conditional maximum

Figure 4.2: *Average partial effects of higher-order lags*



Note: Average partial effects of higher-order lags in a dynamic random-effects probit model estimated on monthly data. The dashed lines give the 95% confidence intervals obtained from 10 bootstrap replications.

likelihood methods (see Appendix H).[11]

Figure 4.2 plots the estimated average partial effects of the first twelve lags and their 95% confidence intervals. We find that all of these APEs are significantly different from zero, which means that we can reject the first-order Markov model on purely statistical grounds. Economically-speaking, however, only the second and possibly the third lag are of a meaningful magnitude with estimated APEs of 1.8 and 0.6 percentage points, respectively. Even for those two lags, the estimated effect is already much smaller than the APE of 8.1 percentage points for the first lag.

The low importance that our model attributes to the higher-order lags implies that it provides little guidance as to why the first-order Markov spec-

[11]Stewart (2007, Section 4.4) estimates a second-order dynamic random-effects probit model in his study of unemployment and low pay. He uses an extension of the Wooldridge approach similar to ours although for a more complex state space, and additionally includes interactions of the two initial lags of the outcome variable with the longitudinal averages of the covariates. Andrén & Andrén (2013) estimate a third-order Markov process in their study of social assistance dynamics in Sweden, but do not mention that they adjust their controls for initial conditions. Both of these studies are based on annual data.

ification fails as dramatically in terms of its time aggregation properties as observed in Table 4.1. Indeed, based on the results of the higher-order specification alone, we might have concluded that a first-order Markov model is a relatively good approximation to the underlying benefit dynamics. This underlines the importance of the time-aggregation test we performed in this section and which demonstrated that first-order Markov models do not provide reliable estimates of the level of state dependence in benefit receipt.

A drawback of the higher-order Markov specification in our view is moreover that it is difficult to give the higher-order lags a meaningful economic interpretation. In the following section, we therefore generalize and extend the models just presented, by allowing first for different processes for entries into and persistence in benefit receipt, and by then accounting explicitly for duration and occurrence dependence in each of these processes.

4.5 Heterogeneity in social assistance entry and exit processes

A common feature of the models discussed thus far is that they treat entries into benefit receipt and persistence in benefit receipt symmetrically in a single, joint specification. In the standard Markov model, the effect of the lagged benefit receipt status y_{it-1} on the probability of benefit receipt in the current period y_{it} is fully captured by the state dependence parameter λ. All other parameters of the model, notably the vector of coefficients β and the individual-specific effect α_i, are assumed not to depend on the lagged benefit receipt status. The model thus constrains both observed and unobserved characteristics to have the same effect on the probability of entries into and persistence in benefit receipt.

In this section, we extend the standard model in two steps to permit more complex benefit dynamics. First, we introduce a more flexible specification with a random-coefficient that allows us to capture differential effects of observed individual characteristics and unobserved individual-specific heterogeneity on

entries and persistence. For brevity, and out of lack for a better name, we refer to this specification as a dynamic random-coefficients probit (DRCP) model. As for the standard model, we again estimate this specification at the monthly level and compare time-aggregated results with those obtained directly from the corresponding annual model. Second, we further extend the DRCP model to include duration and occurrence dependence in benefit receipt, two channels through which the earlier benefit history may impact the probability of benefit receipt and that might explain the observed pattern of significant higher-order lags.

4.5.1 A dynamic random-coefficients probit model

We extend the standard dynamic random-effects probit model by abandoning the assumption of a symmetric process that determines both entries and persistence probabilities. Employing the notation already used in Equation (4.1), we write

$$\begin{aligned} y_{it} &= \mathbb{1}\left\{(\bar{\lambda}+\lambda_i)y_{it-1} + x'_{it-1}(\beta + y_{it-1}\eta) + \alpha_i + \varepsilon_{it} > 0\right\} \\ &\quad \text{for } \; i = 1, ..., N; \; t = 12, ..., T_i. \end{aligned} \tag{4.19}$$

There are two important differences between this model and the standard specification described in Equation (4.1). First, we have added a vector of interaction terms between the lagged dependent variable y_{it-1} and the covariates x_{it-1} with an associated coefficient vector η. These interactions allow the effects of covariates like age, sex, or family status to differ for the processes that determine entry into benefit receipt (as measured by β) and persistence in benefit receipt (measured by $\beta + \eta$). Second, we include a random coefficient λ_i for the lagged dependent variable that can be correlated with the random effect α_i. This random coefficient captures the *individual-specific* effect of past benefit receipt y_{it-1} on the probability of current benefit receipt y_{it} in terms of the deviation from the average effect measured by $\bar{\lambda}$. An equivalent but for our purposes more convenient interpretation of λ_i is as a second random effect that is

interacted with the lagged dependent variable and therefore allows persistent unobserved heterogeneity to differ for the entry and persistence processes.[12] Note that this random-coefficients framework generalizes the standard Markov model, as the DRCP model in Equation (4.19) collapses to the DREP model in Equation (4.1) for $\eta = 0$ and $\lambda_i = 0 \; \forall \, i$.[13]

Assuming $\varepsilon_{it} \sim \mathcal{N}(0,1)$ as before, we express the probability of entry into benefit receipt as

$$P(y_{it} = 1 | y_{it-1} = 0, x_i, \alpha_i, \lambda_i) = \Phi(x'_{it-1}\beta^0 + \alpha_i^0), \tag{4.20}$$

where $\beta^0 = \beta$ and $\alpha_i^0 = \alpha_i$. Similarly, the probability of persistence in benefit receipt is given as

$$P(y_{it} = 1 | y_{it-1} = 1, x_i, \alpha_i, \lambda_i) = \Phi(\bar{\lambda} + x'_{it-1}\beta^1 + \alpha_i^1), \tag{4.21}$$

where $\beta^1 = (\beta + \eta)$ and $\alpha_i^1 = (\alpha_i + \lambda_i)$. Thus, entry and persistence probabilities in our DRCP model can be thought of as coming from two separate equations with different coefficient vectors β^0 and β^1 and two distinct random effects α_i^0 and α_i^1, which however are allowed to be correlated.

To account for the endogeneity of initial conditions, we again follow the approach by Wooldridge (2005), which now translates into expressing the two individual-specific 'random effects' α_i^0 and α_i^1 as linear functions of the receipt status in the initial period y_{i11} and a vector of individual longitudinal averages of time-varying covariates $\bar{x}_i$ as

$$\alpha_i^j = \gamma_0^j + \gamma_1^j y_{i11} + \bar{x}'_i \gamma_2^j + a_i^j, \tag{4.22}$$

[12] Stewart (2007, Section 3.7) also introduces heterogeneity in state dependence in a model of unemployment and low-pay dynamics by specifying a random coefficient for the lagged dependent variable without however allowing the effects of the covariates to vary by lagged benefit receipt status. Meanwhile, Cappellari & Jenkins (2009) introduce interaction terms between the explanatory variables and the lagged dependent variable in a model of social assistance benefit receipt yet restrict unobserved heterogeneity to have the same effect on entries and persistence. Our specification thus combines these two approaches, moreover using monthly rather than annual data.

[13] We estimate the described model over the periods $t = 12, ..., T$ only. This however is not a feature of the model but a restriction we impose ourselves to make the results comparable to those of the model estimated in the following subsection. We therefore use y_{i11} rather than y_{i0} when referring to the outcome in the initial period.

for $j = 0, 1$. We require the residual error terms a_i^0 and a_i^1 to follow a bivariate normal distribution with zero means, variances $(\sigma^2_{a_0}, \sigma^2_{a_1})$ and a correlation τ. As in the standard case, they are moreover uncorrelated with the covariates in the model $(x_{it-1}, \bar{x}_i, y_{i11})$ and the transitory shock ε_{it}. Under these distributional assumptions, we can integrate out (a_i^0, a_i^1) to estimate the model parameters using conditional maximum likelihood methods. In the special case where a_i^0 and a_i^1 are uncorrelated, our approach is equivalent to estimating two separate equations for entries and persistence in benefit receipt.

Average predicted transition rates and the implied average partial effect for the DRCP model are presented in the upper part of Table 4.4. As earlier, we calculate the average partial effect as the average difference between the predicted social assistance persistence and entry probabilities across individuals. The APE now however reflects both the direct effect of the lagged dependent variable $\bar{\lambda}$ and the difference in the effect of observable characteristics, the Wooldridge controls, and persistent unobserved heterogeneity on the probability of entry into and persistence in benefit receipt.

The estimates for the monthly model in column (1) of Table 4.4 indicate a strong increase in the degree of state dependence from the standard to the random-coefficients specification. We obtain an average predicted entry rate of 1.3%, a number very similar to the 1.4% reported for the standard model reported in Table 4.1. The average predicted persistence rate of 32.8% however is strikingly larger than the 14.0% found in the standard model. As a consequence, the average partial effect of 31.4 percentage points is more than twice as high as the 12.6 percentage points estimated in the standard model.

This increase in the average partial effect is not surprising. By letting the effects of covariates and unobserved heterogeneity vary with lagged receipt status, we allow for a much more flexible relationship between the lagged dependent variable and the outcome variable. To the extent that observed and unobserved factors affect entry and persistence probabilities differently, the

Table 4.4: *Time aggregation in the standard DRCP model: monthly to annual*

	(1)	(2)	(3)	(4)
	Monthly model	Annual model	Monthly-to-annual time aggregation	Simulation-based specification check
State dependence				
Average predicted pers. rate (in %)	32.8 (0.9)	12.6 (1.0)	3.0 (0.1)	3.2 (0.2)
Average predicted entry rate (in %)	1.3 (0.0)	2.1 (0.1)	2.8 (0.1)	2.6 (0.1)
Average partial effect (in ppts)	31.4 (0.9)	10.5 (1.1)	0.2 (0.0)	0.5 (0.2)
Unobserved heterogeneity				
σ_{a_1}	0.59 (0.01)	0.65 (0.01)		
σ_{a_0}	0.95 (0.01)	0.79 (0.03)		
τ	0.71 (0.01)	0.70 (0.05)		

Note: The monthly model in column (1) and the annual model in column (2) are the dynamic random-coefficients probit specifications of Equations (4.19)-(4.22) estimated on monthly and annualized data, respectively. Annualized data for model (2) were constructed by keeping the observations for December of each year only. The monthly-to-annual time aggregation results presented in column (3) were obtained by aggregating the transitions of the monthly model to the annual level using the simulation-based approach described in Section 4.3.2. Column (4) gives the results from a dynamic random-coefficients probit model estimated on annualized data simulated from a monthly dynamic random-coefficients probit model. Standard errors in parentheses of columns (1) to (3) were calculated using 10 bootstrap replications; predicted transition rates and standard errors in parentheses of columns (4) were calculated by running the annual model on 10 simulated data sets. Coefficient estimates for the two specifications presented in columns (1) and (2) are reported in Appendix J, Tables J.5 and J.6, respectively. Parameters σ_{a_0} and σ_{a_1} are the standard deviations of a_i^0 and a_i^1, respectively, τ is their correlation.

specification will thus attribute a larger share of observed state dependence in benefit receipt to the lagged dependent variable.

To illustrate the importance of unobserved heterogeneity for social assistance entry and persistence, we report some additional parameters from our DRCP model in the lower part of Table 4.4. As summarized in column (1), unobserved heterogeneity in the monthly model is substantially larger for the process driving entries than for the process driving benefit persistence as implied by σ_{a_0} and σ_{a_1}, the standard deviations of the two individual-specific error components a_i^0 and a_i^1. The estimated correlation between the two error components is given by $\tau = 0.71$, indicating that the two persistent components

of the error term in the monthly model are highly but imperfectly correlated. Intuitively, we may thus think of each individual as having some unobserved ability or preferences a_i^1 related to (avoiding) persistence in social assistance and an ability a_i^0 related to entries to social assistance. Ignoring these differences leads to a downward bias in the estimated degree of state dependence in social assistance benefit receipt.

We find that the differences in estimated state dependence between this model and the standard dynamic random-effects probit model are nearly entirely due to inclusion of the random coefficient. A specification like the one used by Cappellari & Jenkins (2009), which interacts the lagged dependent variable with the covariates but does not include a random coefficient for the lagged dependent variable, gives results that are nearly identical to those from the standard model.[14] This illustrates that the assumption made on the form of unobserved heterogeneity in the standard dynamic random-effects probit model may be overly restrictive.

As for the standard model, we can aggregate the transition matrix for the DRCP model from the monthly to the annual level and compare the results to those obtained directly from a model based on annual data. The benchmark is now the model described by Equations (4.19) and (4.22) estimated only on observations for the month of December of each year.

We find that also the more flexible DRCP model fails the time aggregation test at the monthly level. The results in column (2) of Table 4.4 suggest that there is considerable structural state dependence in the annualized data, with average entry and persistence rates of 2.1% and 12.6%, respectively, and a resulting APE of 10.5 percentage points. By contrast, we again take from column (3) that state dependence in benefit receipt virtually disappears once we aggregate monthly benefit transitions to the annual level. A predicted December-to-December entry rate of 2.8% compares to a persistence rate of 3.0%, which implies an average partial effect of 0.2 percentage points.

[14] We do not report estimates from such a model for the sake of brevity.

The results of column (4) confirm again that it is reasonable to assume that the monthly benefit transitions should retain their probit form when aggregated to the annual level. When estimating the DRCP model at the annual level based on data simulated from the monthly DRCP, we calculate estimated transition rates and an average partial effect close to those obtained by aggregating the results from the monthly model. The annual DRCP model is thus an appropriate benchmark against which to test the monthly specification.

We conclude that the Markov specification remains a poor reflection of the true underlying data-generating process even if we allow for heterogeneity in the processes driving entries into and persistence in benefit receipt. The final subsection of this chapter therefore extends our model to allow for duration and occurrence dependence in social assistance benefit receipt.

4.5.2 A DRCP model with duration and occurrence dependence

We have seen that even a more general Markov model that allows for heterogeneity in the processes for social assistance entries and persistence does not appropriately reflect the dynamics of benefit receipt at the monthly level. Results from a dynamic random-effects probit model with higher-order lags in Section 4.4 moreover suggested that the fit of the monthly DREP model can be improved by accounting for an individual's earlier benefit receipt history. In the final part of our analysis, we therefore abandon the first-order Markov assumption in our DRCP model and allow for more sophisticated ways in which past benefit receipt may impact the likelihood of benefit receipt in the current period. More specifically, we implement some of the theoretical concepts introduced by Heckman & Borjas (1980) and extend our DRCP model to account for duration and occurrence dependence in social assistance benefit receipt.[15]

[15]Heckman & Borjas also suggest that there might be *lagged* duration dependence, *i.e.* a causal link between the probability of benefit receipt and the duration of *previous* benefit spells. Since modelling lagged duration dependence would have further increased the complexity of our specification, we ignore it in our analysis.

A standard assumption made in the literature that uses event-history models to examine benefit dependence is that a recipient's probability of leaving benefits depends on the *length of the ongoing spell*. In the case of unemployment, persistent joblessness might lead to a deterioration of labour market skills and networks over time thereby reducing an individual's probability of finding new employment. In the case of social assistance benefits, a recipient might grow disillusioned or frustrated with increased duration of benefit receipt and reduce efforts to become self-sufficient. Similarly, there might be *duration dependence* outside of benefit receipt if job security or the level of pay rise over time making an individual less likely to enter social assistance as the time spent off benefits rises. All of these are theoretical justifications for negative duration dependence in the probability of exits from social assistance benefit receipt and self-sufficiency, respectively. Note moreover that these effects are *behavioural* and distinct from any additional *composition* effects that may arise as benefit recipients with more favourable labour market characteristics leave benefits more quickly.

Also the *number of previous benefit spells* an individual has had might matter for the risk of future benefit receipt. Intuitively, individuals without a previous social assistance history might worry about a potential stigma from benefit receipt that makes them more reluctant to claim benefits. Persons who previously received benefits might be less susceptible to this type of stigma. There might also be information or time costs to claiming benefits that decline if the individual has submitted a claim before. In such cases, the probability of benefit receipt is positively related to the incidence of a previous benefit spell (or the number of such spells), which is what Heckman & Borjas (1980) refer to as *occurrence dependence*.

There are two main difficulties for an empirical assessment of duration and occurrence dependence in benefit receipt. First, the measurement of precise spell durations and spell numbers requires data on benefit receipt with short

observation intervals, which as discussed are rarely available for research purposes. Second, even in such data, information on an individual's benefit receipt prior to the start of the observation period will be missing. The number of benefit spells counted for an individual in the data will therefore typically not adequately reflect the individual's entire 'social assistance career'.

The administrative data that we use allow us to address both of these issues. Since the data set provides high-quality information on benefit receipt at the monthly level, we can construct exact measures of the number and duration of individuals' benefit spells. Even short spells or temporary departures from social assistance are reflected in the analysis. The large size of the data set moreover permits restricting our sample to individuals whom we observe from the moment they become entitled to claim social assistance benefits on their 18^{th} birthday. This means that we can indeed count the number of spells each individual has had since the beginning of the social assistance career for a sample of sufficient size.

