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Abstract:
Dynamic discrete-choice models have been an important tool in studies of state dependence in benefit receipt. An assumption of such models is that benefit receipt sequences follow a conditional Markov process. This property has implications for how estimated period-to-period benefit transition probabilities should relate when receipt processes are aggregated over time. This paper assesses whether the conditional Markov property holds in welfare benefit receipt dynamics using high-quality monthly data from Norwegian administrative records. We find that the standard conditional Markov model is seriously misspecified. Estimated state dependence is affected substantially by the chosen time unit of analysis, with the average treatment effect of past benefit receipt increasing with the level of aggregation. The model can be improved considerably by permitting richer types of benefit dynamics: Allowing for differences between the processes for entries and persistence we find important disparities especially in terms of the effects of permanent unobserved characteristics. Extending the model further, we obtain strong evidence for duration and occurrence dependence in benefit receipt. Based on our preferred model, the month-to-month persistence probability in benefit receipt for a first-time entrant is 37 percentage points higher than the entry rate of an individual without previous benefit receipt. Over a 12-month period, the average treatment effect is about 5 percentage points.

Keywords: Time aggregation, Markov property, State dependence, Welfare dynamics.

JEL classification: I38, J60, J64, C23, C41

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Sammendrag

Dynamiske diskretvalgmodeller har vært et viktig verktøy i studier av tilstandsavhengighet i stønadsmottak. En forutsetning i slike modeller er at sekvensene av stønadsmottak følger en såkalt betinget Markov prosess. Denne egenskapen har implikasjoner for hvordan de estimerte overgangssannsynligheter fra en periode til den neste bør forholde seg når den dynamiske prosessen som beskriver stønadsmottak aggregeres over tid. Dette arbeidet undersøker om den betingede Markov egenskapen er oppfylt i dynamikk av sosialhjelpsmottak ved å bruke månedlige data innhentet fra en rekke norske administrative registre. Vi finner at standardmodell brukt i litteraturen er alvorlig feilspesifisert. Estimert tilstandsavhengighet påvirkes av den valgte tidsanalyseenheten i betydelig grad, og den gjennomsnittlige effekten av tidligere stønadsmottak øker med graden av tidsaggregering. Modellen kan forbedres betydelig ved å tillate en mer fleksibel modellering av stønadsdynamikk. Når vi tillater prosessene som beskriver henholdsvis innganger til og persistens i sosialhjelpsmottak å variere på en fleksibel måte oppdager vi viktige forskjeller, særlig i effektene av de permanente uobserverte kjennetegn. Ved å utvide modellen ytterligere finner vi både såkalt varighet- og hendelsesavhengighet i sosialhjelpsmottak. Basert på vår mest fleksible og foretrukne modell, finner vi at den månedelige deltakelsessannsynligheten i stønadsmottak for en tidligere støndadsmottaker er 37 prosentpoeng høyere enn sannsynligheten for at en person uten tidligere stønadshistorie mottar sosialhjelp. Over en tolv måneders periode, er den gjennomsnittlige effekten ca. 5 prosentpoeng.

1 Introduction

An established finding in the literature on welfare benefit dynamics is that rates of persistence in individuals' benefit receipt from one period to the next are very high. Given typically low entry rates into benefit receipt, a main focus of recent studies has therefore been to assess the driving forces of this state dependence.[1] Heckman (1978, 1981a) distinguishes two sources of state dependence. First, individuals differ in terms of their personal characteristics. Persistent individual characteristics such as low education or serious health problems that raise the probability of benefit receipt will induce persistence in welfare. A failure to appropriately control for persistent heterogeneity in such characteristics across individuals will lead to *spurious* state dependence. Second, past benefit receipt might itself affect the probability of receiving benefits today. For instance, previous receipt of welfare benefit payments might reduce information costs or the perceived stigma from receiving benefit payments and thus make future benefit receipt more likely. Alternatively, potential employers might interpret past benefit receipt as a negative signal about an applicant's unobserved labour productivity, which would make self-sufficiency less likely. This direct, positive effect of past benefit receipt on the probability of future benefit receipt is referred to as *genuine* or *structural* state dependence.

Two related approaches have been employed in the empirical literature to study the sources of persistence in labour market histories. Duration models analyse the processes leading up to an exit from a labour market state, for instance welfare 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).

The recent literature on welfare benefit dynamics has primarily relied on estimation of dynamic discrete-choice models to study state dependence in benefit receipt.[2] Due to the limited availability of individual-level data on welfare benefit receipt at shorter observation intervals much of the evidence on state dependence in welfare benefit receipt is based on annual data

[1] Our calculations for Norway for instance show that month-to-month entry rates of working-age individuals to Social Economic Assistance were around or below 0.5% for most of the last two decades while persistence rates in benefit receipt averaged around 75% (see Figure 1).

[2] Similar models have also been estimated to study the dynamics of employment (Heckman, 1981a; Hyslop, 1999), unemployment (Arulampalam, Gregg & Gregory, 2001; Gregg, 2001; Biewen & Steffes, 2010), or low-income/poverty (Stewart & Swaffield, 1999; Cappellari & Jenkins, 2004; Stewart, 2007; Cappellari & Jenkins, 2008; Biewen, 2009). More recent studies analyse individual labour market histories using dynamic multinomial-choice models (Uhlendorff, 2006; Prowse, 2012).

that come either from administrative sources (see Hansen & Lofstrom (2008, 2011) or Andrén & Andrén (2013) for Sweden) or from household survey data (Hansen, Lofstrom & Zhang (2006) for Canada, Cappellari & Jenkins (2009) for Britain, and Königs (2013a) or Wunder & Riphahn (2013) for Germany). Notable exceptions are studies of the dynamics of welfare benefit receipt in the U.S. by Chay, Hoynes & Hyslop (1999) and Chay & Hyslop (2000) that are based on four years of monthly administrative data from California and eight waves of four-monthly data from the Survey of Income and Program Participation, respectively. A small number of studies have moreover used monthly data and an event-history framework to analyse welfare spell durations for the U.S. (Blank, 1989; Sandefur & Cook, 1998), Norway (Dahl & Lorentzen, 2003b), and Sweden (Bäckman & Bergmark, 2011; Mood, 2013), each finding evidence for duration dependence in welfare benefit receipt.

A key assumption typically made in 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, spell durations, and the number of previous welfare spells are assumed not to add any predictive power to the model.[3] 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 specifications. If the assumed Markov property does not hold, the results obtained from such an 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 welfare 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.[4]

In this paper, 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

[3]To our knowledge, the only 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.

[4]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 disucssion in Section 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.)

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 it 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 of time unit and is therefore evidence of model misspecification.

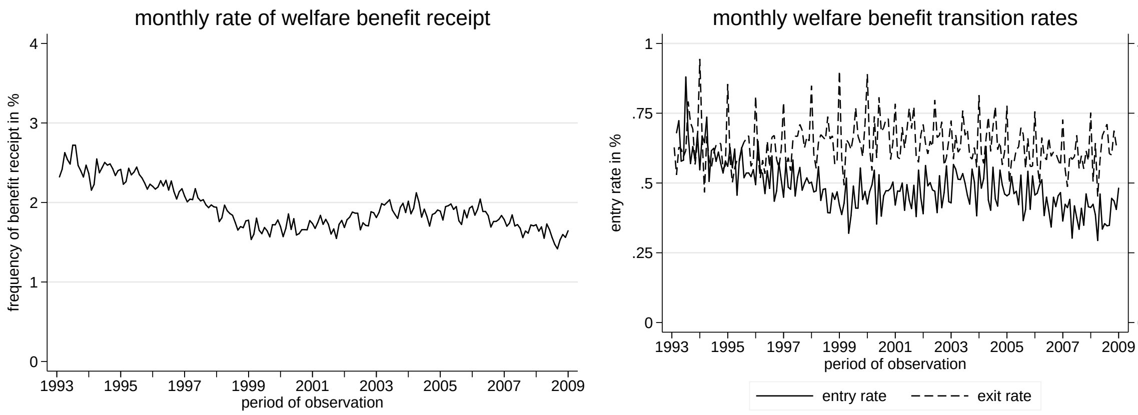
We apply this test to assess the validity of the conditional Markov assumption for a standard dynamic random-effects probit model of welfare benefit receipt in Norway. Using administrative data for six cohorts of young adults over the years 1993-2008, we find that estimates of state dependence derived 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), the two approaches that have been frequently used in the existing work on welfare 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 welfare 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 treatment 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 paper is structured as follows: In the next section, we provide a short introduction to the institutional framework of welfare benefits in Norway and present the data used in our study. Section 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 then provides empirical evidence on the performance of a first-order Markov model of welfare benefit dynamics in Norway. In Section 5, we present results from the 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 6 concludes.

2 Institutional Background and Data

The primary welfare benefit in Norway is Social Economic Assistance (SEA, *Økonomisk sosialhjelp*), which is regulated by the Social Services Act (*Loven om sosiale tjenester*).[5] 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 total household income is taken into consideration when eligibility is determined.[6] 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.



Note: Calculations based on a 5% random sample from the working-age population (aged 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.

Figure 1: Rate of welfare benefit receipt and benefit transition rates

Figure 1 presents first descriptive evidence on the dynamics of SEA benefit receipt in Norway for the period from January 1993 to December 2008. The left panel plots the development of the monthly rate of welfare 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 right panel of Figure 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%.

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

[6]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.

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 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 out-migration 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 1998. All individuals belonging to one of these six cohorts are then followed from the month in which they turn 18 for a period of up to eleven years. After those eleven years, they are dropped from the sample. Individuals may leave the sample before through out-migration or death. We then use the first twelve months of an individual's 132-month observation period to construct a benefit receipt history that we use for conditioning in the more complex models presented in Section 5. Each individual thus stays in the estimation sample for 10 years (120 months) from the month of January when turning 19 to the December just before turning 29 years of age.

Our main motivation for using these non-standard sample selection criteria is to guarantee that we observe all individuals from the beginning of their 'welfare careers'. This will be the case as individuals in Norway are generally not entitled to receive welfare 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 six years of our observation period, we make sure that we observe the first period of potential benefit receipt for each individual.[7] By dropping individuals after ten years we make sure that the resulting sample is weakly balanced.[8] 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 replicate the first part of our analysis using a 5% random sample of all 25-59 year-olds. The corresponding results are presented in the Appendix, Section A.3.

Any analysis of welfare benefit dynamics requires choices about 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 welfare 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), Königs (2013a), and Wunder & Riphahn (2013)). 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 recipient if a positive amount of benefits was received over the entire year (Hansen et al., 2006; Hansen & Lofstrom, 2008, 2011; Andrén & Andrén, 2013). We replicate both of these approaches and compare the results. Finally, as in other countries, welfare 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.

3 Time aggregation in a conditional Markov model

The empirical literature on the dynamics of welfare benefit receipt typically works with dynamic discrete-choice models, which rely on the assumption that benefit receipt dynamics follow a conditional Markov process. An important implication of this Markov property is that for a model that satisfies this property at the monthly level the property carries through to the annual level if the process is aggregated over time. In cases where the assumed Markov property is not

[7]This is important because to test for occurrence dependence in benefit receipt in Section 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 welfare careers.

[8]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 a longer period and thus at higher ages than the following cohorts. In constructing our weakly balanced panel we follow an approach used by Prowse (2012).

valid by contrast, estimation results and in particular the degree of estimated state dependence will depend on the choice of time unit. In this section, we introduce the dynamic random-effects probit (DREP) model, the standard model used for the analysis of welfare benefit dynamics, and show how the conditional transition probabilities estimated from such a model can be aggregated over time. This method will then be used in the next section to assess the time aggregation properties of a model of welfare benefit dynamics based on data from Norway.