We introduce duration and occurrence dependence in our DRCP model by allowing transition probabilities to depend on a series of dummy variables that describe the duration of the ongoing spell *on* or *off* social assistance and the number of previous spells since the start of an individual's social assistance career. Using earlier notation, we specify the probability of entry into social assistance as

$$\begin{aligned} &P(y_{it} = 1 | y_{it-1} = 0, x_i, d_i^0, d_i^1, o_i, \alpha_i, \lambda_i) = \\ &\Phi(x'_{it-1}\beta^0 + \sum_{k=2}^{12} \phi_k^0 d_{ikt-1}^0 + \sum_{k=1}^{11} \psi_k^0 o_{ikt-1} + \alpha_i^0), \end{aligned} \tag{4.23}$$

and the probability of persistence in social assistance as

$$\begin{aligned} &P(y_{it} = 1 | y_{it-1} = 1, x_i, d_i^0, d_i^1, o_i, \alpha_i, \lambda_i) = \\ &\Phi(\bar{\lambda} + x'_{it-1}\beta^1 + \sum_{k=2}^{12} \phi_k^1 d_{ikt-1}^1 + \sum_{k=1}^{11} \psi_k^1 o_{ikt-1} + \alpha_i^1). \end{aligned} \tag{4.24}$$

The 2×11 dummy variables d^0_{ikt-1} and d^1_{ikt-1} indicate whether, in period *t-1*, the ongoing spell off or on social assistance, respectively, had lasted for exactly k periods. The reference case is the first month of a spell on or off social assistance. We restrict the effect of ongoing spells with a duration of more than eleven months to be constant adding single dummies $(d^0_{i12t-1}, d^1_{i12t-1})$ for durations of twelve months and longer. Similarly, the set of dummy variables o_{ikt-1} indicate whether until period *t-1* the individual had terminated exactly k previous benefit spells, the reference case being no previous benefit spells. Again, we specify separate dummies only for the first ten occurrences of benefit receipt and capture the effects of having eleven or more previous social assistance spells through a single dummy o_{i11t-1}. Note that the processes we model start in period $t = 12$, *i.e.* the month of an individual's 19^{th} birthday. All dummies $(d^0_{ik}, d^1_{ik}, o_{ik})$ can therefore be measured for each individual in our sample from the beginning of the individual's social assistance career.

The specification is very flexible in its treatment of duration and occurrence dependence: The effect of spell durations is allowed to differ for the processes describing entries and persistence being described by two separate sets of dummy variables d^0_{ik} and d^1_{ik} with coefficients ϕ^0_k and ϕ^1_k. The number of previous spells enters through a single set of dummies o_{ik}, however with separate sets of coefficients ψ^0_k and ψ^1_k for the entry and persistence process, respectively. As before, the model allows the effects of observable individual characteristics x_{it-1} and persistent unobserved heterogeneity (α^0_i, α^1_i) to differ between entries and persistence in benefit receipt. The extended DRCP specification is again a generalization of the standard DRCP model and therefore also of the standard DREP model.[16]

[16]We could have again written the extended DRCP model in form of a single equation analogous to Equation (4.19) for the simpler DRCP, where each coefficient in the persistence process would be the sum of the corresponding coefficient for the entry process and the coefficient of an interaction term with the lagged dependent variable. An exception are the coefficients of the duration dummies, as these dummies are by construction coded as being equal to zero if the individual is not in the respective state. For $\phi^0_k = \psi^0_k = \phi^1_k = \psi^1_k = 0 \; \forall \, k$, the extended DRCP model in Equations (4.23)-(4.24) reduces to the standard DRCP model in Equations (4.20)-(4.21).

By allowing for essentially separate processes for entries into and persistence in benefit receipt, and by explicitly modelling duration dependence in benefit receipt, our extended DRCP model transforms into a quite general two-state discrete-time event-history model. One main difference from standard applications of event-history models is that as a way to handle the initial conditions problem, data used for such models typically come from *flow sampling*, that is, a sample from the population of *e.g.* those who enter social assistance receipt in given period. Our modelling framework instead uses data from *population sampling* including both recipients and non-recipients, and we explicitly account for the endogeneity of initial conditions employing the approach proposed by Wooldridge (2005).[17] Our random-coefficients modelling framework can thus be viewed as a synthesis of dynamic discrete-choice modelling and event-history modelling, in essence proposing a dynamic discrete-choice model that nests a discrete-time event-history model. It thus facilitates the application of recent solutions to the initial conditions problem developed for dynamic discrete-choice models to event-history analysis. Like certain event-history models, our framework moreover exploits the availability of data on multiple spells in panel data sets as a source of identifying variation that allows us to distinguish duration dependence and unobserved heterogeneity in each state (Honoré, 1993; Van den Berg, 2001).

A challenge for estimation of the above specification is that the extended DRCP model is likely to suffer from a much more serious endogeneity problem than the Markov specifications discussed earlier. In particular, the unobserved heterogeneity terms α_i^0 and α_i^1 are no longer correlated only with the lagged dependent variable but also with each of the duration and occurrence dummies that are functions of past benefit receipt. To be able to integrate out

[17] Bratsberg, Raaum & Røed (2010) estimate a model for migrants' transitions between employment and non-employment with duration dependence in each state comparable to our DRCP model. They avoid the initial conditions problem by working with a sample of individuals who start with an initial employment spell of three years.

the individual-specific error components for consistent estimation, we therefore need to also extend our set of controls for initial conditions.

As earlier, we specify the unobserved individual-specific effects to be linear functions of an individual's longitudinal averages of all time-varying explanatory variables and the benefit status in the initial period y_{i11}. We now however also add both sets of duration dummies (on and off social assistance) and the occurrence dummies, all measured in the initial period. We thus do not only control for the benefit receipt status in the initial period but also for how long the individual had been in this state at that point in time and for how many previous spells the individual has had.

The resulting expression for the individual-specific effects α_i^0 and α_i^1 can be written as

$$\alpha_i^j = \gamma_0^j + \gamma_1^j y_{i11} + \bar{x}_i'\gamma_2^j + \sum_{k=2}^{12}\gamma_{3,k}^j d_{ik11}^0 + \sum_{k=2}^{12}\gamma_{4,k}^j d_{ik11}^1 + \sum_{k=1}^{5}\gamma_{5,k}^j o_{ik11} + a_i^j \quad (4.25)$$

for $j = 0, 1$.[18] Note that depending on whether the individual is on or off social assistance in the initial period y_{i11}, either of the two sets of duration variables in the initial period d_{i11}^0 and d_{i11}^1 will be all zero. We again require the remaining unobserved heterogeneity terms a_i^0 and a_i^1 to follow a bivariate normal distribution with zero means, variances $(\sigma_{a_0}^2, \sigma_{a_1}^2)$ and a correlation τ. Under these distributional assumptions, we can again integrate out (a_i^0, a_i^1) to estimate the model parameters on monthly data using conditional maximum likelihood methods. For a further discussion of this 'extended' Wooldridge approach see Appendix H.[19]

We start by presenting APEs of spell durations and occurrences on the probability of benefit receipt to illustrate the importance of duration and oc-

[18] The index k of the sum over the occurrence dummies for the initial spell only runs up to five as this is the maximum number of previous spells that can be observed in the data in the twelve months leading up to the initial period.

[19] An alternative approach to the initial conditions problem would have been to estimate the model over the entire period $t = 1, ..., T_i$ rather than $t = 12, ..., T_i$, and to assume that we truly observe the start of the process we are modelling on each individual's 18^{th} birthday by setting $y_{i0} = 0 \; \forall \; i$. We believe that the approach we use is preferable, because it does not impose exogeneity of the initial conditions.

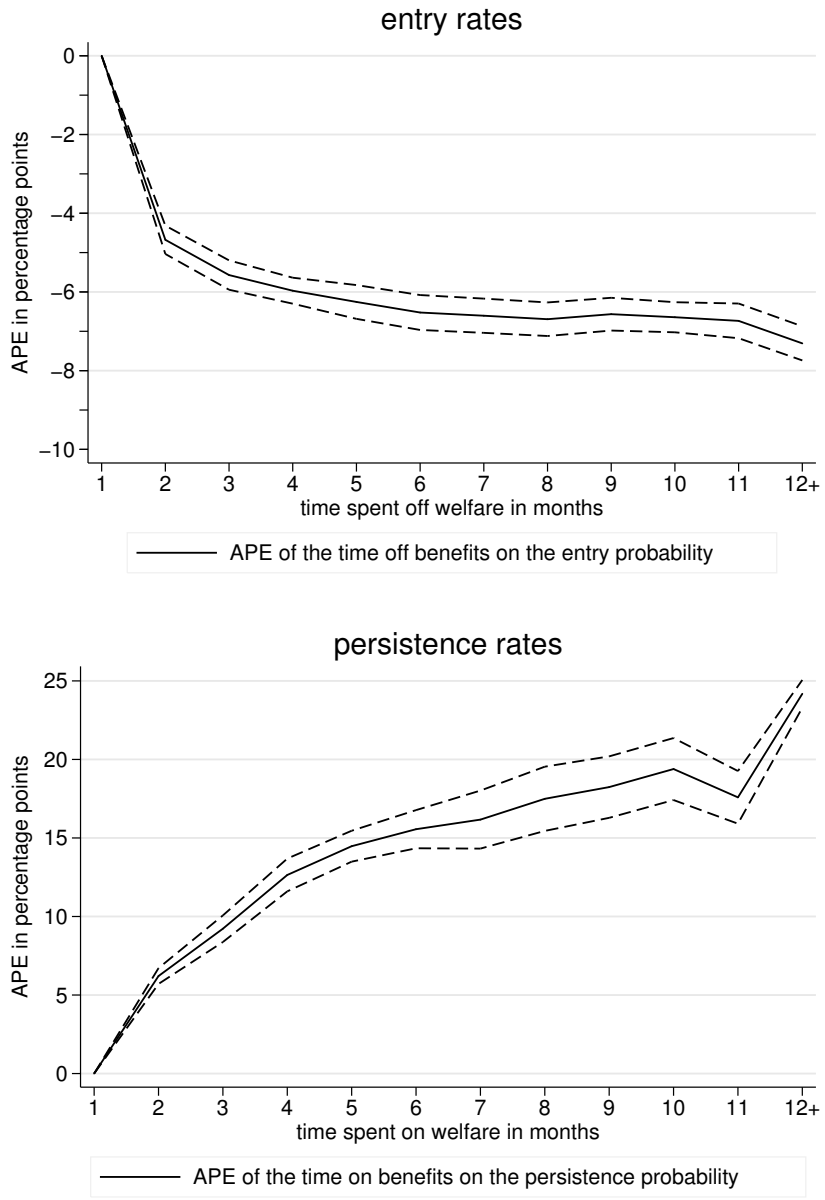
currence dependence in social assistance benefit receipt dynamics. Tables I.4 and I.5 in Appendix I give an overview of the distributions of spell lengths and spell numbers in the sample. Figure 4.3 plots the average partial effect of the time spent on and off social assistance on the probability of social assistance persistence and social assistance entry, respectively. We obtain these APEs by predicting the probability of benefit receipt in the next month while varying the time on or off benefits for each individual for the respective values of the lagged receipt status. APEs are expressed in reference to a spell in its initial month either on or off social assistance. All other characteristics of the individual, including the occurrence variables, are kept fixed at their observed values.[20]

We find substantial duration dependence both on and off social assistance. In the upper panel, we give the average partial effect of the duration off benefits on the rate of social assistance entry. We find a strongly declining pattern from the first to the second period of benefit receipt. The probability of entering benefits is about 5 percentage points lower for an individual who has been off benefits for two periods compared to the initial period off benefits. The APE becomes more negative as the time off benefits rises, however at a strongly declining rate approaching a negative 7 percentage points for spells of length 6 months or longer. An individual who has not received benefits for the last 12 months is thus, *ceteris paribus*, about 7-8 percentage points less likely to enter benefits the next month than if he were in his first month off benefits.

The lower panel shows very strong duration dependence also for persistence in benefit receipt. Individuals in the second month of their social assistance spell are about 5 percentage points more likely to stay on benefits than if they were in their initial month. The effect further increases strongly with additional time on benefits reaching 15 percentage points or more for benefit spells of duration 5 months or longer.

[20]Since occurrence variables are defined as the number of completed social assistance spells, neither being on or off social assistance is inconsistent with any value for the occurrence variable.

Figure 4.3: *Duration dependence in entry and persistence rates*



Note: Average partial effects have been calculated based on the coefficient estimates reported in Appendix J, Table J.7. An individual's number of previous benefit spells and all covariates are kept fixed in the calculations. APEs are calculated with spell durations of one month on / off benefits as the reference category. The dashed lines give 95% confidence intervals obtained by bootstrapping with 10 replications.

We produce similar calculations for the set of occurrence dummies included in our model and present the average partial effects of previous spell occurrences on social assistance entry and persistence rates in Figure 4.4. Again, these APEs are obtained by varying only the number of previous benefit spells an individual has had and the lagged benefit receipt status while keeping the individual's current spell duration on or off benefits, respectively, fixed at the observed value.

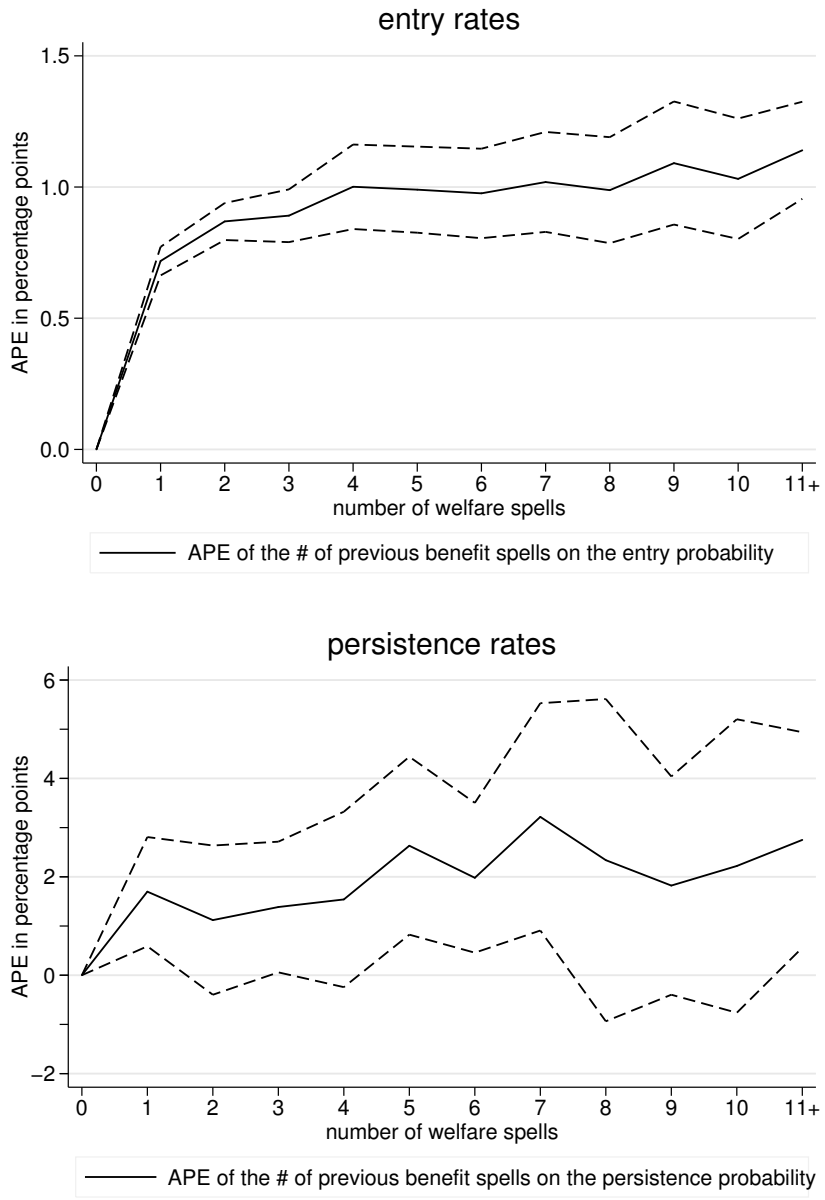
Results suggest that a history of previous social assistance benefit receipt substantially increases the likelihood of entry into benefits, but that the *number* of previous benefit spells matters little beyond the first spell. As shown in the upper panel of Figure 4.4, we find a significantly positive average partial effect of having had previous spells in benefit receipt; the size of this effect is relatively stable across spell numbers at around 0.8-1 percentage points.

We do not find a comparable effect of the number of previous benefit spells on the probability of persistence in benefit receipt. While we calculate a mildly positive average partial effect of previous benefit spells on benefit persistence, five of the eleven occurrence dummies reported in Table J.7 are not significant at the 5% level. APEs plotted in the lower panel of Figure 4.4 give a similar result.