3.1 The dynamic random-effects probit model

Let us define a binary outcome variable y_{it} such that for $y_{it} = 1$ individual i is in welfare receipt in period t. We specify the model

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

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 k observable characteristics x_{it-1}, a time-invariant individual-specific term α_i and a transitory error term ε_{it}.[9] Observables x_{it-1} may include individual and household characteristics, such as sex, age, education, family composition, and possibly partner characteristics. The individual-specific term α_i enters additively in the indicator function and captures all unobserved determinants of welfare benefit receipt that are time-invariant for an individual over the observation period. Such factors may for instance include persistent unobserved labour market ability or the individual's attitudes towards receiving welfare benefits. Its distributional assumptions are discussed below. 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.[10]

For this specification, we can thus write the conditional probability of welfare benefit receipt as

$$P(y_{it} = 1 | y_{i0}, ..., y_{it-1}, x_i, \alpha_i) = \Phi(\lambda y_{it-1} + x'_{it-1}\beta + \alpha_i), \tag{2}$$

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

The model described by Equations (1) and (2) rests on two crucial assumptions. First, we assume that the welfare dynamics are correctly described by a first-order Markov model. Thus, 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 on the dependent variable or feedback

[9] Following Cappellari & Jenkins (2009), we measure all time-varying observable characteristics in period *t-1*. This is unlikely to make a difference for monthly data but seems more appropriate than using values at time t when working with annual data. In practice, we however find that this matters little for our estimation results.

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

effects between the dependent variable and current or future values of x_i are ruled out.[11] Under these conditions, we can interpret the coefficient of the lagged dependent variable λ as measuring *structural* state dependence. The spurious state dependence induced by persistent unobserved heterogeneity is captured by the individual-specific effect α_i.

Under these assumptions, we write the joint likelihood of an individual's benefit receipt status over all time periods $y_i' = (y_{i1} \ ... \ y_{iT})$ conditional on (y_{i0}, x_i, α_i) as

$$f(y_{i1}, ..., y_{iT}|y_{i0}, x_i, \alpha_i; \theta) = \prod_{t=1}^{T} f(y_{it}|y_{it-1}, x_{it-1}, \alpha_i; \theta), \tag{3}$$

where $\theta = (\lambda \ \beta')'$ denotes the vector of coefficients. As in the static random-effects probit model, the unobserved individual-specific error component α_i needs to be integrated out before we can construct the likelihood function and estimate θ. In case of a dynamic probit model, this however requires us to specify the relationship between α_i and the outcome in the initial period y_{i0}, which enters the model as the lag of the outcome in period $t = 1$. A failure to adequately account for the endogeneity of initial conditions leads to an overestimate of structural state dependence unless in the special case where the initial outcome is exogenous (see for instance Chay & Hyslop (2000)).

Following the approach proposed by Wooldridge (2005), we address this initial conditions problem by specifying the density of α_i conditional on y_{i0} and x_i.[12] This allows us to model the joint density of $y_{i1} \ ... \ y_{iT}|y_{i0}, x_i$ unconditional on α_i as

$$f(y_{i1}, ..., y_{iT}|y_{i0}, x_i; \theta, \gamma) = \int \prod_{t=1}^{T} f(y_{it}|y_{it-1}, x_{it-1}, \alpha_i; \theta) g(\alpha_i|y_{i0}, x_i; \gamma) d\alpha_i. \tag{4}$$

Specifically, Wooldridge (2005) proposes to specify the individual-specific term as $\alpha_i = \gamma_0 + \gamma_1 y_{i0} + x_i'\gamma_2 + a_i$ and let the remaining error term a_i be distributed as $(a_i|y_{i0}, x_i) \sim \mathcal{N}(0, \sigma_a^2)$. Next to determining the relationship between α_i and y_{i0}, this distributional assumption allows for a correlation of the individual-specific component α_i with the explanatory variables of the type proposed by Chamberlain (1980). The residual individual-specific component a_i is uncorrelated with the regressors x_i, y_{i0} and the transitory shock ε_{it} but by construction not with the lagged dependent variable y_{it-1}.

Using this distributional assumption, we can integrate out the persistent error component

[11] Using German data, Biewen (2009) provides evidence on such feedback effects in a joint model of poverty, employment, and household composition, where future 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 welfare benefit receipt dynamics in assuming no such feedback effects.

[12] An alternative and widely-used approach due to Heckman (1981b) accounts for the correlation between the initial condition y_{i0} and the individual-specific effect by specifying a distribution of $y_{i0}|x_i, \alpha_i$ and by then estimating the joint distribution of $f(y_{i0}, ..., y_{iT}|x_i, \alpha_i; \theta)$. While the different approaches have been shown to differ little in performance for panels of appropriate lengths (Arulampalam & Stewart, 2009; Akay, 2012), the Wooldridge approach has the advantage of being computationally simple and of extending in a relatively straightforward way to settings with richer state dependence (see the further discussion in the Appendix, Section A.1, and an earlier application by Stewart (2007, p. 527)).

and write the joint density of $y_{i1}, ..., y_{iT}|y_{i0}, x_i$ as

$$\int \prod_{t=1}^{T} \left\{ \left[\Phi(\lambda y_{it-1} + x'_{it-1}\beta + \gamma_1 y_{i0} + x'_i\gamma_2 + a_i)\right]^{y_{it}} \right.$$
$$\left. \left[1 - \Phi(\lambda y_{it-1} + x'_{it-1}\beta + \gamma_1 y_{i0} + x'_i\gamma_2 + a_i)\right]^{(1-y_{it})} \right\} \left(\frac{1}{\sigma_a}\right) \phi \left(\frac{a}{\sigma_a}\right) da. \quad (5)$$

This density is identical to the one of the standard random-effects probit model with the additional explanatory variables y_{i0} and x_i added in each period. Parameter estimates for this model can thus be obtained through maximum likelihood estimation. In empirical practice, the vector of past and future values of all covariates x_i is typically substituted for by the individual longitudinal averages of all time-varying observables $\bar{x}_i$ as in the correlated random-effects model introduced by Mundlak (1978). This is also what we do in our analysis.[13]

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 (Butler & Moffitt, 1982; Rabe-Hesketh, Skrondal & Pickles, 2005).[14]

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 welfare dynamics – is that sequences of benefit receipt satisfy the conditional Markov property at the annual level. In reality, the minimum period of welfare 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

$$P(y_t = 1|y_0, \ldots, y_{t-1}) = P(y_t = 1|y_{t-1}) \quad (6)$$

it must also hold that

$$P(y_{t+k} = 1|y_0, \ldots, y_{t-1}) = P(y_{t+k} = 1|y_{t-1}) \quad (7)$$

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

[13]Following Rabe-Hesketh & Skrondal (2013), we tested an alternative specification that controls for the initial values of the observable characteristics x_{i0} in addition to $\bar{x}_i$, however find that this does not affect our results.

[14]As a robustness check, we replicated our results using a Monte Carlo Expectation Maximization (MCEM) algorithm that gives simulation based maximum likelihood estimates (Wei & Tanner, 1990). The two routines gave nearly identical results. Small differences in the estimates should be expected since the quadrature routine only gives an approximate likelihood and the MCEM routine has simulation errors.

measured every 12 months, and monthly benefit transitions can be aggregated up to the annual level. If, by contrast, 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.

In this subsection, we set out to derive the relationship between the predicted transition probabilities at the monthly, annual and biennial level in a Markov model. Crucially, we propose two methods – one analytical, one simulation-based – that allow us to derive testable predictions about the validity of the conditional Markov assumption at either the monthly or the annual level. In the next section, we then go on to test the Markov assumption for the two approaches of defining the annual welfare benefit variable using monthly data on welfare benefit receipt in Norway.

3.2.1 Aggregating transition probabilities over time

In a Markov model of welfare benefit receipt dynamics, it is relatively straightforward to aggregate up monthly transition probabilities to the annual level. Using the results from Section 3.1, we define the conditional probability of entry into benefit receipt in a Wooldridge-type dynamic random-effects probit model as

$$p_{it}^{01}(y_{it-1}, y_{i0}, x_i, a_i) = P(y_{it} = 1 | y_{it-1} = 0, y_{i0}, x_i, a_i) = \Phi(x'_{it-1}\beta + \gamma_1 y_{i0} + \bar{x}'_i \gamma_2 + a_i) \tag{8}$$

and the corresponding persistence probability as

$$p_{it}^{11}(y_{it-1}, y_{i0}, x_i, a_i) = P(y_{it} = 1 | y_{it-1} = 1, y_{i0}, x_i, a_i) = \Phi(\lambda + x'_{it-1}\beta + \gamma_1 y_{i0} + \bar{x}'_i \gamma_2 + a_i). \tag{9}$$

We construct a transition matrix based on these expressions as

$$\mathcal{A}_{it}(y_{it-1}, y_{i0}, x_i, a_i) = \begin{bmatrix} 1 - p_{it}^{01} & p_{it}^{01} \\ 1 - p_{it}^{11} & p_{it}^{11} \end{bmatrix}, \tag{10}$$

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

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

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 realised for a given individual i. Similarly, iterating Equation (11), we can write the vector of expected outcomes s periods ahead as

$$E(z_{it}|z_{it-s}, y_{i0}, x_i, a_i) = \left(\prod_{j=0}^{s-1} \mathcal{A}'_{it-j}\right) z_{it-s}. \tag{12}$$

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

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

$$A^m_{it} \quad = \quad \int \mathcal{A}^m_{it} \frac{1}{\sigma_a} \phi\left(\frac{a}{\sigma_a}\right) da, \tag{13}$$

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

$$A^y_{it} \quad = \quad \int \left(\prod_{s=0}^{11} \mathcal{A}^m_{it-s}\right) \frac{1}{\sigma_a} \phi\left(\frac{a}{\sigma_a}\right) da. \tag{14}$$

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 $\mathcal{N}(0, \hat{\sigma}^2_a)$ rather than by integrating over this distribution. To produce precise estimates of individual-specific transition rates, we therefore need multiple simulations for each individual. Since however we are not interested in precise estimates of individual-specific transition rates but only the population averages, the large sample sizes utilized in our analysis make it satisfactory to perform only a single simulation for each individual.

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

$$\widehat{P}(y_{it} = 1|y_{it-1} = 0, x_i, y_{i0}, \tilde{a}_i) = \Phi\left(x'_{it-1}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i \hat{\gamma}_2 + \tilde{a}_i\right), \tag{15}$$

and the corresponding persistence probability for the same individual as

$$\widehat{P}(y_{it} = 1 | y_{it-1} = 1, x_i, y_{i0}, \tilde{a}_i) = \Phi\left(\hat{\lambda} + x'_{it-1}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2 + \tilde{a}_i\right). \tag{16}$$

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 treatment effect (ATE)[15] as

$$ATE = \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} \left[\widehat{P}(y_{it} = 1 | y_{it-1} = 1, x_i, y_{i0}, \tilde{a}_i) - \widehat{P}(y_{it} = 1 | y_{it-1} = 0, x_i, y_{i0}, \tilde{a}_i)\right]. \tag{17}$$

This ATE describes the estimated structural effect of benefit receipt last month on the probability of welfare benefit receipt in the current month.

The two monthly transition probabilities for each individual and period given by Equations (15) and (16) 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 treatment effect implied by the estimates from the *monthly* model. This effect can be readily compared to the average treatment 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 (14) 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 welfare 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

$$\hat{y}_{it+1} \quad = \quad \mathbb{1}\left\{\hat{\lambda} y_{it} + x'_{it}\hat{\beta} + \hat{\gamma}_1 y_{i0} + \bar{x}'_i\hat{\gamma}_2 + \tilde{a}_i + \tilde{e}_{it+1} > 0\right\}. \tag{18}$$

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*.

[15]The term typically used in studies of benefit dynamics is that of the average partial effect (APE), see for instance Wooldridge (2005) and Stewart (2007). We follow Wooldridge (2001, p. 68) in using the term ATE for a discrete variable.

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 5.2. For all Markov models that we estimate, we however verified that the two approaches yield indeed identical results.