Overall, the findings of substantial duration dependence in the processes leading up to entries and persistence in benefit receipt and of occurrence dependence in entry rates into benefit receipt suggest that the extended DRCP model might indeed provide a substantial improvement over our simpler specifications. The pattern of the occurrence dependence we find may be consistent with a theory of stigma or information costs: Experiencing benefit receipt today leads to a permanent rise in the likelihood of re-entry into benefits in the future, but any later spells do not further raise the benefit entry rate.

As earlier, we use the model estimates to predict social assistance entry and persistence rates for each individual in the sample and calculate the degree of

Figure 4.4: *Occurrence dependence in entry and persistence rates*



Note: Average partial effects have been calculated based on the coefficient estimates reported in Table J.7. For the calculation of the APEs, an individual's benefit receipt status, the duration of the ongoing spell and all covariates are kept fixed in the calculations. APEs are calculated using the case of no previous benefit receipt as the reference category. The dashed lines give 95% confidence intervals with standard errors obtained by bootstrapping with 10 replications.

estimated state dependence in benefit receipt. Again, we allocate to each individual in the sample a counterfactual benefit receipt status, which now however comes with its set of duration and occurrence dummies. As we have seen, the effect of spell durations and occurrences differs by benefit receipt status. The level of estimated state dependence therefore depends on the assumptions we make about how long each individual has been on or off benefits and on how many previous benefit spells she has had.

For our calculations, we work with the assumption that individuals are as close as possible to a 'clean' benefit receipt history. Average predicted entry rates are constructed under the assumption that an individual had no previous benefit receipt and spent the maximum duration off benefits in the current spell. Similarly, average predicted persistence rates are calculated assuming that the individual entered social assistance in the last period and has no previous benefit receipt history. The assumptions we make are thus favourable to finding both low predicted entry and persistence rates. All covariates are again kept fixed at their observed values.

The degree of estimated state dependence in monthly benefit receipt calculated under these assumptions rises further compared to the standard DRCP model without duration and occurrence dependence. The average predicted rate of entry into benefits drops from 1.3% to 0.3% as shown in column (1) of Table 4.5. This likely reflects the favourable assumptions of no previous benefit spells and a maximum duration off benefits made when calculating predicted entry rates. The persistence rate rises from 32.8% to 37.3%. The resulting APE of benefit receipt in the previous period on the likelihood of benefit receipt in the current period rises from 31.4 percentage points in the simple DRCP to 37.0 percentage points in the extended DRCP with duration and occurrence dependence.

The fit of the model is substantially improved by the inclusion of duration and occurrence dummies. This is illustrated by a substantial decline in the estimated degree of unobserved heterogeneity in the processes driving entries

and exits implied by the parameter estimates reported in the bottom part of of Table 4.5. The estimated standard deviations of the two individual-specific error components $\hat{\sigma}_{a_0}$ and $\hat{\sigma}_{a_1}$ decline from 0.95 to 0.35 and from 0.59 to 0.32, respectively. The estimated correlation between these two error components rises slightly from 0.71 to 0.78.

Compared to the predicted transition rates presented in Table 4.5, the magnitude of estimated duration and occurrence dependence is substantial. While the average predicted entry rate for individuals with the maximum duration off benefits is only 0.3%, it is about 7 percentage points higher for individuals in their first month off benefits as shown in Figure 4.3. Also the seemingly modest occurrence dependence effect of around 0.8 percentage points after a single previous spell shown in Figure 4.4 is sizeable if we consider that it permanently raises an individual's predicted entry rate into benefits from 0.3% to 1.1%. For individuals on benefits, the 20 percentage-point drop in exit rates after ten months of benefit receipt implied by Figure 4.3 compares to an average predicted exit rate from benefits of 37% for an individual in the first month of the benefit spell.[21]

A consequence of the significant duration and occurrence dependence estimated by the extended DRCP model at the monthly level is finally that unlike for all previous specifications, some state dependence persists if we use the estimation results to simulate benefit transitions over an entire year. In column (2) of Table 4.5, we report December-to-December transition probabilities obtained from aggregating up the monthly transition probabilities to the annual level:[22] The likelihood of benefit receipt today is 6.3% in the case of benefit receipt 12 months ago compared to 1.3% conditional on non-receipt 12 months

[21]Note that a comparison of APEs for the occurrence and duration dummies with average predicted transition rates is only approximate: When calculating the APEs of duration dummies, we kept the values of all occurrence dummies fixed *at their observed values* (and vice versa). By contrast, when predicting entry and persistence rates, we assume a 'clean' benefit receipt history. The two calculations thus do not make identical assumptions on an individual's benefit receipt history, an aspect that might have an impact on effect sizes due to the non-linearity of the model.

[22]Column (2) of Table 4.5 is thus the analogue to column (3) of Tables 4.1-4.4.

Table 4.5: *Time aggregation in the extended DRCP model: monthly to annual*

	(1)	(2)
	Monthly model	Monthly-to-annual time aggregations
State dependence		
Average predicted persistence rate (in %)	37.3 (0.5)	6.3 (0.2)
Average predicted entry rate (in %)	0.3 (0.0)	1.3 (0.1)
Average partial effect (in ppts)	37.0 (0.5)	5.0 (0.2)
Unobserved heterogeneity		
σ_{a_1}	0.32 (0.01)	
σ_{a_0}	0.35 (0.01)	
τ	0.78 (0.03)	

Note: The monthly model in column (1) refers to the dynamic random-coefficients probit specifications in Equations (4.23)-(4.25) with occurrence and duration dependence estimated on monthly data. The monthly-to-annual time aggregation results presented in column (2) were obtained by aggregating the transitions of the monthly model to the annual level using the approach described in Section 4.3.2. Standard errors in parentheses of columns (1) to (3) were calculated using 10 bootstrap replications. Coefficient estimates for the two specifications presented in columns (1) and (2) are reported in Appendix J, Table J.7. Parameters σ_{a_0} and σ_{a_1} are the standard deviations of a_i^0 and a_i^1, respectively, while τ is their correlation.

ago. The average partial effect of benefit receipt 12 months ago on benefit receipt today is hence estimated at 5.0 percentage points.

To evaluate the time aggregation properties of our model, we would finally again like to test how these aggregated results from our extended DRCP model of monthly benefit dynamics compare to a corresponding model estimated on annualized data. The appropriate benchmark in this case is an annual DRCP specification that now moreover includes the complete vector of duration and occurrence variables measured at the monthly level. As before, such an annual model exhibits the conditional Markov property because the Markov assumption has been made for the corresponding monthly model.

Consistent estimation of such a model however is not trivial. The endogenous duration and occurrence variables are no longer deterministic functions of the current and past values of the dependent variable, because the latter is now measured at the annual level. Even the extended Wooldridge approach proposed for the monthly model in Equation (4.25) will therefore no longer give

consistent estimates at the annual level (see the discussion in Appendix H).[23] The evaluation procedure used previously for the simpler Markov models therefore does not readily extend to a specification with duration and occurrence variables.

We propose an alternative evaluation method as a specification check for our extended DRCP model. The test builds on the idea that if the model does well at describing the true benefit receipt dynamics at the monthly level, benefit receipt sequences simulated from the model should be 'similar' to those produced by the true data-generating process. The required 'similarity' of the simulated and the true data must hold for all characteristics of the two data sets. This implies that we should expect any other model – also one that is misspecified – to give similar results when estimated on the two data sets provided that the model used to construct the simulated data is well-specified. Larger disparities in results between the two by contrast should be seen as evidence that the model does not well at replicating the true benefit dynamics.

Our specification test thus consists of running a different model on both the simulated and the true data and to compare the results. Specifically, we use the simpler standard annual DRCP model discussed in Section 4.5.1 for this exercise. This allows us to produce results comparable to those obtained in the time aggregation tests carried out in earlier parts of our analysis. We start by simulating benefit receipt dynamics for each individual over a 12-month period based on the estimates from the extended monthly DRCP model. This step is identical to the one employed in the simulation-based specification checks for the simpler models. We then estimate the standard annual DRCP model on the annualized simulated data. The results are compared to those we got from running the standard DRCP on the annualized true data as presented in Table 4.4. The results of the specification test are presented in Table 4.6.

[23] We have estimated annual models that include monthly measures of spell durations and occurrences with simulated data and find that they give heavily-biased predictions. This confirms that the inconsistency is not merely a theoretical concern.

Table 4.6: *Specification tests of the extended monthly DRCP model on annual data*

	Standard annual DRCP model	Simulation-based specification check	
	(1) Annualized true data	(2) Annualized data from extended monthly DRCP	(3) Annualized data from standard monthly DRCP
Average predicted persistence rate (in %)	12.6 (1.0)	6.4 (0.6)	3.2 (0.2)
Average predicted entry rate (in %)	2.1 (0.1)	2.2 (0.1)	2.6 (0.1)
Average partial effect (in ppts)	10.5 (1.1)	4.1 (0.6)	0.5 (0.2)

Note: The annual model in column (1) refers to the 'standard' dynamic random-coefficients probit (DRCP) specifications in Equations (4.19)-(4.22) estimated on annualized data (identical to column (2) of Table 4.4). The estimates presented in column (2) were obtained by estimating the standard DRCP specification in Equations (4.19)-(4.22) using annualized data that were simulated based on the extended monthly DRCP model in Equations (4.23)-(4.25). The estimates presented in column (3) were obtained by estimating the 'standard' DRCP specification in Equations (4.19)-(4.22) using annualized data that were simulated based on the 'standard' monthly DRCP model (identical to column (4) of Table 4.4). Standard errors in parentheses of column (1) were calculated using 10 bootstrap replications; predicted transition rates and standard errors in parentheses of columns (2)-(3) were calculated based on running the annual model on 10 simulated data sets.

We find that also our extended monthly DRCP model only partially succeeds at predicting the monthly social assistance benefit dynamics in Norway. As seen earlier, the simple annual DRCP model estimated on annualized true data predicts an average partial effect of benefit receipt 12 months ago of 10.5 percentage points (column (1), reproduced from Table 4.4)). Estimating the same model on data simulated from the extended monthly DRCP model, we obtain average predicted persistence rates that are 60% lower (column (2)). For an essentially unchanged average predicted entry rate, this implies a decline in the APE to only 4.1 percentage points. Note that as emphasized earlier, we are not very much interested here in the estimated level of state dependence *per se*, because we already know that the simple annual DRCP model is misspecified. The difference between the levels of state dependence estimated from this model when running it on true data and on data simulated from our ex-

tended monthly DRCP model however indicates that also our most complex model does not do very well at reproducing the true month-to-month benefit dynamics.

The extended monthly DRCP model does however provide a substantial improvement over the simpler standard monthly DRCP model without duration and occurrence dependence. Column (3) presents results obtained from estimating the standard annual DRCP model on data simulated from the standard monthly DRCP model. The average predicted persistence rate is lower still, making the estimated APE drop to 0.5 percentage points. While our preferred extended monthly DRCP model thus does not succeed at replicating the same level of estimated state dependence that is observed in the true data, it gets much closer to the benchmark result than the simpler standard monthly DRCP model.

4.6 Conclusion

Dynamic discrete-choice models are an important tool for the analysis of labour market dynamics and in particular the study of state dependence in benefit receipt. One central assumption of such models is that sequences of benefit receipt follow a Markov process: Conditional on the observed and unobserved characteristics of an individual the first lag of the dependent variable is sufficient for predicting the outcome. Such models are usually estimated on data with annual observations from administrative records or household surveys due to a lack of data with shorter observation intervals. The main parameter of interest is the average partial effect of the lagged dependent variable, which is interpreted as capturing structural state dependence in benefit receipt.

In this chapter, we showed that a standard Markov model gives highly inconsistent estimates of the degree of structural state dependence in social assistance benefit dynamics in Norway. Based on monthly administrative data over the 16-year period from 1993-2008, we estimated the same specification at

the monthly, annual and biennial level and used the results to predict individuals' implied period-to-period transition matrices. Due to the Markov property of the model, these transition matrices can be aggregated up over time and thus be compared across specifications.

Results indicate that the magnitude of estimated state dependence rises strongly with the level of time aggregation. The annual model grossly overestimates the degree of state dependence predicted by a corresponding model at the monthly level; state dependence estimated by a biennial model is even higher. This finding is robust to employing the 'benefit year' approach rather than the 'point-in-time' approach for defining the annual benefit receipt variable.

We conclude that the standard dynamic random-effects probit model is misspecified and not suited for an analysis of structural state dependence in social assistance benefit receipt in Norway. Our results might moreover be evidence that, more generally, the Markov assumption commonly made in studies of labour market dynamics is too crude a simplification. Estimates of structural state dependence derived from such models are likely to be driven by the choice of the observation interval. The results from such analyses should therefore be interpreted with caution.

In the second part of the chapter, we illustrated that benefit dynamics in Norway are indeed much more complex than it is commonly allowed for in econometric modelling. We presented a generalization of the standard model in the form of a more flexible dynamic random-coefficients probit (DRCP) model. This specification maintains the Markov assumption but allows the processes driving entries into benefits and persistence in benefit receipt to differ with an individual's observable and unobservable characteristics. We found that an individual's two individual-specific error terms for the social assistance entry and persistence process are positively but imperfectly correlated. Allowing for this heterogeneity leads to a substantial increase in the level of estimated state dependence at the monthly level. Yet, month-to-month state dependence remains low enough to vanish in the time aggregation process to the annual

level; a comparable annual model again gives a substantially higher level of state dependence.

The fit of the model could be substantially improved by abandoning the Markov assumption. Further extending the DRCP model, we allowed for duration and occurrence dependence in benefit receipt and indeed found sizeable effects of the current spell length and the number of previous spells on the probability of benefit receipt. While there is substantial duration dependence in both entries and persistence in benefit receipt, the number of previous benefit spells only affects entry rates. The average predicted month-to-month persistence rate for first-time entrants is 37 percentage points higher than the average predicted entry rate for individuals who have never received benefits. Aggregated to the annual level, this corresponds to an average partial effect of 5 percentage points. A simple simulation-based specification test shows that benefit receipt sequences produced by this model are still clearly distinguishable from those of the true data-generating process. The model however represents a notable improvement over the simpler DRCP model without duration and occurrence dependence.

Our findings suggest that much more complex models of benefit receipt dynamics may be needed for reliable estimates of state dependence in benefit receipt. We have shown that in Norwegian social assistance benefit dynamics 'state dependence' arises from a combination of the simple intercept shift induced by the standard lagged dependent variable, disparities in the impact of observable and unobservable characteristics on entry and persistence probabilities, and the effect of time spent in the current state on the probability of future benefit receipt. Future work will have to seek ways of better describing these different dynamics possibly by combining and extending some of the approaches we have used. The existence of duration and occurrence dependence however implies that such work will likely have to be based on data with comparably short observation intervals that permit identification of individuals' benefit spells.

A methodological contribution of this chapter is finally that it offers an intuitive and simple test of the validity of the Markov assumption for cases where no data with short observation intervals are available. By aggregating up the predicted transition matrices obtained from first-order Markov models for instance at the annual level and comparing them to the predictions of the corresponding benchmark model at the biennial level, authors will be able to quickly evaluate the reliability of their estimates of the level of structural state dependence. This specification test shall be useful for assessing the robustness of the results obtained from simple models of social assistance benefit receipt dynamics or labour-market-state dynamics more broadly.

Chapter 5

Conclusions

This dissertation presented an analysis of the dynamics of social assistance benefit receipt in a selection of European countries.

In Chapter 2, I studied state dependence in benefit receipt in Germany over the years 1995 to 2011. I employed the standard approach used in most of the existing work on social assistance dynamics estimating a dynamic random-effects probit model on annual data from the German Socio-Economic Panel (SOEP). Most of the state dependence found in the raw data is due to observed and unobserved individual heterogeneity and therefore spurious. There remains a significant structural component, with benefit receipt at the time of the last interview being associated with a 13 percentage-point increase in the likelihood of benefit receipt at the current interview. This finding is in line with results reported in comparable earlier studies for other countries. It can also be reconciled with earlier diverging findings by Wunder & Riphahn (2014) and Riphahn & Wunder (2013), who use data from the same source but a slightly different methodology. I did not find evidence of substantial time variation in state dependence over the observation period, in particular around the 2005 'Hartz' reforms.

In Chapter 3, I exploited separate administrative data sets for four European countries – Luxembourg, the Netherlands, Norway and Sweden – to describe patterns of social assistance receipt at the monthly level. Benefit dynamics differ strongly across countries: In Norway and Sweden, benefit spells

tend to be very short but most recipients have several spells. In Luxembourg and the Netherlands, one-third and one-quarter of spells, respectively, last two years or longer, but individuals are much less likely to return to benefit receipt after leaving. Unsurprisingly, these short-term dynamics are partly lost as data are aggregated to the annual level. The higher turnover in benefit receipt in Norway and Sweden is reflected in much higher aggregate annual exit rates; annualized benefit receipt rates in the two countries are however much higher than the share of individuals receiving benefits at a given point in time. Especially in countries where turnover in benefit receipt is high, annual data overestimate the degree of persistence in benefit receipt.

In Chapter 4, which is joint work with Manudeep Bhuller and Christian Brinch, I presented evidence for a possible misspecification of the standard first-order Markov models used in much of the existing literature on social assistance dynamics (including in Chapter 2). Estimates of structural state dependence in Norway derived from annual data are substantially higher than those obtained from running the same model on monthly data. When aggregated to the biennial level, estimates from annual models imply again lower levels of state dependence than those estimated directly from a biennial model. This indicates that the first-order Markov assumption is an oversimplification of benefit dynamics in Norway, and that estimates of state dependence derived from first-order Markov models may more generally be unreliable. Allowing for richer types of serial persistence in monthly benefit receipt, we found evidence of substantial duration dependence both in and off benefits and of occurrence dependence for benefit entries.

The heterogeneity in benefit receipt dynamics observed across the countries studied in Chapter 3 points to the fact that social assistance systems may have different functions. In the two Nordic countries, where spell durations are short but repeat receipt is frequent, social assistance appears to play the intended role of providing short-term emergency assistance. In Luxembourg and the Netherlands, a substantial share of recipients rely on social assistance as a

means of long-term income-support as indicated by the much larger fraction of recipients who remain on benefits for prolonged periods. This may be problematic not only because of the resulting high expenditures for benefit payments; the large shares of long-term benefit recipients also imply that many individuals may have little perspective of becoming self-sufficient, which may lead to frustration and disengagement.

Based alone on administrative data, I am not able to tell to what extent the different receipt patterns described in Chapter 3 are driven by the features of the social assistance systems. While long spell durations in Luxembourg and the Netherlands may indicate that recipients lack sufficient incentives to seek employment, they could also reflect the composition of the recipient population and the role of social assistance in the broader social security system. As discussed, more frequent long-term benefit receipt in Luxembourg and the Netherlands may for instance be due to potentially higher shares of single parents or individuals with health problems among recipients; in Norway, these less employable recipients would typically receive other types of income-support benefits. While the administrative data used in Chapter 3 are suited exceptionally well for the presented analysis of social assistance spell durations, additional information on the composition of the recipient population and on the importance of alternative income-support programmes would facilitate a more comprehensive interpretation of the results. Such information could for instance be obtained from household surveys, which tend to provide much richer data on individual and household characteristics.

A consequence of the findings from Chapter 3 is that benefit dynamics may be much more complex than it is typically accounted for in empirical work on state dependence. Results for Norway presented in Chapter 4 indicate that short spell durations and frequent 'cycling' into and out of benefits reflect complicated patterns of serial persistence in benefit receipt. There appears to be substantial duration dependence in entry and persistence rates. This would underline that measures to support or activate benefit recipients should

ideally be implemented early-on in a benefit spell when exits from benefits are easiest. Since moreover a previous benefit history seems to permanently raise the likelihood of re-entry into benefits, there indeed appear to be long-term 'scarring effects' of benefit receipt. An open question remains whether the magnitude of such occurrence dependence effects depends on the length of the previous spell, a possibility that our model did not account for.

The implications of these results for analyses of state dependence at the annual level are not entirely obvious. The time aggregation exercise of Chapter 4 indicates that the standard annual first-order Markov model is seriously misspecified in the case of Norway; even the results obtained when estimating such a model at the monthly level were not particularly insightful. Estimates of state dependence derived from such models may thus be generally unreliable and should be interpreted with care. However, also the most complex monthly model of benefit dynamics indicates that there is state dependence in benefit receipt. The 5 percentage-point average partial effect obtained when aggregating the transitions of this model to the annual level (Chapter 4, Table 4.5) is moreover strikingly close to the 4.9 percentage points estimated with the simplest first-order Markov model at the annual level (Chapter 4, Table 4.1). Strictly speaking, the two numbers are not directly comparable because the former has been calculated under the counterfactual assumption of a clean benefit receipt history for all individuals. Still, the finding that both models yield similar levels of estimated structural state dependence may indicate that results obtained from an annual first-order Markov model are not completely off as long as benefit receipt is measured at a single point in time each year (as done in Chapter 2).[1]

[1]Chapter 4 showed that the level of estimated state dependence can be highly sensitive to the method used for defining the yearly benefit variable. A 'benefit year' approach leads to much stronger estimated state dependence than if benefit receipt is measured at one point in time. The average partial effect from a model based on the 'benefit year' definition is therefore also no longer comparable in size to that obtained from the monthly model with duration and occurrence dependence. While models that use the 'benefit year' and the 'point-in-time' approach may be interpreted as measuring different 'types' of state dependence, the implications of this distinction for the measurement of state dependence have not been

A simple simulation exercise based on the estimation results presented in Chapter 2 indicates that the first-order Markov assumption is an oversimplification also in the case of Germany. Benefit transition matrices predicted from the coefficient estimates of the standard model (presented in Chapter 2, Table 2.2) do not aggregate up from the annual to the biennial level as one should expect if the first-order Markov assumption were true. The 13 percentage-point average partial effect calculated at the annual level (Chapter 2, Table 2.3) shrinks to 3.5 percentage points when aggregated to the biennial level; estimation results obtained directly from a biennial model still give an average partial effect of 12 percentage points (*not shown*). These calculations may suggest that the annual model presented in Chapter 2 still underestimates true state dependence, for instance by not correctly capturing some form of occurrence dependence that is picked up by the biennial model. Yet, the results from Chapter 4 suggest that the level of state dependence from the annual model may not be too far from the result I would have obtained by estimating a much more complex model at the monthly level. Without reliable information on the underlying monthly benefit dynamics, it is therefore difficult to give the above calculations a meaningful interpretation or even to evaluate what they imply for robustness of the results presented in Chapter 2.

Irrespective of this, the findings from Chapters 3 and 4 illustrate that it is difficult to give estimates of annual first-order Markov models a clear interpretation. The results derived from the most complex monthly model for Norway indicate that benefit receipt today has a measurable impact on the likelihood of benefit receipt in 12 months. The large majority of spells in Norway however are short, with only about one out of twenty spells lasting twelve months or longer. The significant average partial effect in a first-order Markov model at the annual level would therefore have to be understood as capturing occurrence dependence in benefit receipt, possibly combined with a duration dependence effect over the few spells that last for an entire year. In Luxembourg or the

explicitly discussed.

Netherlands, where 40% to 60% of spells last for a year or longer, the first-order Markov assumption may be easier to interpret, because in the raw data, benefit receipt twelve months ago is more strongly associated with benefit receipt in the current period.

Even when estimated on monthly data, dynamic discrete-choice models are not very well suited to provide evidence on the potential drivers of state dependence in social assistance receipt. The duration dependence in and off benefits measured in Chapter 4 suggests that part of the structural effect of past benefit receipt might arise gradually during the spell. This is consistent for instance with the hypothesis that benefit receipt induces a loss of motivation or self-control with the recipient and hence reduces job-search efforts with increased spell duration. At the same time, occurrence dependence may hint at a one-time drop in information costs or the emergence of a stigma effect that arises upon an individual's first entry into benefits.

Measured state dependence in benefit receipt may however also reflect persistence in labour market and income dynamics more broadly. Contini & Negri (2007) point out that spells of benefit receipt tend to coincide with periods of joblessness and low income. Even in the absence of a direct effect of benefit receipt, structural state dependence in unemployment or poverty may therefore drive a wedge between social assistance persistence and entry rates. Observed state dependence in social assistance may thus not be due to any detrimental effects of benefit receipt as such but rather be driven for instance by the loss of human capital suffered by benefit recipients during periods of unemployment. The policy conclusions in the two cases would differ.

The preferred solution for identifying channels through which structural state dependence arises would clearly be the use of (quasi-)experimental methods that are suited to identify causal effects. One rare example of such an approach is a recent study by Kroft, Lange & Notowidigdo (2013), who look for stigma effects as a potential source of duration dependence in unemployment. By sending out fake CVs to job postings in the United States, they

show that otherwise-comparable applicants with longer reported durations in unemployment have lower chances of being invited for an interview. Employers thus indeed appear to use the length of an ongoing unemployment spell as a signal about productivity when screening applicants.

In the absence of such studies, more complex models of labour market transitions may help shed further light on the drivers of state dependence. One approach would be to maintain the binary framework but control for the employment status in a model of benefit dynamics. Identification would rest on those unemployed individuals who do not receive benefits and on those benefit recipients who work. Administrative data sets like the *FD-Trygd* provide a sample of sufficient size to disentangle the effects of benefit receipt and unemployment. The major difficulty in such models arises from the need to account for feedback effects between benefit receipt and employment that might make such models intractable (see the related discussion by Biewen (2009)). The more suitable alternative may therefore be the estimation of dynamic multinomial choice models. As shown by Riphahn & Wunder, such models can provide information on the sources of state dependence in benefit receipt, for instance by explicitly comparing exit rates to work of benefit recipients and inactive non-recipients. To address the critique of Contini & Negri (2007), this approach would have to be extended to cover a larger number of competing states. Availability of high-quality administrative data with large sample sizes and increases in computational power may however mean that such an analysis will soon be possible even for long monthly panels.

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Appendices

Appendix A

Variable definitions for Chapter 2

Table A.1: *Variable definitions for Chapter 2*

Variable name	Definition
benefit receipt variables	
SA_t	dummy variable =1 if in year t, the individual lived in a household in which *any single household member* reported social assistance benefit receipt at the time of the interview, =0 if no household member reported social assistance benefit receipt; for a description which benefit programmes are considered for defining the variable, see Table 2.1
SA_{t-1}	dummy variable =1 if in year $t-1$, the individual lived in a household in which *any single household member* reported social assistance benefit receipt at the time of the interview, =0 if no household member reported social assistance benefit receipt
SA_0	dummy variable =1 if the individual, in the year when first observed, lived in a household in which *any single household member* reported social assistance benefit receipt at the time of the interview, =0 if no household member reported social assistance benefit receipt;
individual characteristics	
female	dummy variable =1 if the individual is female, =0 otherwise
migrant	dummy variable =1 if the individual is first- or second-generation migrant, =0 otherwise
age	individual's age in years
age^2	individual's squared age in years (divided by 100)
years of education	individual's completed years of schooling
years of education^2	individual's squared completed years of schooling
good health	dummy variable =1 if the individual's self-assessed health is "good" or "very good", =0 otherwise
poor health	dummy variable =1 if the individual's self-assessed health is "poor" or "bad", =0 otherwise
household characteristics	
single, no children	dummy variable =1 if the individual is not partnered and lives in a household without children (< 18 years), =0 otherwise
single, with children	dummy variable =1 if the individual is not partnered and lives in a household with children (< 18 years), =0 otherwise
couple, no children	dummy variable =1 if the individual is partnered and lives in a household without children (< 18 years), =0 otherwise
couple, with children	dummy variable =1 if the individual is partnered and lives in a household with children (< 18 years), =0 otherwise

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Table A.1 – continued from previous page –

Variable name	Definition
child $\leq$ 6 years	dummy variable =1 if the individual lives in a household with at least one child aged $\leq$ 6 years, =0 otherwise
household size	number of individuals living in the household
partner characteristics	
migrant	dummy variable =1 if the individual's partner is first- or second-generation migrant, =0 otherwise or if the individual is not partnered
age	age in years of the individual's partner, =0 if the individual is not partnered
age^2	squared age in years (divided by 100) of the individual's partner, =0 if the individual is not partnered
years of education	completed years of schooling of the individual's partner, =0 if the individual is not partnered
years of education^2	squared completed years of schooling of the individual's partner, =0 if the individual is not partnered
good health	dummy variable =1 if the self-assessed health of the individual's partner is "good" or "very good", =0 otherwise or if the individual is not partnered
poor health	dummy variable =1 if the self-assessed health of the individual's partner is "poor" or "bad", =0 otherwise or if the individual is not partnered
time dummies	
1997, ..., 2011	dummy variables =1 in the respective calendar year, =0 otherwise
H_t	dummy variable =1 in the 'post-Hartz' calendar years 2005-11, =0 otherwise
reg. unemployment rate	yearly unemployment rate in the individual's state of residence
Eastern Germany	dummy variable =1 if the individual lived in one of the Eastern German states (excl. Berlin) when first observed in the sample

Note: All variables are measured at the time of the interview in the respective year. An exception is the 'Eastern Germany' dummy, which is defined as the state of residence at the time of the interview in the year when the individual is first observed (see justification in the main text).
The benefit variables SA_t, SA_{t-1} and SA_0 are constructed at the household level taking into account receipt of social assistance benefits *by any member of the household* at the time of the interview. All other variables are defined *at the individual level*; 'household characteristics' are, however, by construction identical for two cohabiting partners. The 'partner characteristics' of a partnered individual correspond to the 'individual characteristics' of the partner (and vice versa).

Appendix B

Descriptive statistics for Chapter 2

Table B.1: *Sample size and attrition*

Year	Sample Size		Sample entries and exits (# of individuals)	
	Individuals	Households	Entries	Exits
1995	5,164	3,046	–	0
1996	6,063	3,575	899	920
1997	5,607	3,331	464	882
1998	5,835	3,505	1,110	609
1999	5,573	3,375	347	726
2000	9,419	5,752	4,572	631
2001	9,332	5,684	544	1,331
2002	9,416	5,734	1,415	991
2003	8,856	5,429	431	1,016
2004	8,184	5,079	344	890
2005	7,624	4,791	330	854
2006	7,924	5,051	1,154	811
2007	7,462	4,783	349	993
2008	6,726	4,358	257	948
2009	5,975	3,915	197	1,194
2010	4,937	3,260	156	867
2011	4,070	2,721	0	–
all	17,733	10,908	12,569	13,663

Note: The numbers provided are for the estimation sample. The column 'Entries' gives the number of individuals whose first observation is in the respective year; 'Exits' gives the number of individuals whose last observation is in that year. In the initial year, no entries could be calculated (–) and the number of exits is zero by construction because the estimation sample only includes individuals observed for at least two consecutive periods. Similarly, in the final year, no exits could be calculated and the number of entries is zero by construction. *Source*: SOEP, 2011

Table B.2: *The lengths of individuals' observation periods*

Length of observation period in years	# of individuals	Share of individuals in %	Cumulative share of individuals in %
2	3,120	18	18
3	2,382	13	31
4	1,831	10	41
5	1,544	9	50
6	1,476	8	58
7	978	6	64
8	906	5	69
9	812	5	74
10	974	5	79
11	642	4	83
12	1,192	7	89
13	344	2	91
14	463	3	94
15	270	2	95
16	214	1	97
17	585	3	100
all	17,733‘		

Note: The numbers provided are for the estimation sample, which is why individuals with only one observation are not included. Median and mean observation length are 5 waves and 6.6 waves, respectively.
Source: SOEP, 2011

Table B.3: *Sample means of selected variables*

Variable	Cross-year average		1995-2004		2005-11	
	SA=0	SA=1	SA=0	SA=1	SA=0	SA=1
female	0.50	0.56	0.50	0.55	0.51	0.58
migrant	0.16	0.27	0.15	0.27	0.17	0.28
age	42.7	42.0	42.4	41.2	43.3	42.8
years of education	12.3	11.0	12.1	10.9	12.5	11.0
good health	0.55	0.41	0.55	0.45	0.53	0.38
poor health	0.13	0.24	0.13	0.22	0.14	0.27
single, without children	0.27	0.29	0.24	0.22	0.30	0.36
single, with children	0.06	0.23	0.06	0.22	0.07	0.24
couples, without children	0.29	0.16	0.30	0.15	0.29	0.17
couples, with children	0.38	0.32	0.40	0.40	0.34	0.24
child $\leq$ 6 years	0.18	0.27	0.19	0.31	0.16	0.23
household size	2.7	2.9	2.8	3.1	2.7	2.7
reg. unemployment rate	9.7	11.5	10.2	12.3	8.9	10.7
Eastern Germany	0.15	0.32	0.15	0.32	0.15	0.31

Note: Columns 2 and 3 give sample means of the listed variables averaged across all periods for social assistance non-recipients (SA=0) and recipients (SA=1), respectively. Columns 4-5 and 6-7 give means for both groups in the pre-Hartz period (1995-2004) and post-Hartz period (2005-11), respectively. The definition of social assistance used is the same as in the main text. Means have been calculated using individual cross-sectional sampling weights. *Source*: SOEP, 2011

Table B.4: *Sample means of selected variables in the initial and final wave*

Variable	1995	2011	Cross-year average
female	0.49	0.53	0.51
migrant	0.15	0.18	0.17
age	42.2	43.7	42.7
years of education	11.8	12.6	12.2
good health	0.51	0.50	0.53
poor health	0.15	0.16	0.14
single, without children	0.22	0.34	0.27
single, with children	0.06	0.08	0.08
couples, without children	0.34	0.29	0.28
couples, with children	0.39	0.29	0.37
child $\leq$ 6 years	0.19	0.15	0.19
household size	2.8	2.6	2.8
reg. unemployment rate	9.5	7.3	9.8
Eastern Germany	0.17	0.19	0.17

Note: Columns 2, 3 and 4 give sample means of the listed variables for the initial year 1995, the final year 2011, and averaged over all years in the sample, respectively. Means have been calculated using individual cross-sectional sampling weights. *Source*: SOEP, 2011