3.2.2 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 can not necessarily be approximated by a 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 An empirical test of the Markov model for welfare transitions

In this section, we test the assumption that the dynamics of welfare entry and persistence can be described jointly 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's characteristics (age, years of education, and immigrant status) for married or co-habiting individuals. All our specifications moreover include year dummies and a control for the municipal unemployment rate. Since the focus of our paper is on the analysis of state dependence in welfare benefit receipt, we limit ourselves to reporting average predicted transition rates and the corresponding average treatment effect of past benefit receipt for each specification. Coefficient estimates for the various specifications are reported in the Appendix, Section A.2.

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 2, our data provide monthly information on the receipt of welfare 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 obtainable 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.[16] 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.

[16]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 5. Königs (2013b) shows that there is some seasonality in benefit receipt for individuals in their low 20s, who are more likely to access welfare benefits during the summer months to bridge gaps in their educational schedules (see also Figures A.1 and A.2 in the Appendix). 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 ATEs 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 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 treatment 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 ATE implied by our monthly model when aggregated to the annual level. The results in column (4) finally 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.

Table 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
Avg. predicted persistence rate (in %)	14.0 (0.2)	7.0 (0.2)	3.1 (0.1)	2.9 (0.0)
– Avg. predicted entry rate (in %)	1.4 (0.0)	2.1 (0.1)	3.1 (0.1)	2.9 (0.0)
= Average treatment 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 annual model in column (2) refer to 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 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 based on running the annual model on 10 simulated data sets. Coefficient estimates for the two specifications presented in columns (1) and (2) are reported in Table A.1 in the Appendix.

The results presented in columns (1) and (2) suggest that benefit dynamics are characterised 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 welfare 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 treatment effect, i.e. the effect of benefit receipt last month on the probability of benefit receipt this month, is 12.6 percentage points. Estimates from the annual model imply a somewhat lower average treatment 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 treatment 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 1 however indicate that based on the estimates of our monthly model, the probability of benefit receipt in the current period is virtually independent of whether an individual did or did not receive welfare benefits twelve months ago. Our simulation-based results give an annual average treatment 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 welfare 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 treatment 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 3.2.2, the previous exercise is valid only if the benefit receipt process observed in the annualized data can be approximated by a similar type of dynamics 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 1. We constructed ten data sets based on the estimates from the monthly model, each of which we used to extract annual data, estimate an annual dynamic random-effects probit model on these data, and calculate predicted transition rates and average treatment effects. Column (4) presents the mean and standard deviation of these transition rates and average treatment effects over the ten replications.

We find that 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 as aggregated from the monthly to the annual level. The strong difference in results between the two annual models when 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.

Next, we perform a time aggregation exercise to test whether the Markov assumption is satisfied at the annual level. Our results again show 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 2 the estimates obtained directly from the annual and biennial models, while column (3) gives the average predicted transition probabilities and ATEs 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 welfare dynamics with ATEs 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 treatment effect of 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 ATE 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.

Table 2: Time aggregation: Annual to Biennial ('point-in-time')

	(1) Annual model	(2) Biennial model	(3) 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 treatment effect (in ppts)	5.2 (0.3)	3.5 (0.3)	1.5 (0.1)

Note: The annual model in column (1) and biennial model in column (2) refer to 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 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 Table A.2 in the Appendix.

4.2 Time aggregation for the 'benefit year' variable

As discussed in Section 3.2, the more widely-used approach for defining an annual welfare benefit receipt variable has been to look at receipt at 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 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.[17]

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 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 2. We calculate a

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

predicted persistence rate into benefit receipt of 20.1% and an entry rate of 4.4%, which translates into an ATE 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 2.

Table 3: Time aggregation: Annual to Biennial ('benefit year')

	(1) Annual model	(2) Biennial model	(3) 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 treatment effect (in ppts)	15.7 (0.4)	6.8 (0.6)	4.5 (0.2)

Note: The annual model (1) and biennial model (2) refer to 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 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 Table A.3 in the Appendix.

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

4.3 A model with higher-order lags

Our findings thus far show that standard models of welfare 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 however 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 (1) as

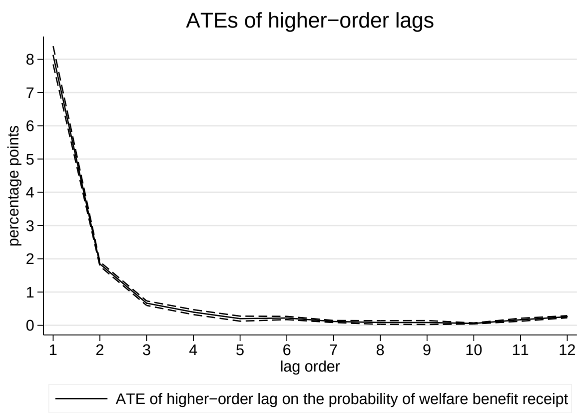
$$y_{it} \quad = \quad \mathbb{1}\left\{\sum_{s=1}^{12}\lambda_s y_{it-s} + x_{it-1}'\beta + \alpha_i + \varepsilon_{it} > 0\right\} \quad \text{for} \quad i = 1, ..., N; \; t = 12, ..., T, \qquad (19)$$

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

$$\alpha_i = \gamma_0 + \sum_{s=0}^{11} \gamma_{1,s} y_{is} + \bar{x}_i' \gamma_2 + a_i. \tag{20}$$

Under distributional assumptions similar to those discussed in Section 3.1, we can then integrate out α_i and estimate this model using conditional maximum likelihood methods (see Appendix, Section A.1).[18]

Figure 2 plots the estimated average treatment effects of the first twelve lags and their 95% confidence intervals. We find that all of these ATEs are significantly different from zero, which means that we can reject the Markov model also on purely statistical grounds. Economically-speaking however, only the second and possibly the third lag are of a meaningful magnitude with estimated ATEs of 1.8 and 0.6 percentage points, respectively. Even for those two lags, the estimated effect is already much smaller than the ATE of 8.1 percentage points for the first lag.



Note: Average treatment 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.

Figure 2: Average treatment effects for the higher-order Markov model

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 specification fails as dramatically in terms of its time aggregation properties as observed in Table 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 robust estimates of the level of state dependence in benefit

[18] 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.

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 generalise and extend the models just presented, by allowing first for different processes to determine entries into and persistence in benefit receipt, and by then accounting explicitly for duration and occurrence dependence in each of these processes.

5 Modelling heterogeneous welfare 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 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 lagged benefit receipt status. The model thus constraints 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, 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 can impact the probability of benefit receipt and that might explain the observed pattern of significant higher-order lags.

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 (1), we write

$$\begin{aligned} y_{it} &= \mathbb{1}\left\{(\bar{\lambda}+\lambda_i)y_{it-1} + x'_{it-1}(\beta + y_{it-1}\eta) + \alpha_i + \varepsilon_{it} > 0\right\} \\ & \quad \text{for} \;\; i = 1, ..., N; \; t = 12, ..., T. \end{aligned} \tag{21}$$

There are two important differences between this model and the standard specification described in Equation (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.[19] Note that this random-coefficients framework generalizes the standard Markov model, as the DRCP model in Equation (21) collapses to the DREP model in Equation (1) for $\eta = 0$ and $\lambda_i = 0 \; \forall \, i$.[20]

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

$$P(y_{it} = 1 | y_{it-1} = 0, x_i, \alpha_i, \lambda_i) = \Phi(x'_{it-1}\beta^0 + \alpha_i^0), \tag{22}$$

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

$$P(y_{it} = 1 | y_{it-1} = 1, x_i, \alpha_i, \lambda_i) = \Phi(\bar{\lambda} + x'_{it-1}\beta^1 + \alpha_i^1), \tag{23}$$

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

$$\alpha_i^j = \gamma_0^j + \gamma_1^j y_{i11} + \bar{x}'_i \gamma_2^j + a_i^j, \tag{24}$$

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 treatment effect for the DRCP model are presented in Panel A of Table 4. As earlier, we calculate the average treatment

[19]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 welfare 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.

[20]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 below.

effect as the average difference between the predicted welfare persistence and entry probabilities across individuals. The ATE 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 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 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 treatment 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 treatment 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 specification will thus attribute a larger share of observed state dependence in benefit receipt to the lagged dependent variable.

Table 4: Time aggregation in the standard DRCP model: Monthly to Annual

	(1) Monthly model	(2) Annual model	(3) Monthly-to-annual time aggregation	(4) Simulation-based specification check
A. State dependence				
Avg. predicted persistence rate (in %)	32.8 (0.9)	12.6 (1.0)	3.0 (0.1)	3.2 (0.2)
- Avg. predicted entry rate (in %)	1.3 (0.0)	2.1 (0.1)	2.8 (0.1)	2.6 (0.1)
= Average treatment effect (in ppts)	31.4 (0.9)	10.5 (1.1)	0.2 (0.0)	0.5 (0.2)
B. 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 annual model in column (2) refer to the dynamic random-coefficients probit specifications of Equations (21)-(24) 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 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 based on running the annual model on 10 simulated data sets. Coefficient estimates for the two specifications presented in columns (1) and (2) are reported in Table A.5 of the Appendix. 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.

To illustrate the impact of unobserved heterogeneity on welfare entry and persistence, we report some additional parameters from our DRCP model in Panel B of Table 4. As summarised 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 welfare and an ability a_i^0 related to entries to welfare. Ignoring these differences leads to a downward bias in the estimated degree of state dependence in welfare benefit receipt.

Indeed, 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.[21] This illustrates that the assumption made on the form of unobserved heterogeneity in the standard dynamic random-effects probit model is 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 (21) and (24) 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 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 ATE 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 treatment effect of 0.2 percentage points.

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 treatment 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 thus 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 article therefore extends our model to allow for duration and occurrence dependence in welfare benefit receipt.

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 welfare 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

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

higher-order lags in Section 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 can 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 welfare benefit receipt.[22]

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 welfare 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 welfare as the time spent off benefits rises. All of these are theoretical justifications for negative duration dependence in the probability of exits from welfare 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 welfare 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 'welfare 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 welfare are reflected in the analysis. The large size of the data set moreover permits us to restrict our sample to individuals whom we observe from the moment

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

they become entitled to claim welfare 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 welfare 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* welfare and the number of previous spells since the start of an individual's welfare career. Using earlier notation, we specify the probability of entry into welfare as

$$P(y_{it} = 1 | y_{it-1} = 0, x_i, d_i^0, d_i^1, o_i, \alpha_i, \lambda_i) = \Phi(x_{it-1}'\beta^0 + \sum_{k=2}^{12} \phi_k^0 d_{ikt-1}^0 + \sum_{k=1}^{11} \psi_k^0 o_{ikt-1} + \alpha_i^0), \quad (25)$$

and the probability of persistence in welfare as

$$P(y_{it} = 1 | y_{it-1} = 1, x_i, d_i^0, d_i^1, o_i, \alpha_i, \lambda_i) = \Phi(\bar{\lambda} + x_{it-1}'\beta^1 + \sum_{k=2}^{12} \phi_k^1 d_{ikt-1}^1 + \sum_{k=1}^{11} \psi_k^1 o_{ikt-1} + \alpha_i^1). (26)$$

The 2 × 11 dummy variables d_{ikt-1}^0 and d_{ikt-1}^1 indicate whether, in period *t-1*, the ongoing spell off or on welfare, respectively, had lasted for exactly k periods. The reference case is the first month of a spell on or off welfare. We restrict the effect of ongoing spells with a duration of more than eleven months to be constant adding single dummies $(d_{i12t-1}^0, d_{i12t-1}^1)$ 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 welfare 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 $(d_{ik}^0, d_{ik}^1, o_{ik})$ can therefore be measured for each individual in our sample from the beginning of the individual's welfare 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_{ik}^0 and d_{ik}^1 with coefficients ϕ_k^0 and ϕ_k^1. The number of previous spells enters through a single set of dummies o_{ik}, however with separate sets of coefficients ψ_k^0 and ψ_k^1 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_i^0, α_i^1) 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.[23]

By allowing for essentially separate processes for entries into and persistence in benefit

[23] We could have again written the extended DRCP model in form of a single equation analogous to Equation (21) for the simpler DRCP, where each coefficient in the persistence process would be written as 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 $\phi_k^0 = \psi_k^0 = \phi_k^1 = \psi_k^1 = 0 \; \forall \, k$, the extended DRCP model in Equations (25)-(26) reduces to the standard DRCP model in Equations (22)-(23).