Appendix C

Coefficient estimates for models with time-varying state dependence

Table C.1: *Specifications with post-Hartz interactions*

	Full sample		Western Germany		Eastern Germany	
SA_{t-1}	1.105***	(0.036)	1.231***	(0.045)	0.863***	(0.061)
$H_t \times SA_{t-1}$	0.070	(0.049)	0.005	(0.060)	0.191**	(0.083)
individual characteristics						
female	-0.004	(0.034)	-0.020	(0.040)	0.041	(0.065)
age	-0.067***	(0.016)	-0.074***	(0.019)	-0.035	(0.031)
age^2	0.078***	(0.019)	0.085***	(0.022)	0.044	(0.037)
years of education	-0.236***	(0.048)	-0.192***	(0.053)	-0.613***	(0.144)
years of education^2	0.005***	(0.002)	0.004*	(0.002)	0.018***	(0.005)
good health	-0.038	(0.038)	0.003	(0.045)	-0.122*	(0.068)
poor health	0.112**	(0.047)	0.140**	(0.056)	0.058	(0.086)
migrant	0.278***	(0.055)	0.264***	(0.057)	0.471**	(0.222)
household characteristics						
single, with children	0.042	(0.095)	0.139	(0.122)	-0.025	(0.153)
couple, no children	0.187*	(0.102)	0.302**	(0.123)	0.016	(0.186)
couple, with children	0.001	(0.106)	0.021	(0.130)	0.059	(0.190)
child $\leq$ 6 years	0.035	(0.052)	0.127**	(0.061)	-0.180*	(0.098)
household size	0.057*	(0.030)	0.069**	(0.035)	0.013	(0.059)
partner characteristics						
age	-0.019***	(0.005)	-0.018***	(0.006)	-0.024**	(0.011)
age^2	0.026***	(0.009)	0.026***	(0.010)	0.032*	(0.017)
years of education	0.057***	(0.015)	0.040**	(0.018)	0.111***	(0.033)
years of education^2	-0.004***	(0.001)	-0.004***	(0.001)	-0.007***	(0.002)
good health	0.013	(0.041)	0.030	(0.050)	-0.036	(0.074)
poor health	0.208***	(0.053)	0.235***	(0.064)	0.158*	(0.095)
migrant	0.186***	(0.058)	0.198***	(0.060)	0.602**	(0.239)
Wooldridge controls						
SA_0	1.310***	(0.056)	1.261***	(0.069)	1.374***	(0.098)
avg: good health	-0.153**	(0.066)	-0.171**	(0.081)	-0.113	(0.120)
avg: poor health	0.269***	(0.084)	0.291***	(0.099)	0.174	(0.160)
avg: single, w. children	0.421***	(0.125)	0.315**	(0.155)	0.465**	(0.223)
avg: couple, no children	-0.481***	(0.117)	-0.680***	(0.144)	-0.280	(0.212)
avg: couple, w. children	-0.235*	(0.130)	-0.389**	(0.157)	-0.088	(0.243)
avg: child $\leq$ 6 years	0.352***	(0.080)	0.188**	(0.094)	0.872***	(0.161)
avg: household size	0.065*	(0.035)	0.069*	(0.040)	0.053	(0.073)
avg: reg. unempl. rate	0.017	(0.012)	0.014	(0.018)	-0.015	(0.021)
avg: good health (prtnr)	-0.144**	(0.070)	-0.050	(0.085)	-0.246**	(0.125)
avg: poor health (prtnr)	0.145	(0.093)	0.211*	(0.110)	0.050	(0.179)
individual characteristics (post-Hartz)						
female	-0.003	(0.045)	0.016	(0.054)	-0.020	(0.082)
age	-0.025	(0.024)	-0.023	(0.029)	-0.026	(0.047)
age^2	0.023	(0.029)	0.022	(0.034)	0.027	(0.055)
years of education	-0.202***	(0.071)	-0.221***	(0.078)	-0.310	(0.198)
years of education^2	0.006**	(0.003)	0.008**	(0.003)	0.009	(0.007)
good health	-0.060	(0.058)	-0.180**	(0.070)	0.196*	(0.107)
poor health	-0.071	(0.073)	-0.087	(0.086)	-0.059	(0.142)
migrant	-0.022	(0.070)	0.002	(0.072)	-0.493	(0.301)
household characteristics (post-Hartz)						
single, with children	-0.047	(0.143)	-0.095	(0.183)	-0.027	(0.236)

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Table C.1 – continued from previous page –

dependent variable: SA_t	Full sample		Western Germany		Eastern Germany	
couple, no children	-0.263	(0.161)	-0.445**	(0.197)	0.136	(0.287)
couple, with children	-0.219	(0.163)	-0.208	(0.202)	-0.267	(0.285)
child $\leq$ 6 years	0.075	(0.089)	0.051	(0.103)	0.114	(0.180)
household size	0.042	(0.049)	-0.047	(0.059)	0.260***	(0.093)
partner characteristics (post-Hartz)						
age	0.000	(0.007)	0.003	(0.009)	-0.005	(0.015)
age^2	0.000	(0.012)	-0.008	(0.014)	0.017	(0.024)
years of education	-0.004	(0.022)	0.017	(0.025)	-0.064	(0.044)
years of education^2	-0.001	(0.001)	-0.002	(0.001)	0.002	(0.002)
good health	-0.039	(0.066)	-0.089	(0.079)	0.079	(0.120)
poor health	-0.136	(0.086)	-0.142	(0.101)	-0.155	(0.163)
migrant	-0.064	(0.078)	-0.101	(0.082)	-0.220	(0.331)
Wooldridge controls (post-Hartz)						
SA_0	-0.078	(0.055)	-0.083	(0.069)	-0.089	(0.095)
avg: good health	0.127	(0.098)	0.224*	(0.120)	-0.066	(0.174)
avg: poor health	0.095	(0.128)	0.099	(0.151)	0.053	(0.245)
avg: single, w. children	-0.216	(0.176)	-0.147	(0.220)	-0.010	(0.314)
avg: couple, no children	0.242	(0.182)	0.505**	(0.222)	-0.210	(0.330)
avg: couple, w. children	-0.043	(0.190)	0.079	(0.231)	-0.014	(0.349)
avg: child $\leq$ 6 years	-0.041	(0.126)	0.081	(0.147)	-0.413	(0.257)
avg: household size	-0.058	(0.055)	0.038	(0.064)	-0.356***	(0.110)
avg: reg. unempl. rate	0.009	(0.017)	0.015	(0.024)	0.001	(0.028)
avg: good health (prtnr)	0.187*	(0.104)	0.089	(0.127)	0.326*	(0.186)
avg: poor health (prtnr)	0.174	(0.145)	0.103	(0.171)	0.182	(0.281)
reg. unemployment rate	0.041***	(0.011)	0.051***	(0.017)	0.035**	(0.015)
$H_t\times$ reg. unempl. rate	-0.003	(0.014)	-0.016	(0.022)	0.017	(0.019)
Eastern Germany	0.267***	(0.069)				
$H_t\times$ Eastern Germany	-0.068	(0.089)				
constant	0.111	(0.453)	-0.045	(0.517)	2.930**	(1.168)
$H_t\times$ constant	2.324***	(0.687)	2.274***	(0.797)	3.252**	(1.610)
σ_a	0.839***	(0.026)	0.784***	(0.030)	0.953***	(0.048)
ρ	0.413***	(0.015)	0.381***	(0.018)	0.476***	(0.025)
log Likelihood	-14,528.497		-9,664.339		-4,780.015	
# of observations	100,434		79,790		20,644	
# of individuals	17,733		14,010		3,723	

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

Note: Standard errors are given in parentheses. SA_t, SA_{t-1} and SA_0 are the observed social assistance receipt status in the current, the last, and the initial observed period, respectively, H_t is a dummy variable for the post-Hartz years 2005-2011. All other covariates are lagged by one period. Blocks of variables labelled '(post-Hartz)' have been interacted with the post-Hartz dummy. 'avg:' denotes an individual's longitudinal average of a variable. Values of age^2 have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011

Table C.2: *Specifications with survey-year interactions*

dependent variable: SA_t	Full sample		Western Germany		Eastern Germany	
SA_{t-1}	1.199***	(0.095)	1.359***	(0.119)	0.932***	(0.163)
calendar-year interactions						
SA_{t-1}×1997	-0.377***	(0.127)	-0.254	(0.158)	-0.522**	(0.228)
SA_{t-1}×1998	-0.177	(0.132)	-0.246	(0.159)	-0.181	(0.249)
SA_{t-1}×1999	-0.181	(0.126)	-0.119	(0.155)	-0.425*	(0.222)
SA_{t-1}×2000	-0.114	(0.132)	0.050	(0.164)	-0.495**	(0.230)
SA_{t-1}×2001	-0.022	(0.118)	-0.107	(0.147)	0.100	(0.205)
SA_{t-1}×2002	0.156	(0.124)	0.077	(0.154)	0.220	(0.212)
SA_{t-1}×2003	-0.025	(0.125)	-0.103	(0.156)	0.044	(0.211)
SA_{t-1}×2004	-0.001	(0.122)	-0.220	(0.152)	0.316	(0.212)
SA_{t-1}×2005	-0.329***	(0.122)	-0.333**	(0.153)	-0.349*	(0.207)
SA_{t-1}×2006	0.000	(0.124)	-0.023	(0.155)	-0.013	(0.212)
SA_{t-1}×2007	0.081	(0.121)	-0.038	(0.151)	0.320	(0.212)
SA_{t-1}×2008	0.133	(0.126)	-0.027	(0.156)	0.351	(0.221)
SA_{t-1}×2009	0.063	(0.131)	-0.135	(0.165)	0.431*	(0.224)
SA_{t-1}×2010	0.321**	(0.139)	0.153	(0.174)	0.542**	(0.237)
SA_{t-1}×2011	0.028	(0.143)	-0.254	(0.179)	0.593**	(0.254)
individual characteristics						
female	-0.004	(0.029)	-0.010	(0.033)	0.023	(0.056)
age	-0.079***	(0.013)	-0.078***	(0.015)	-0.076***	(0.024)
age^2	0.088***	(0.015)	0.089***	(0.017)	0.085***	(0.029)
years of education	-0.289***	(0.041)	-0.252***	(0.045)	-0.696***	(0.126)
years of education^2	0.007***	(0.002)	0.006***	(0.002)	0.020***	(0.005)
good health	-0.058**	(0.028)	-0.069**	(0.034)	-0.020	(0.051)
poor health	0.076**	(0.035)	0.095**	(0.041)	0.022	(0.067)
migrant	0.266***	(0.045)	0.263***	(0.046)	0.268	(0.184)
household characteristics						
single, with children	0.030	(0.064)	0.098	(0.082)	-0.046	(0.105)
couple, no children	0.072	(0.074)	0.100	(0.090)	0.056	(0.133)
couple, with children	-0.073	(0.075)	-0.061	(0.092)	-0.034	(0.132)
child $\leq$ 6 years	0.101***	(0.039)	0.166***	(0.046)	-0.062	(0.077)
household size	0.063***	(0.022)	0.051*	(0.026)	0.102**	(0.043)
partner characteristics						
age	-0.018***	(0.004)	-0.016***	(0.005)	-0.021***	(0.008)
age^2	0.023***	(0.007)	0.020***	(0.008)	0.027**	(0.013)
years of education	0.056***	(0.012)	0.047***	(0.014)	0.087***	(0.024)
years of education^2	-0.005***	(0.001)	-0.004***	(0.001)	-0.006***	(0.001)
good health	0.005	(0.031)	0.002	(0.038)	0.019	(0.057)
poor health	0.152***	(0.041)	0.174***	(0.048)	0.100	(0.076)
migrant	0.164***	(0.048)	0.163***	(0.049)	0.502**	(0.200)
calendar-year effects						
1997	-0.011	(0.064)	0.120	(0.074)	-0.321**	(0.129)
1998	0.079	(0.065)	0.157**	(0.079)	0.074	(0.127)
1999	-0.045	(0.067)	0.016	(0.080)	-0.022	(0.130)
2000	-0.043	(0.067)	-0.122	(0.084)	0.177	(0.124)
2001	-0.072	(0.061)	-0.080	(0.072)	0.042	(0.120)
2002	-0.125**	(0.063)	-0.159**	(0.077)	0.051	(0.123)
2003	0.107*	(0.060)	0.094	(0.072)	0.268**	(0.121)

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Table C.2 – continued from previous page –

	Full sample		Western Germany		Eastern Germany	
2004	0.080	(0.063)	0.109	(0.074)	0.217*	(0.131)
2005	0.221***	(0.063)	0.249***	(0.073)	0.360***	(0.130)
2006	0.191***	(0.067)	0.203**	(0.083)	0.417***	(0.132)
2007	-0.048	(0.069)	0.049	(0.082)	-0.142	(0.139)
2008	0.010	(0.068)	0.018	(0.082)	0.023	(0.128)
2009	0.130*	(0.070)	0.196**	(0.082)	-0.132	(0.137)
2010	0.070	(0.074)	0.050	(0.089)	0.045	(0.139)
2011	0.085	(0.079)	0.176*	(0.090)	-0.340**	(0.172)
Wooldridge controls						
SA_0	1.256***	(0.048)	1.221***	(0.059)	1.307***	(0.085)
avg: good health	-0.111**	(0.055)	-0.084	(0.066)	-0.190*	(0.102)
avg: poor health	0.309***	(0.071)	0.334***	(0.083)	0.230	(0.141)
avg: single, w. children	0.299***	(0.093)	0.250**	(0.114)	0.357**	(0.173)
avg: couple, no children	-0.370***	(0.089)	-0.452***	(0.108)	-0.295*	(0.166)
avg: couple, w. children	-0.265***	(0.097)	-0.355***	(0.116)	-0.129	(0.186)
avg: child $\leq$ 6 years	0.256***	(0.064)	0.191***	(0.074)	0.520***	(0.132)
avg: household size	0.059**	(0.028)	0.084***	(0.032)	-0.018	(0.058)
avg: good health (prtnr)	-0.099*	(0.057)	-0.026	(0.069)	-0.221**	(0.106)
avg: poor health (prtnr)	0.205***	(0.079)	0.260***	(0.092)	0.116	(0.159)
avg: reg. unempl. rate	0.022**	(0.010)	0.042**	(0.018)	0.004	(0.017)
reg. unemployment rate	0.039***	(0.010)	0.021	(0.017)	0.007	(0.016)
Eastern Germany	0.242***	(0.056)				
constant	0.890**	(0.374)	0.538	(0.425)	4.834***	(0.987)
σ_a	0.823***	(0.026)	0.778***	(0.031)	0.934***	(0.048)
ρ	0.404***	(0.015)	0.377***	(0.019)	0.466***	(0.026)
log Likelihood	-14,507.179		-9,659.002		-4,737.816	
# of observations	100,434		79,790		20,644	
# of individuals	17,733		14,010		3,723	

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

Note: Standard errors are given in parentheses. SA_t, SA_{t-1} and SA_0 are the observed social assistance receipt status in the current, the last, and the initial observed period, respectively. All other covariates are lagged by one period. 'avg:' denotes an individual's longitudinal average of a variable. Values of age^2 have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. *Source*: SOEP, 2011

Appendix D

Results from weighted estimations

Table D.1: *Weighted specifications*

dependent variable: SA_t	Full sample		Western Germany		Eastern Germany	
SA_{t-1}	1.135***	(0.049)	1.218***	(0.063)	0.928***	(0.067)
individual characteristics						
female	-0.006	(0.043)	-0.021	(0.049)	0.054	(0.075)
migrant	0.323***	(0.075)	0.324***	(0.076)	0.372	(0.291)
age	-0.107***	(0.019)	-0.102***	(0.022)	-0.117***	(0.035)
age^2	0.125***	(0.023)	0.119***	(0.027)	0.136***	(0.041)
years of education	-0.267***	(0.067)	-0.256***	(0.071)	-0.580***	(0.200)
years of education^2	0.006**	(0.003)	0.006**	(0.003)	0.016**	(0.007)
good health	-0.084**	(0.040)	-0.095**	(0.048)	-0.043	(0.070)
poor health	0.063	(0.049)	0.063	(0.056)	0.040	(0.104)
household characteristics						
single, with children	-0.078	(0.108)	-0.027	(0.143)	-0.165	(0.149)
couple, no children	0.078	(0.101)	0.117	(0.120)	-0.063	(0.196)
couple, with children	-0.058	(0.117)	-0.066	(0.143)	-0.051	(0.205)
child $\leq$ 6 years	0.101*	(0.058)	0.186***	(0.064)	-0.189	(0.116)
household size	0.089**	(0.036)	0.065	(0.042)	0.168**	(0.067)
partner characteristics						
age	-0.023***	(0.006)	-0.021***	(0.007)	-0.023**	(0.011)
age^2	0.030***	(0.010)	0.029**	(0.012)	0.027	(0.018)
years of education	0.059***	(0.021)	0.048**	(0.024)	0.109***	(0.040)
years of education^2	-0.005***	(0.001)	-0.004***	(0.002)	-0.008***	(0.002)
good health	-0.021	(0.048)	-0.027	(0.058)	0.018	(0.077)
poor health	0.186***	(0.054)	0.213***	(0.063)	0.101	(0.103)
migrant	0.159**	(0.079)	0.149*	(0.080)	0.684**	(0.294)
calendar-year effects						
1997	-0.107	(0.092)	0.013	(0.109)	-0.473***	(0.159)
1998	0.082	(0.087)	0.168	(0.103)	-0.105	(0.173)
1999	-0.092	(0.081)	-0.007	(0.093)	-0.280	(0.173)
2000	-0.010	(0.089)	-0.042	(0.106)	0.077	(0.168)
2001	-0.142*	(0.075)	-0.147	(0.091)	-0.103	(0.150)
2002	-0.077	(0.081)	-0.100	(0.101)	0.017	(0.147)
2003	0.073	(0.079)	0.060	(0.093)	0.154	(0.172)
2004	0.096	(0.084)	0.099	(0.099)	0.181	(0.158)
2005	0.148*	(0.086)	0.130	(0.098)	0.290	(0.185)
2006	0.278***	(0.091)	0.312***	(0.107)	0.338*	(0.177)
2007	0.014	(0.090)	0.041	(0.111)	0.018	(0.147)
2008	0.033	(0.085)	-0.003	(0.103)	0.133	(0.143)
2009	0.153	(0.098)	0.152	(0.120)	0.111	(0.156)
2010	0.092	(0.091)	0.047	(0.110)	0.200	(0.161)
2011	-0.010	(0.107)	-0.005	(0.129)	-0.078	(0.170)
Wooldridge controls						
SA_0	1.475***	(0.081)	1.431***	(0.101)	1.499***	(0.116)
avg: good health	-0.087	(0.080)	-0.073	(0.095)	-0.121	(0.132)
avg: poor health	0.273***	(0.100)	0.301***	(0.115)	0.167	(0.185)
avg: single, w. children	0.414***	(0.145)	0.311*	(0.178)	0.647***	(0.242)
avg: couple, no children	-0.387***	(0.136)	-0.561***	(0.154)	0.094	(0.263)
avg: couple, w. children	-0.248	(0.153)	-0.334*	(0.179)	0.023	(0.291)
avg: child $\leq$ 6 years	0.185*	(0.095)	0.084	(0.105)	0.595***	(0.187)

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Table D.1 – continued from previous page –

	Full sample		Western Germany		Eastern Germany	
avg: household size	0.033	(0.045)	0.065	(0.051)	-0.075	(0.090)
avg: good health (prtnr)	0.034	(0.085)	0.137	(0.102)	-0.296**	(0.143)
avg: poor health (prtnr)	0.177*	(0.107)	0.228*	(0.124)	0.051	(0.204)
avg: reg. unempl. rate	0.032*	(0.017)	0.054**	(0.025)	-0.016	(0.023)
reg. unemployment rate	0.021	(0.016)	0.003	(0.024)	0.017	(0.026)
Eastern Germany	0.297***	(0.083)				
constant	1.338**	(0.575)	1.105*	(0.643)	4.976***	(1.526)
σ_a	0.890***	(0.040)	0.848***	(0.049)	0.966***	(0.060)
ρ	0.442***	(0.022)	0.418***	(0.028)	0.483***	(0.031)
log Likelihood	-48,919,089		-36,256,937		-12,385,710	
# of observations	100,434		79,790		20,644	
# of individuals	17,733		14,010		3,723	

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

Note: Standard errors are given in parentheses. SA_t, SA_{t-1} and SA_0 are the observed social assistance receipt status in the current, the last, and the initial observed period, respectively. All other covariates are lagged by one period. 'avg:' denotes an individual's longitudinal average of a variable. Values of age^2 have been scaled through division by 100. The breakdown of the sample into Western and Eastern Germany is based on region of residence in an individual's initial observed period. Estimates have been obtained using individual cross-sectional sampling weights. *Source*: SOEP, 2011

Table D.2: *Weighted specifications - predicted transition rates, APE and PPR*

	Full sample	Western Germany	Eastern Germany
Average predicted entry rate in %	6.1	4.8	13.3
Average predicted persistence rate in %	18.9	17.8	27.8
Average partial effect (in ppts)	12.8	13.1	14.6
Predicted probability ratio (in ppts)	3.1	3.7	2.1

Note: Calculations are based on the coefficient estimates presented in Table D.1. All averages have been calculated using individual cross-sectional sampling weights. Results for Western and Eastern Germany are *within-sample* predictions. *Source*: SOEP, 2011

Appendix E

Variable definitions for Chapter 3

Table E.1: *Variable definitions for Chapter 3*

Variable name	Definition
benefit receipt variables	
SA_t *(monthly)*	dummy variable =1 if in month t, the individual lived in a household in which social assistance benefit receipt was recorded for *any single household member* in that month, =0 if no social assistance benefit receipt was recorded for any household member; benefit programmes considered when defining the benefit variable are the *revenue minimum garanti* (RMG) for Luxembourg, *bijstand* for the Netherlands, Social Economic Assistance for Norway and Social Assistance for Sweden.
SA_t *(annualized)*	dummy variable =1 if in December of year t, the individual lived in a household in which social assistance benefit receipt was recorded for *any single household member at any time during the calendar year*, =0 if no social assistance benefit receipt was recorded for any household member
individual characteristics	
female	dummy variable =1 if the individual is female, =0 otherwise
immigrant	dummy variable =1 if the individual was born abroad, =0 otherwise; for Luxembourg and Norway: individuals without local nationality; for Sweden: individuals living in a household in which at least one of the adults was born abroad
age	individual's age in years
household characteristics	
single, no children	dummy variable =1 if the individual lives in the household of a non-partnered adult without children as defined in the data source, =0 otherwise
single, with children	dummy variable =1 if the individual lives in the household of a non-partnered adult with children as defined in the data source, =0 otherwise
couple, no children	dummy variable =1 if the individual lives in the household of partnered adults without children as defined in the data source, =0 otherwise
couple, with children	dummy variable =1 if the individual lives in the household of partnered adults with children as defined in the data source, =0 otherwise

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Table E.1 – continued from previous page –

Note: All variables except the annualized benefit variable are measured once per month. The benefit variable SA_t is constructed at the household level taking into account benefit receipt by any member in that month (monthly benefit variable) / at any time during the calendar year (annual benefit variable). 'Individual characteristics' are defined at the individual level, 'household characteristics' are defined at the household level and identical for all members of the household. Household-type variables were defined from corresponding variables in the original data (*i.e.* not by classifying households based on their observed composition and the age of their members). No reliable household-type information is available for Luxembourg.

Appendix F

Results for unbalanced panels

Table F.1: *The number of benefit spells per individual – unbalanced panel*

	# of	# of spells		share of individuals in %	
	of recipients	median	mean	with 2 spells	with 5 spells
Luxembourg	15,338	1	1.4	27.2	0.4
Netherlands	4,785	1	1.4	29.6	1.0
Norway	42,367	2	2.2	54.0	10.0
Sweden	4765,259	2	3.3	64.6	24.1

Note: Calculations are for the period from January 2001 to December 2008 and based on monthly administrative data from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd) and Sweden (NBHW database). The panel includes individuals who are only observed for part of the observation period. The sample used takes account of short-term exits (compare panel A in Table 3.4).

Table F.2: *The total duration of benefit receipt across spells – unbalanced panel*

country	duration in months		share of recipients in % with a duration		
	median	mean	$\geq$ 6 months	$\geq$ 12 months	$\geq$ 24 months
Luxembourg	31	39.8	88.2	75.7	58.5
Netherlands	23	36.3	79.6	65.8	49.7
Norway	7	14.9	54.4	36.7	20.9
Sweden	9	19.0	60.2	44.7	27.8

Note: Calculations are for the period from January 2001 to December 2008 and based on monthly administrative data from Luxembourg (FNS database), the Netherlands (SSB), Norway (FD-Trygd) and Sweden (NBHW database). The panel includes individuals who are only observed for part of the observation period. The total period of benefit receipt is the cumulative time spent in social assistance across all spells who are observed to start during the years 2001 - 2008.

Appendix G

Variable definitions for Chapter 4

Table G.1: *Variable definitions for Chapter 4 - benefit variables*

Variable name	Definition
monthly specifications (Tables J.1, J.4, J.5 and J.7)	
SEA_t	dummy variable =1 if in month t, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member
SEA_{t-s} with $s = 1, ..., 12$	dummy variable =1 if in month $t - s$, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member
SEA_0	dummy variable =1 if in the initial month of the observation period (January of the years 1993 to 1999), the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member; In the specification with higher-order lags (Table J.4), the values of the benefit variable in the initial twelve months SEA_0 to SEA_{11} are used. In the DRCP models (Tables J.5 and J.7), SEA_{11} is used (see explanation in the main text).
annual specifications ('point-in-time') (Tables J.1, J.2 and J.6)	
SEA_t	dummy variable =1 if in December of year t, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member
SEA_{t-1}	dummy variable =1 if in December of year $t - 1$, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member
SEA_0	dummy variable =1 if in December of the initial year (1993 to 1999), the individual lived in a household in which SEA benefit receipt was recorded for *any single household member* in that month, =0 if no SEA benefit receipt was recorded for any household member. In the DRCP models (Table J.6), only SEA_{11} is used (see explanation in the main text).

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Table G.1 – continued from previous page –

Variable name	Definition
annual specifications ('benefit year') (Table J.3)	
SEA_t	dummy variable =1 if in December of year t, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member at any time during that calendar year*, =0 if no SEA benefit receipt was recorded for any household member
SEA_{t-1}	dummy variable =1 if in December of year $t-1$, the individual lived in a household in which SEA benefit receipt was recorded for *any single household member at any time during that calendar year*, =0 if no SEA benefit receipt was recorded for any household member
SEA_0	dummy variable =1 if in December of the initial year (1993 to 1999), the individual lived in a household in which SEA benefit receipt was recorded for *any single household member at any time during that calendar year*, =0 if no SEA benefit receipt was recorded for any household member
duration and occurrence dummies (Table J.7 only)	
d_k^0 with $k = 2, ..., 12$	set of dummy variables =1 if in the previous period, the ongoing spell *off SEA benefits* had lasted exactly k months ($d_{12}^0 = 1$ if the spell off benefits had lasted 12 months *or longer*), =0 if the ongoing spell off benefits had lasted more or less than k months or if the individual was *on* SEA benefits; the reference category is a spell off benefits in its first month ($d_1^0 = 1$); d_k^0 is the notation used for the set of duration dummies d_k included in the *entry equation*
d_k^1 with $k = 2, ..., 12$	set of dummy variables =1 if in the previous period, the ongoing spell *on SEA benefits* had lasted exactly k months ($d_{12}^0 = 1$ if the spell on benefits had lasted 12 months *or longer*), =0 if the ongoing spell on benefits had lasted more or less than k months or if the individual was *off* SEA benefits; the reference category is a spell on benefits in its first month (d_1^1=1); d_k^1 is the notation used for the set of duration dummies d_k included in the *persistence equation*
o_k with $k = 1, ..., 11$	set of dummy variables =1 if in the previous period, the individual had completed exactly k earlier spells of SEA receipt; ($o_{11} = 1$ if the number of earlier benefit spells was 11 *or greater*), =0 if in the previous period the individual had more or less than k earlier benefit spells; the reference category is no previous benefit spell ($o_0 = 1$); the same set of occurrence dummies o_k is included in both the *entry* and *persistence equation*

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Table G.1 – continued from previous page –

Variable name	Definition
$d_{k,11}$ with $k = 2, ..., 12$	set of dummy variables =1 if in the initial period (here: *t=11*), the ongoing spell, *whether on or off* SEA benefits, lasted exactly k months, =0 if the ongoing spell lasted more or less than k months. The reference category is a spell on or off benefits in its first month ($d_{1,11} = 1$). $d_{k,11}$ is the set of duration dummies for the initial period included in both the *entry* and the *persistence equation*; since it is interacted with the outcome variable in the initial period SEA_{11}, the effect of an initial spell *on* and *off* benefits can differ.
$o_{k,11}$ with $k = 1, ..., 4$	set of dummy variables =1 if in the initial period (here: *t=11*), the individual had completed exactly k previous spells of SEA receipt, =0 if in the initial period, the individual had more or less than k earlier benefit spells. The reference category is the case of no previous benefit spell ($o_{0,11} = 1$); the same set of occurrence dummies o_k is included in the both the *entry* and *persistence equation*

Note: The benefit variables SEA_t, SEA_{t-s} and SEA_0 (and thus also the derived duration and occurrence dummies) are constructed *at the household level* taking into account the receipt of SEA benefits by any member of the household in the given month. The timing of the measurement differs by variable type ('point-in-time' *vs.* 'benefit year') as described in the Table. Construction of the benefit variables in the biennial model (Tables J.2 and J.3) is done as for the corresponding annual model, with every second observation dropped.

Table G.2: *Variable definitions for Chapter 4 - covariates*

Variable name	Definition
individual characteristics	
female	dummy variable =1 if the individual is female, =0 otherwise
immigrant	dummy variable =1 if the individual does not have the Norwegian nationality, =0 otherwise
age	individual's age in years
age^2	individual's squared age in years (divided by 100)
years of education	individual's completed years of schooling
years of education^2	individual's squared completed years of schooling
household characteristics	
single, no children	dummy variable =1 if the individual lives in the household of a non-partnered adult without children as defined in the data source, =0 otherwise
single, with children	dummy variable =1 if the individual lives in the household of a non-partnered adult with as defined in the data source, =0 otherwise
couple, no children	dummy variable =1 if the individual lives in the household of partnered adults without children as defined in the data source, =0 otherwise
couple, with children	dummy variable =1 if the individual lives in the household of partnered adults with children as defined in the data source, =0 otherwise
child, aged 0-5 years	dummy variable =1 if the individual lives in a household with at least one child aged $\leq$ 5 years, =0 otherwise
household size	number of individuals living in the household
spouse characteristics	
immigrant	dummy variable =1 if the individual's spouse does not have the Norwegian nationality, =0 otherwise or if the individual is not married
age	age in years of the individual's spouse, =0 if the individual is not married
age^2	squared age in years of the individual's spouse, =0 if the individual is not married
years of education	completed years of schooling of the individual's spouse, =0 if the individual is not married
years of education^2	squared completed years of schooling of the individual's spouse, =0 if the individual is not married
year dummies	
1995, ..., 2008	dummy variables =1 in the respective calendar year, =0 otherwise
local unemployment rate	unemployment rate in the individual's municipality of residence

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Table G.2 – continued from previous page –

Note: All variables are measured at the time of the interview in the respective year. 'Individual characteristics' are defined at the individual level, 'household characteristics' are defined at the household level and identical for all members of the household. Household-type variables were defined from corresponding variables in the original data (*i.e.* not by classifying households based on their observed composition and the age of their members). The 'spouse characteristics' of a married individual correspond to the 'individual characteristics' of her spouse (and *vice versa*). Spouse characteristics are equal to zero for individuals who are partnered but not married.

Appendix H

An extension of the standard Wooldridge approach

The dynamic random-effects probit model with persistent unobserved heterogeneity suffers from an endogeneity problem due to the correlation of the lagged dependent variable with the time-invariant individual-specific error component α_i, as discussed in Chapter 2. This initial conditions problem is even more severe in the specifications estimated in Sections 4.4.3 and 4.5.2 that additionally include higher-order lags or measures of spell duration and the number of previous benefit spells. The reason is that these variables are functions of past realizations of the dependent variable and that they therefore by construction do not satisfy the strict exogeneity assumption made for the covariates.

In the standard model, we use the approach introduced by Wooldridge (2005) to address the initial conditions problem by specifying a relationship between α_i and the outcome in the initial period y_{i0}, which the model conditions on. We can extend this method to allow for feedback effects from y_{it} to the endogenous covariates (duration and occurrence variables or the higher-order lags) that we call w_{it} by employing an approach developed by Wooldridge (2000) for dealing more broadly with endogenous regressors in dynamic unobserved-effects models.

Using the product law for conditional densities, we decompose the joint density of (y_{it}, w_{it}) as

$$f(y_{it}, w_{it}|y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \zeta) = f(y_{it}|y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \theta) \times f(w_{it}|y_{it}, y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \omega). \quad \text{(H.1)}$$

As in Section 4.3.1, the joint likelihood of the benefit receipt status and the set of endogenous covariates over all time periods follows as

$$f(y_i, w_i|y_{i0}, w_{i0}, x_i; \zeta) = \int \prod_{t=1}^{T} f(y_{it}|y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \theta) \times f(w_{it}|y_{it}, y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \omega) g(\alpha_i|y_{i0}, w_{i0}, x_i; \gamma)\, d\alpha_i, \quad \text{(H.2)}$$

where (y_{i0}, w_{i0}) are the initial-period values of the benefit receipt status and the endogenous covariates, respectively. Since in our monthly models, the

endogenous covariates w_{it} are deterministic functions of y_{it}, y_{it-1}, and w_{it-1}, this joint distribution simplifies to

$$f(y_i, w_i | y_{i0}, w_{i0}, x_i; \zeta) = \int \prod_{t=1}^{T} f(y_{it} | y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \theta) g(\alpha_i | y_{i0}, w_{i0}, x_i; \gamma) \, d\alpha_i. \quad \text{(H.3)}$$

As in the simple case, we can use this joint density to consistently estimate the model under the assumption that the distribution of $\alpha_i | y_{i0}, w_{i0}, x_i$ is conditionally normal with expectation linear in all arguments and constant variance. The likelihood function of our monthly specification with higher-order lags or duration and occurrence variables is hence similar to that of the standard random-effects probit model, where the initial values of the benefit variable $y_{i0}, ..., y_{i11}$ and of the duration and occurrence variables w_{i0} and again a vector of the lags and leads of all time-varying exogenous covariates x_i are included as additional regressors. As in the standard case, we replace x_i by the vector of longitudinal averages $\bar{x}_i$.