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 welfare 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).[24] 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 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 welfare) 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

$$\alpha_i^j = \gamma_0^j + \gamma_1^j y_{i11} + \bar{x}_i' \gamma_2^j + \sum_{k=2}^{12} \gamma_{3,k}^j d_{ik11}^0 + \sum_{k=2}^{12} \gamma_{4,k}^j d_{ik11}^1 + \sum_{k=1}^{5} \gamma_{5,k}^j o_{ik11} + a_i^j, \tag{27}$$

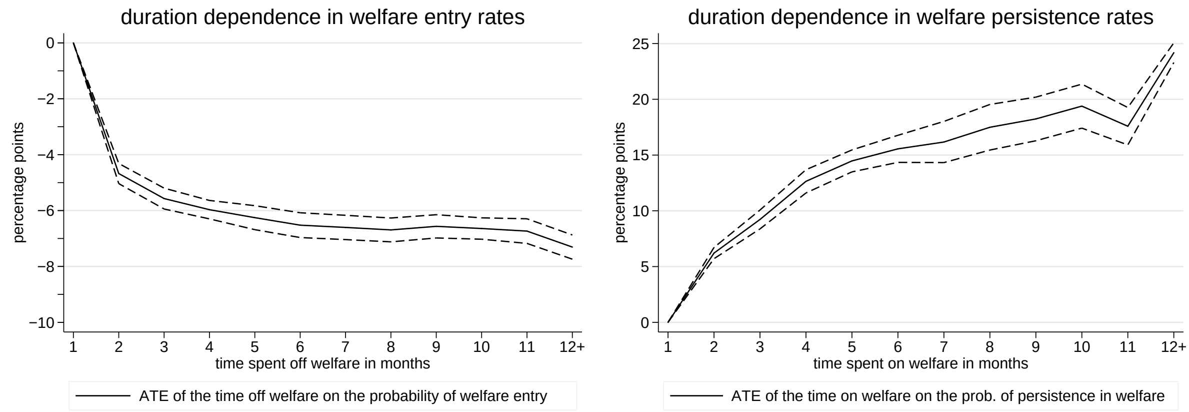
for $j = 0, 1$.[25] Note that depending on whether the individual is on or off welfare in the initial period y_{i11}, either of the two sets of duration variables in the initial period d_{i11}^0 and d_{i11}^1 will be all zero. We again require the remaining unobserved heterogeneity terms a_i^0 and a_i^1 to follow a bivariate normal distribution with zero means, variances $(\sigma_{a_0}^2, \sigma_{a_1}^2)$ and a correlation τ. Under

[24] 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.

[25] 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.

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 Section A.1 of the Appendix.[26]

We start by presenting ATEs of spell durations and occurrences on the probability of benefit receipt to illustrate the importance of duration and occurrence dependence in welfare benefit receipt dynamics. Figure 3 plots the average treatment effect of the time spent on and off welfare on the probability of welfare persistence and welfare entry, respectively. We obtain these ATEs 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. ATEs are expressed in reference to a spell in its initial month either on or off welfare. All other characteristics of the individual, including the occurrence variables, are kept fixed at their observed values.[27]



Note: Average treatment effects have been calculated based on the coefficient estimates reported in Table A.6 in the Appendix. An individual's number of previous benefit spells and all covariates are kept fixed in the calculations. ATEs 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.

Figure 3: Duration dependence in entry and persistence rates

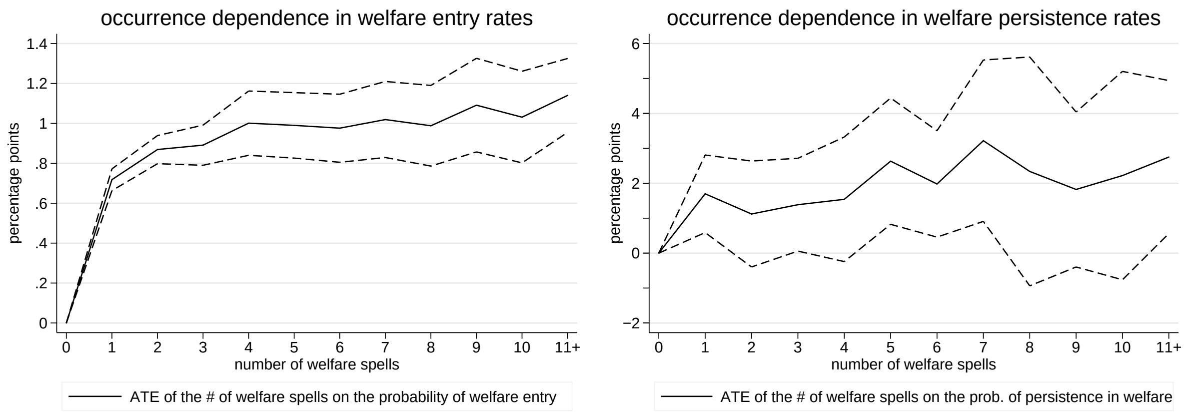
We find substantial duration dependence both on and off welfare. In the left panel, we give the average treatment effect of the duration off benefits on the rate of welfare entry. We find a strongly declining pattern from the first to the second period of benefit receipt. The probability of entering 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 ATE 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.

[26] An alternative approach to the initial conditions problem would have been to estimate the model over the entire period $t = 1, ..., T$ rather than $t = 12, ..., T$, 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.