Note however that this approach does not easily extend to the annual model counterpart to the monthly models in Section 4.5.2 that we might have wanted to use as a benchmark for the predictions of our monthly specifications. Here, w_{it} is no longer a deterministic function of $(y_{it}, y_{it-1}, w_{it-1})$, because our measures of spell durations and occurrences are based on *monthly* information whereas the the outcome variable y_{it} is now measured at the *annual* level. In the annual case, Equation (H.2) therefore cannot be simplified to give Equation (H.3) and the outlined approach does not lead to consistent estimates.

Appendix I

Descriptive statistics for Chapter 4

Table I.1: *Sample size and attrition*

Cohort	Cohort entry wave	Cohort exit wave	# of individuals	Share with incomplete obs. period (in %)	Average obs. period (in months)
1	Jan. 1993	Dec. 2002	4,883	3.7	118.3
2	Jan. 1994	Dec. 2003	4,503	4.6	117.9
3	Jan. 1995	Dec. 2004	4,330	3.5	118.2
4	Jan. 1996	Dec. 2005	4,168	4.1	118.2
5	Jan. 1997	Dec. 2006	4,445	3.8	118.2
6	Jan. 1998	Dec. 2007	4,248	4.0	118.1
7	Jan. 1999	Dec. 2008	4,322	4.0	118.1

Note: The numbers provided are for the monthly estimation sample. The maximum observation period for an individual is 120 months (10 years).

Table I.2: *Sample means of selected variables in the initial and final wave*

Variable	First monthly wave	Last monthly wave	Cross-wave average
female	0.49	0.49	0.49
migrant	0.05	0.04	0.04
age	18.0	27.9	22.9
years of education	9.7	13.4	12.2
single, without children	0.03	0.43	0.24
single, with children	0.20	0.11	0.16
couples, without children	0.00	0.05	0.02
couples, with children	0.77	0.41	0.58
child $\leq$ 5 years	0.06	0.23	0.11
household size	4.0	2.4	3.1
local unemployment rate	6.7	3.1	4.2

Note: The table gives sample means of the listed variables in the first wave (Januaries 1993-1999) and last monthly wave (Decembers 2002-2008) observed for each cohort, and averaged across all monthly waves.

Table I.3: *Sample means of selected variables*

Variable	Monthly data		Annualized data		Biennialized data	
	SEA=0	SEA=1	SEA=0	SEA=1	SEA=0	SEA=1
female	0.49	0.50	0.49	0.47	0.49	0.48
migrant	0.04	0.11	0.04	0.11	0.04	0.11
age	23.4	22.8	22.9	22.5	22.3	22.0
years of education	12.5	10.1	12.3	10.1	12.2	10.0
single, without children	0.26	0.43	0.21	0.36	0.18	0.46
single, with children	0.15	0.28	0.16	0.31	0.17	0.31
couples, without children	0.02	0.01	0.02	0.01	0.01	0.01
couples, with children	0.57	0.28	0.61	0.32	0.64	0.33
child $\leq$ 5 years	0.11	0.15	0.10	0.14	0.09	0.14
household size	3.0	2.4	3.2	2.6	3.3	2.6
local unemployment rate	4.0	4.4	3.9	4.3	4.0	4.4

Note: The table gives sample means of the listed variables for social assistance non-recipients (SEA=0) and recipients (SEA=1), respectively, averaged across all time periods. In the annualized and biennialized data, the 'point-in-time'-approach has been used for constructing the benefit variable (see explanation in the main text). Descriptive statistics calculated using the 'benefit year'-approach are likely to be slightly different.

Table I.4: *Spell durations*

Spell duration in months	Spells on benefits		Spells off benefits	
	# of spells	Share in %	# of spells	Share in %
1	13,930	43.5	10,326	16.7
2	6,254	19.5	3,586	5.8
3	3,543	11.1	2,246	3.6
4	2,058	6.4	1,679	2.7
5	1,385	4.3	1,392	2.3
6	965	3.0	1,144	1.8
7	746	2.3	885	1.4
8	534	1.7	787	1.3
9	419	1.3	772	1.3
10	315	1.0	660	1.1
11	289	0.9	573	0.9
12 and longer	1,567	4.9	37,844	61.1
all spells	32,005	100.0	61,894	100.0

Note: The table gives the length of individuals' spells on and off benefits in months. All spells observed in the monthly estimation sample are considered in the calculations. Left- and right-censored spells are included with their censored spell lengths. Median and mean spell durations for spells on benefits are 2 months and 3.4 months, respectively. For spells off benefits, median and mean spell durations are 36 and 57 months, respectively.

Table I.5: *Individuals' spell count*

Spell count	# of spells	Share in %
0	23,924	76.8
1	2,040	6.6
2	1,184	3.8
3	815	2.6
4	572	1.8
5	483	1.6
6	423	1.4
7	310	1.0
8	274	0.9
9	217	0.7
10	187	0.6
11 and more	705	2.3
total	31,134	100.0

Note: The table gives the distribution of the number of separate benefit spells per individual. All spells observed in the monthly estimation sample - including left- and right-censored spells - are considered in the calculations. The median and mean spell count for individuals in the sample are 0 spells and 1.0 spells, respectively.

Appendix J

Coefficient estimates

dependent variable: SEA_t	MONTHLY MODEL		ANNUAL MODEL	
$SEA_{(t-1)}$	1.845***	(0.006)	0.824***	(0.022)
individual characteristics				
female	-0.179***	(0.018)	-0.125***	(0.023)
immigrant	0.376***	(0.038)	0.271***	(0.044)
age	-0.051**	(0.023)	0.188***	(0.065)
age^2	0.077	(0.048)	-0.390***	(0.140)
years of education	0.011	(0.007)	0.121***	(0.012)
years of education^2	-0.003***	(0.000)	-0.013***	(0.001)
household characteristics				
single, with children	-0.066***	(0.011)	0.088***	(0.033)
couple, no children	-0.192***	(0.032)	-0.177*	(0.106)
couple, with children	-0.213***	(0.014)	-0.055	(0.045)
child aged 0-5 years	-0.114***	(0.010)	-0.154***	(0.030)
household size	0.042***	(0.004)	0.048***	(0.013)
spouse characteristics				
immigrant	0.014	(0.051)	-0.049	(0.128)
age	-0.005	(0.005)	-0.006	(0.012)
age^2	0.003	(0.011)	0.010	(0.030)
years of education	0.052***	(0.013)	0.048	(0.033)
years of education^2	-0.005***	(0.001)	-0.005**	(0.002)
year dummies				
1995	0.174***	(0.061)	0.015	(0.106)
1996	0.192***	(0.057)	0.085	(0.099)
1997	0.187***	(0.054)	0.059	(0.095)
1998	0.148***	(0.050)	-0.055	(0.092)
1999	0.079*	(0.047)	-0.057	(0.090)
2000	0.075*	(0.044)	-0.038	(0.087)
2001	0.070*	(0.041)	-0.031	(0.083)
2002	0.084**	(0.038)	-0.007	(0.081)
2003	0.087**	(0.035)	0.043	(0.078)
2004	0.101***	(0.032)	0.074	(0.076)
2005	0.079***	(0.030)	0.037	(0.076)
2006	0.081***	(0.029)	0.070	(0.075)
2007	0.067**	(0.028)	0.001	(0.076)
2008	-0.013	(0.028)	-0.113	(0.081)
Wooldridge controls				
SEA_0	1.451***	(0.034)	0.995***	(0.037)
single, with children	-0.206***	(0.044)	-0.407***	(0.060)
couple, no children	-0.941***	(0.117)	-1.206***	(0.192)
couple, with children	-0.977***	(0.054)	-1.007***	(0.078)
child aged 0-5 years	1.161***	(0.039)	0.783***	(0.054)
household size	-0.042***	(0.014)	-0.036*	(0.021)
local unemployment rate	4.456***	(0.770)	3.203***	(1.200)
local unemployment rate	2.272***	(0.263)	2.772***	(0.812)
constant	-2.101***	(0.279)	-4.244***	(0.737)
σ_a	0.999***	(0.010)	0.782***	(0.016)
ρ	0.499***	(0.005)	0.379***	(0.010)
log Likelihood	-173,477.470		-23,624.319	
# of observations	3,279,708		272,878	
# of individuals	30,899		30,899	

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Table J.1 – continued from previous page –

* p<0.10, ** p<0.05, *** p<0.01
Note: Standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits in a given month for the monthly model and in December of the given year for the annual model. 'Wooldridge controls' include the outcome variable in the initial period SEA_0 and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years.

Table J.1: *The standard Markov specification, 'point-in-time' definition: monthly and annual model*

dependent variable: SEA_t	ANNUAL MODEL		BIENNIAL MODEL	
$SEA_{(t-1)}$	0.854***	(0.022)	0.617	(0.041)
individual characteristics				
female	-0.119***	(0.022)	-0.148	(0.028)
immigrant	0.266***	(0.042)	0.310	(0.053)
age	0.199***	(0.064)	0.375	(0.115)
age^2	-0.412***	(0.140)	-0.797	(0.256)
years of education	0.120***	(0.011)	0.153	(0.015)
years of education^2	-0.013***	(0.001)	-0.016	(0.001)
household characteristics				
single, with children	0.090***	(0.033)	0.195***	(0.052)
couple, no children	-0.176*	(0.106)	-0.132	(0.160)
couple, with children	-0.052	(0.045)	0.071	(0.071)
child aged 0-5 years	-0.158***	(0.030)	0.029	(0.047)
household size	0.047***	(0.013)	0.018	(0.020)
spouse characteristics				
immigrant	-0.040	(0.126)	-0.369*	(0.190)
age	-0.006	(0.012)	0.013	(0.018)
age^2	0.011	(0.029)	-0.014	(0.044)
years of education	0.052	(0.032)	0.031	(0.048)
years of education^2	-0.005**	(0.002)	-0.004	(0.003)
year dummies				
1995	0.066	(0.052)		
1996	0.043	(0.052)		
1997	-0.075	(0.055)	-0.108*	(0.056)
1998	-0.079	(0.059)		
1999	-0.061	(0.062)	-0.151**	(0.067)
2000	-0.052	(0.061)		
2001	-0.027	(0.063)	-0.097	(0.069)
2002	0.020	(0.068)		
2003	0.051	(0.069)	0.003	(0.080)
2004	0.017	(0.072)		
2005	0.050	(0.076)	-0.012	(0.082)
2006	-0.021	(0.082)		
2007	-0.131	(0.092)	-0.214**	(0.098)
2008	0.135	(0.101)		
Wooldridge controls				
SEA_0	0.949***	(0.036)	0.891***	(0.051)
single, with children	-0.409***	(0.059)	-0.550***	(0.079)
couple, no children	-1.200***	(0.189)	-1.017***	(0.247)
couple, with children	-1.001***	(0.077)	-1.098***	(0.104)
child aged 0-5 years	0.764***	(0.053)	0.488***	(0.072)
household size	-0.031	(0.021)	0.008	(0.028)
local unempl. rate	3.265***	(1.187)	3.402**	(1.546)
local unempl. rate	2.664***	(0.810)	1.884	(1.234)
constant	-4.331***	(0.726)	-6.181***	(1.268)
σ_a	0.759***	(0.016)	0.757***	(0.030)
ρ	0.366***	(0.010)	0.364***	(0.018)
log Likelihood	-23,648.885		-11,295.134	
# of observations	272,641		121,027	
# of individuals	30,719		30,719	

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Table J.2 – continued from previous page –

* p<0.10, ** p<0.05, *** p<0.01
note: standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits in December of the given year for both the annual and biennial model. 'Wooldridge controls' include the outcome variable in the initial period SEA_0 and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years.

Table J.2: *The standard Markov specification, 'point-in-time' definition: annual and biennial model*

dependent variable: SEA_t	ANNUAL MODEL		BIENNIAL MODEL	
$SEA_{(t-1)}$	1.226***	(0.016)	0.689***	(0.030)
individual characteristics				
female	-0.085***	(0.017)	-0.065***	(0.023)
immigrant	0.227***	(0.035)	0.205***	(0.048)
age	0.103**	(0.058)	-0.107	(0.120)
age^2	-0.237**	(0.126)	0.221	(0.262)
years of education	0.085***	(0.010)	0.135***	(0.013)
years of education^2	-0.010***	(0.001)	-0.015***	(0.001)
household characteristics				
single, with children	0.129***	(0.027)	0.104**	(0.045)
couple, no children	-0.080	(0.076)	0.057	(0.120)
couple, with children	0.018	(0.037)	0.012	(0.060)
child aged 0-5 years	-0.110***	(0.024)	-0.001	(0.040)
household size	0.031***	(0.011)	0.045**	(0.017)
spouse characteristics				
immigrant	-0.161	(0.100)	-0.148	(0.146)
age	-0.000	(0.010)	0.002	(0.014)
age^2	0.003	(0.024)	0.003	(0.035)
years of education	0.073***	(0.026)	0.093**	(0.038)
years of education^2	-0.007***	(0.002)	-0.009***	(0.002)
year dummies				
1995	0.041	(0.046)		
1996	0.051	(0.040)		
1997	0.033	(0.044)	-0.151***	(0.045)
1998	-0.081*	(0.046)		
1999	-0.032	(0.050)	-0.131**	(0.054)
2000	-0.044	(0.047)		
2001	-0.023	(0.049)	-0.122**	(0.053)
2002	-0.002	(0.053)		
2003	0.047	(0.054)	-0.079	(0.059)
2004	-0.035	(0.056)		
2005	-0.028	(0.060)	-0.146**	(0.066)
2006	-0.046	(0.063)		
2007	-0.099	(0.078)	-0.195**	(0.085)
2008	0.140*	(0.078)		
Wooldridge controls				
SEA_0	0.896***	(0.024)	1.010***	(0.040)
single, with children	-0.340***	(0.047)	-0.151**	(0.066)
couple, no children	-0.977***	(0.136)	-1.023***	(0.190)
couple, with children	-0.866***	(0.060)	-0.661***	(0.087)
child aged 0-5 years	0.659***	(0.041)	0.423***	(0.059)
household size	-0.008	(0.016)	-0.021	(0.024)
local unempl. rate	2.475***	(0.920)	2.886**	(1.297)
local unempl. rate	3.474***	(0.645)	1.699*	(1.005)
constant	-2.749***	(0.650)	-0.285	(1.355)
σ_a	0.691***	(0.013)	0.793***	(0.024)
ρ	0.323***	(0.008)	0.386***	(0.014)
log Likelihood	-38,376.555		-18,199.340	
# of observations	246,753		108,089	
# of individuals	30,576		30,576	

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Table J.3 – continued from previous page –

* p<0.10, ** p<0.05, *** p<0.01
note: standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits at any time during the year / during every second year for the annual and biennial model, respectively. 'Wooldridge controls' include the outcome variable in the initial period SEA_0 and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years.

Table J.3: *The standard Markov specification, 'benefit year' definition: annual and biennial model*

dependent variable: SEA_t	MONTHLY MODEL WITH HIGHER-ORDER LAGS	
$SEA_{(t-1)}$	1.402***	(0.007)
higher-order lags		
$SEA_{(t-2)}$	0.562***	(0.008)
$SEA_{(t-3)}$	0.247***	(0.008)
$SEA_{(t-4)}$	0.157***	(0.008)
$SEA_{(t-5)}$	0.082***	(0.009)
$SEA_{(t-6)}$	0.090***	(0.009)
$SEA_{(t-7)}$	0.047***	(0.009)
$SEA_{(t-8)}$	0.035***	(0.009)
$SEA_{(t-9)}$	0.035***	(0.009)
$SEA_{(t-10)}$	0.024***	(0.009)
$SEA_{(t-11)}$	0.069***	(0.009)
$SEA_{(t-12)}$	0.106***	(0.008)
individual characteristics		
female	-0.110***	(0.014)
immigrant	0.183***	(0.029)
age	-0.046**	(0.023)
age^2	0.079	(0.049)
years of education	0.041***	(0.006)
years of education^2	-0.005***	(0.000)
household characteristics		
single, with children	-0.025**	(0.011)
couple, no children	-0.140***	(0.033)
couple, with children	-0.165***	(0.015)
child aged 0-5 years	-0.075***	(0.011)
household size	0.048***	(0.004)
spouse characteristics		
immigrant	0.040	(0.051)
age	-0.006	(0.005)
age^2	0.011	(0.011)
years of education	0.052***	(0.013)
years of education^2	-0.005***	(0.001)
year dummies		
1995	0.107**	(0.052)
1996	0.119**	(0.048)
1997	0.115**	(0.046)
1998	0.088**	(0.043)
1999	0.051	(0.041)
2000	0.063	(0.039)
2001	0.056	(0.037)
2002	0.069**	(0.035)
2003	0.071**	(0.032)
2004	0.079**	(0.031)
2005	0.060**	(0.030)
2006	0.058**	(0.029)
2007	0.041	(0.028)
2008	-0.022	(0.029)
Wooldridge controls		
SEA_0	0.275***	(0.036)
SEA_1	0.229***	(0.039)

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Table J.4 – continued from previous page –

	MONTHLY MODEL WITH HIGHER-ORDER LAGS	
SEA_2	0.108***	(0.040)
SEA_3	0.087**	(0.038)
SEA_4	0.158***	(0.036)
SEA_5	0.220***	(0.037)
SEA_6	0.133***	(0.039)
SEA_7	0.006	(0.042)
SEA_8	0.099**	(0.044)
SEA_9	0.177***	(0.045)
SEA_{10}	-0.016	(0.048)
SEA_{11}	-0.048	(0.047)
single, with children	-0.137***	(0.034)
couple, no children	-0.689***	(0.092)
couple, with children	-0.641***	(0.043)
child aged 0-5 years	0.760***	(0.030)
household size	-0.042***	(0.011)
local unemployment rate	3.098***	(0.616)
local unemployment rate	2.241***	(0.266)
constant	-2.208***	(0.275)
σ_a	0.702***	(0.008)
ρ	0.330***	(0.005)
log Likelihood	-163,515.291	
# of observations	3,279,708	
# of individuals	30,899	

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

note: standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits in a given month for the monthly model and in December of the given year for the annual model. 'Wooldridge controls' include the outcome variable in the initial twelve periods SEA_0 to SEA_{11} and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years.

Table J.4: *The higher-order Markov specification: monthly model*

dependent variable: SEA_t	MONTHLY MODEL			
	entry equation		persistence equation	
individual characteristics				
female	-0.179***	(0.018)	-0.038*	(0.020)
immigrant	0.328***	(0.037)	0.248***	(0.034)
age	0.155***	(0.027)	-0.423***	(0.045)
age^2	-0.376***	(0.058)	0.910***	(0.096)
years of education	0.022***	(0.008)	-0.003	(0.008)
years of education^2	-0.004***	(0.000)	-0.001***	(0.001)
household characteristics				
single, with children	-0.090***	(0.013)	-0.044**	(0.021)
couple, no children	-0.233***	(0.038)	0.010	(0.066)
couple, with children	-0.288***	(0.017)	-0.019	(0.028)
child aged 0-5 years	-0.113***	(0.013)	-0.123***	(0.020)
household size	0.060***	(0.005)	-0.005	(0.008)
spouse characteristics				
immigrant	-0.034	(0.060)	0.062	(0.088)
age	-0.003	(0.005)	-0.006	(0.008)
age^2	0.005	(0.013)	0.007	(0.019)
years of education	0.050***	(0.015)	0.029	(0.023)
years of education^2	-0.005***	(0.001)	-0.003**	(0.002)
year dummies				
1995	0.191***	(0.066)	0.061	(0.082)
1996	0.205***	(0.061)	0.083	(0.076)
1997	0.210***	(0.058)	0.063	(0.073)
1998	0.168***	(0.055)	0.029	(0.070)
1999	0.090*	(0.052)	-0.000	(0.068)
2000	0.090**	(0.049)	-0.004	(0.066)
2001	0.078**	(0.045)	0.020	(0.063)
2002	0.116***	(0.043)	0.001	(0.060)
2003	0.117***	(0.040)	0.002	(0.058)
2004	0.141***	(0.038)	-0.011	(0.056)
2005	0.126***	(0.036)	-0.036	(0.055)
2006	0.111***	(0.035)	0.012	(0.054)
2007	0.073**	(0.034)	0.039	(0.054)
2008	-0.023	(0.036)	0.001	(0.056)
Wooldridge controls				
SEA_{11}	1.379***	(0.033)	0.751***	(0.029)
single, with children	-0.173***	(0.044)	-0.155***	(0.047)
couple, no children	-0.938***	(0.120)	-0.424***	(0.138)
couple, with children	-0.895***	(0.055)	-0.486***	(0.064)
child aged 0-5 years	1.118***	(0.039)	0.438***	(0.045)
household size	-0.059***	(0.015)	-0.006	(0.017)
local unempl. rate	4.720***	(0.783)	-0.573	(0.967)
local unempl. rate	1.981***	(0.312)	2.721***	(0.520)
constant	-4.347***	(0.330)	4.802***	(0.525)
σ_a	0.947***	(0.010)	0.590***	(0.012)
ρ	0.473***	(0.005)	0.258***	(0.008)
τ		0.714***	(0.014)	
log Likelihood		-170,903.910		
# of observations		3,279,708		

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Table J.5 – continued from previous page –

	MONTHLY MODEL	
	entry equation	persistence equation
# of individuals	30,899	

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

Note: Standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits in a given month. 'Wooldridge controls' include the outcome variable in the initial period SEA_{11} and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years. Parameters σ_a in the entry and the persistence equation are the standard deviations of the individual-specific random effects α_i^0 and α_i^1 as specified in Equations (4.20)-(4.21), respectively, τ is their correlation.

Table J.5: *The standard DRCP specification: monthly model*

dependent variable: SEA_t	ANNUAL MODEL entry equation		persistence equation	
individual characteristics				
female	-0.128***	(0.024)	-0.078*	(0.046)
immigrant	0.256***	(0.047)	0.229***	(0.071)
age	0.406***	(0.074)	-0.573***	(0.155)
age^2	-0.865***	(0.161)	1.269***	(0.334)
years of education	0.132***	(0.013)	0.062***	(0.019)
years of education^2	-0.014***	(0.001)	-0.008***	(0.001)
household characteristics				
single, with children	0.089**	(0.039)	0.057	(0.071)
couple, no children	-0.247*	(0.127)	0.143	(0.221)
couple, with children	-0.065	(0.052)	-0.039	(0.100)
child aged 0-5 years	-0.157***	(0.034)	-0.150**	(0.065)
household size	0.063***	(0.015)	0.007	(0.028)
spouse characteristics				
immigrant	-0.049	(0.141)	-0.121	(0.254)
age	0.004	(0.013)	-0.023	(0.024)
age^2	-0.005	(0.032)	0.045	(0.061)
years of education	0.029	(0.036)	0.067	(0.066)
years of education^2	-0.004*	(0.002)	-0.004	(0.004)
year dummies				
1995	-0.025	(0.115)	0.031	(0.232)
1996	0.086	(0.107)	0.021	(0.214)
1997	0.039	(0.103)	0.099	(0.204)
1998	-0.086	(0.100)	0.033	(0.198)
1999	-0.104	(0.098)	0.120	(0.195)
2000	-0.100	(0.096)	0.197	(0.192)
2001	-0.087	(0.092)	0.195	(0.185)
2002	-0.071	(0.089)	0.247	(0.181)
2003	-0.031	(0.087)	0.338*	(0.177)
2004	0.009	(0.085)	0.340*	(0.174)
2005	-0.034	(0.085)	0.320*	(0.173)
2006	-0.002	(0.085)	0.325*	(0.172)
2007	-0.039	(0.086)	0.150	(0.172)
2008	-0.176*	(0.094)	0.090	(0.181)
Wooldridge controls				
SEA_{11}	0.949***	(0.041)	0.695***	(0.058)
single, with children	-0.402***	(0.065)	-0.330***	(0.117)
couple, no children	-1.215***	(0.203)	-1.117***	(0.407)
couple, with children	-1.008***	(0.085)	-0.706***	(0.162)
child aged 0-5 years	0.849***	(0.058)	0.194*	(0.106)
household size	-0.054**	(0.023)	0.023	(0.044)
local unempl. rate	3.480***	(1.304)	-0.972	(2.640)
local unempl. rate	2.743***	(0.914)	3.014	(1.891)
constant	-6.704***	(0.843)	5.583***	(1.757)
σ_a	0.791***	(0.018)	0.654***	(0.032)
ρ	0.385***	(0.011)	0.300***	(0.021)
τ		0.700***	(0.048)	
log Likelihood		-23,511.214		
# of observations		272,878		

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Table J.6 – continued from previous page –

	ANNUAL MODEL	
	entry equation	persistence equation
# of individuals	30,899	

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

Note: Standard errors in parentheses. The dependent variable SEA_t measures receipt of social assistance benefits in December of the given year. 'Wooldridge controls' include the outcome variable in the initial period SEA_{11} and longitudinal averages of the remaining variables listed. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years. Parameters σ_a in the entry and the persistence equation are the standard deviations of the individual-specific random effects α_i^0 and α_i^1 as specified in Equations (4.20)-(4.21), respectively, τ is their correlation.

Table J.6: *The standard DRCP specification: annual model*

dependent variable: SEA_t	MONTHLY MODEL			
	entry equation		persistence equation	
individual characteristics				
female	-0.058***	(0.010)	0.000	(0.015)
immigrant	0.084***	(0.019)	0.096***	(0.025)
age	-0.055**	(0.028)	-0.350***	(0.046)
age^2	0.058	(0.059)	0.732***	(0.097)
years of education	0.046***	(0.005)	0.012*	(0.007)
years of education^2	-0.005***	(0.000)	-0.002***	(0.000)
duration variables				
d_2	-0.528***	(0.014)	0.188***	(0.013)
d_3	-0.700***	(0.016)	0.284***	(0.016)
d_4	-0.803***	(0.017)	0.396***	(0.019)
d_5	-0.889***	(0.018)	0.459***	(0.022)
d_6	-0.980***	(0.020)	0.502***	(0.024)
d_7	-1.030***	(0.021)	0.527***	(0.027)
d_8	-1.051***	(0.022)	0.575***	(0.031)
d_9	-1.006***	(0.023)	0.604***	(0.034)
d_{10}	-1.050***	(0.024)	0.656***	(0.037)
d_{11}	-1.061***	(0.025)	0.583***	(0.039)
d_{12}	-1.429***	(0.011)	0.789***	(0.020)
occurrence variables				
o_1	0.368***	(0.014)	0.051***	(0.017)
o_2	0.418***	(0.018)	0.033*	(0.019)
o_3	0.419***	(0.020)	0.041*	(0.022)
o_4	0.453***	(0.023)	0.046*	(0.024)
o_5	0.448***	(0.025)	0.079***	(0.026)
o_6	0.442***	(0.028)	0.059**	(0.029)
o_7	0.452***	(0.030)	0.096***	(0.031)
o_8	0.444***	(0.033)	0.070**	(0.034)
o_9	0.477***	(0.036)	0.054	(0.037)
o_{10}	0.456***	(0.038)	0.066	(0.041)
o_{11}	0.489***	(0.037)	0.083**	(0.036)
household characteristics				
single, with children	-0.059***	(0.013)	-0.037*	(0.020)
couple, no children	-0.134***	(0.038)	-0.022	(0.063)
couple, with children	-0.186***	(0.017)	-0.027	(0.027)
child aged 0-5 years	-0.078***	(0.013)	-0.092***	(0.020)
household size	0.062***	(0.005)	0.005	(0.008)
spouse characteristics				
immigrant	0.025	(0.050)	0.055	(0.075)
age	-0.004	(0.005)	-0.006	(0.007)
age^2	0.010	(0.011)	0.010	(0.017)
years of education	0.043***	(0.013)	0.027	(0.020)
years of education^2	-0.004***	(0.001)	-0.003**	(0.001)
year dummies				
1995	0.071	(0.045)	0.016	(0.070)
1996	0.065	(0.042)	0.042	(0.065)
1997	0.074*	(0.041)	0.031	(0.062)
1998	0.046	(0.039)	0.011	(0.061)
1999	0.011	(0.039)	-0.007	(0.060)

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Table J.7 – continued from previous page –

	MONTHLY MODEL			
	entry equation		persistence equation	
2000	0.030	(0.038)	0.000	(0.058)
2001	0.027	(0.036)	0.016	(0.056)
2002	0.059*	(0.035)	0.005	(0.055)
2003	0.060*	(0.034)	0.015	(0.053)
2004	0.086**	(0.034)	-0.007	(0.053)
2005	0.072**	(0.033)	-0.022	(0.052)
2006	0.058*	(0.033)	0.020	(0.052)
2007	0.028	(0.034)	0.040	(0.052)
2008	-0.039	(0.036)	0.003	(0.055)
Wooldridge controls				
SEA_{11}	0.010	(0.039)	-0.015	(0.042)
single, with children	-0.017	(0.026)	-0.069*	(0.037)
couple, no children	-0.454***	(0.070)	-0.152	(0.108)
couple, with children	-0.277***	(0.033)	-0.199***	(0.051)
child, aged 0-5 years	0.382***	(0.024)	0.141***	(0.035)
household size	-0.057***	(0.009)	-0.008	(0.014)
local unempl. rate	1.391***	(0.503)	-1.677**	(0.797)
$d_{2,11}$	-0.011	(0.043)	-0.047	(0.048)
$d_{2,11} * SEA_{11}$	0.055	(0.060)	0.088	(0.065)
$d_{3,11}$	-0.075*	(0.041)	-0.058	(0.047)
$d_{3,11} * SEA_{11}$	0.209***	(0.064)	0.075	(0.069)
$d_{4,11}$	-0.116***	(0.039)	-0.108**	(0.045)
$d_{4,11} * SEA_{11}$	0.289***	(0.064)	0.293***	(0.067)
$d_{5,11}$	-0.048	(0.043)	-0.083*	(0.050)
$d_{5,11} * SEA_{11}$	0.117	(0.077)	0.263***	(0.081)
$d_{6,11}$	-0.154***	(0.052)	-0.221***	(0.066)
$d_{6,11} * SEA_{11}$	0.234***	(0.082)	0.327***	(0.092)
$d_{7,11}$	-0.172***	(0.066)	-0.216***	(0.084)
$d_{7,11} * SEA_{11}$	0.185*	(0.098)	0.329***	(0.110)
$d_{8,11}$	-0.113*	(0.066)	-0.016	(0.079)
$d_{8,11} * SEA_{11}$	0.330***	(0.108)	0.219*	(0.115)
$d_{9,11}$	-0.085	(0.071)	-0.285***	(0.092)
$d_{9,11} * SEA_{11}$	0.360***	(0.119)	0.546***	(0.129)
$d_{10,11}$	-0.128	(0.093)	-0.224*	(0.126)
$d_{10,11} * SEA_{11}$	0.267**	(0.125)	0.519***	(0.151)
$d_{11,11}$	-0.311***	(0.111)	-0.087	(0.156)
$d_{11,11} * SEA_{11}$	0.553***	(0.138)	0.239	(0.176)
$d_{12,11}$	-0.268***	(0.043)	-0.227***	(0.045)
$d_{12,11} * SEA_{11}$	0.523***	(0.059)	0.579***	(0.058)
$o_{1,11}$	0.022	(0.031)	0.021	(0.032)
$o_{2,11}$	0.089**	(0.036)	0.061	(0.038)
$o_{3,11}$	0.161***	(0.050)	0.054	(0.052)
$o_{4,11}$	0.185*	(0.097)	0.003	(0.102)
local unempl. rate	1.613***	(0.311)	2.694***	(0.501)
constant	-0.114	(0.324)	4.283***	(0.531)
σ_a	0.354***	(0.010)	0.319***	(0.009)
ρ	0.111***	(0.006)	0.092***	(0.005)
τ		0.784***	(0.026)	
log Likelihood		-158,370.707		

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Table J.7 – continued from previous page –

	MONTHLY MODEL	
	entry equation	persistence equation
# of observations	3,279,708	
# of individuals	30,899	

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

Note: standard errors in parentheses. The dependent variable SEA_t measures receipt of SEA benefits in a given month. The variables labelled d_k and o_k are dummy variables measuring the duration of the ongoing spell and the number of previous spells, respectively. 'Wooldridge controls' include the outcome variable in the initial period SEA_{11} and longitudinal averages of the time-varying variables, as well as the values of the duration dummies and occurrence dummies in the initial period ($d_{k,11}$ and $o_{k,11}$) and their interactions with the outcome variable in the initial period (SEA_{11}). For further information, see Table G.1 in Appendix G. age^2 has been divided by 100 to produce suitably-sized coefficient estimates. The relevant base categories are males, natives, singles without children, and households without a child aged below 5 years. Parameters σ_a in the entry and the persistence equation are the standard deviations of the individual-specific random effects α_i^0 and α_i^1 as specified in Equations (4.23)-(4.24), respectively, τ is their correlation.

Table J.7: *The extended DRCP specification with duration and occurrence dependence: monthly model*