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

The right panel shows very strong duration dependence also for persistence in benefit receipt. Individuals in the second month of their welfare spell are about 5 percentage points more likely to stay on benefits than if they were in their initial month on benefits. 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.

We produce similar calculations for the set of occurrence dummies included in our model and present the average treatment effects of previous spell occurrences on welfare entry and persistence rates in Figure 4. Again, these ATEs 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.



Note: Average treatment effects have been calculated based on the coefficient estimates reported in Table A.6. For the calculation of the ATEs, an individual's benefit receipt status, the duration of the ongoing spell and all covariates are kept fixed in the calculations. ATEs 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.

Figure 4: Occurrence dependence in entry and persistence rates

Results suggest that a history of previous welfare 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 left panel of Figure 4, we find a significantly positive average treatment 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 treatment effect of previous benefit spells on benefit persistence, five of the eleven occurrence dummies reported in Table A.6 are not significant at the 5% level. ATEs plotted in the right panel of Figure 4 give a similar result.

Overall, the finding 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 suggests 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 welfare entry and persistence rates for each individual in the sample and calculate the degree of 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 welfare 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 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 ATE 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 inclusion of duration and occurrence dummies leads to a substantial improvement in the fit of the model. 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 Panel B of of Table 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 5, the magnitude of estimated duration and occurrence dependence is indeed 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 3. Also the seemingly modest occurrence-dependence effect of around 0.8 percentage points after a single previous spell shown in Figure 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 3 compares to an average predicted exit rate from benefits of 37% for an individual in the first month of the benefit spell.[28]

Table 5: Time aggregation in the extended DRCP model: Monthly to Annual

	(1) Monthly model	(2) Monthly-to-annual time aggregations
A. 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 treatment effect (in ppts)	37.0 (0.5)	5.0 (0.2)
B. 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 (25)-(27) 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 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 Table A.6 of the Appendix. 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.

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 5, we report December-to-December transition probabilities obtained from aggregating up the monthly transition probabilities to the annual level[29]: In the case of benefit receipt 12 months ago the likelihood of benefit receipt today is 6.3% compared to 1.3% conditional on non-receipt 12 months ago. The average treatment 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 straightforward. The endogenous dura-

[28]Note that a comparison of ATEs for the occurrence and duration dummies with average predicted transition rates is only approximate: For calculating the ATEs 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.

[29]Column (3) of Table 5 is thus the analogue to columns (2) of Tables 1-4.

tion 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 (27) will therefore no longer give consistent estimates at the annual level (see the discussion in the Appendix, Section A.1).[30] 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. Indeed, the required 'similarity' of the simulated and the true data must hold for all characteristics of the two data sets. For instance, we should expect any other model – also one that is misspecified – to provide 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.

Table 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 treatment 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 (21)-(24) estimated on annualized data (identical to column (2) of Table 4). The estimates presented in column (2) were obtained by estimating the standard DRCP specification in Equations (21)-(24) using annualized data that were simulated based on the extended monthly DRCP model in Equations (25)-(27). The estimates presented in column (3) were obtained by estimating the standard DRCP specification in Equations (21)-(24) using annualized data that were simulated based on the standard monthly DRCP model (identical to column (4) of Table 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.

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 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

[30] 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.

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. The results of the specification test are presented in Table 6.

We find that also our extended monthly DRCP model only partially succeeds at predicting the monthly welfare benefit dynamics in Norway. As seen earlier, the simple annual DRCP model estimated on annualized true data predicts an average treatment effect of benefit receipt 12 months ago of 10.5 percentage points (column (1), reproduced from Table 4)). Estimating the same model on data simulated from the extended monthly DRCP model, we obtain average predicted entry rates that are nearly 50% lower (column (2)). For an essentially unchanged average predicted entry rate, this implies a decline in the ATE to only 4.1 percentage points. Note that as emphasised 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 extended 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 ATE 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.

6 Conclusion

Dynamic discrete-choice models have been 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 standard 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 treatment effect of the lagged dependent variable, which is interpreted as capturing structural state dependence in benefit receipt.

In this paper, we showed that a standard Markov model gives highly inconsistent estimates of the degree of structural state dependence in welfare benefit dynamics in Norway. Based on monthly administrative data over the 16-year period 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 welfare 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 paper, 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 unobserved heterogeneity terms for the welfare 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 substantial overestimate of the level of state dependence.

We finally illustrated that 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 affects only 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 treatment 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 represents a notable improvement however 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 welfare 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 article 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 welfare benefit receipt dynamics or labour-market-state dynamics more broadly.

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Appendix

A.1 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 Section 3. This initial conditions problem is even more severe in the specifications estimated in Sections 4.3 and 5.2 that additionally include either higher-order lags or measures of spell duration and the number of previous benefit spells. These variables are all functions of past realizations of the dependent variable and 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

$$\begin{aligned} f(y_{it}, w_{it}|y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \zeta) = f(y_{it}|y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \theta) \times \\ f(w_{it}|y_{it}, y_{it-1}, w_{it-1}, x_{it-1}, \alpha_i; \omega). \end{aligned} \tag{A.1}$$

As in Section 3.1, the joint likelihood of the benefit receipt status and the set of endogenous covariates over all time periods follows as

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

where (y_{i0}, w_{i0}) are the initial-period values of the benefit receipt status and the endogenous covariates, respectively. This expression is the analogue of Equation (4) in the standard model. 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

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

As in the simple case, we can use this joint density to consistently estimate the model under the assumption that the distribution $\alpha_i|y_{i0}, w_{i0}, x_i$ 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} 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 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 (A.2) therefore cannot be simplified to give Equation (A.3) and the outlined approach does not lead to consistent estimates.

A.2 Detailed estimation results

	MONTHLY MODEL		ANNUAL MODEL	
$y_{(t-1)}$	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				
y_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	

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Table A.1 – continued from previous page –

# of observations	3,279,708	272,878
# of individuals	30,899	30,899

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

note: standard errors in parentheses. The dependent variable measures receipt of welfare 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 y_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 A.1: The standard Markov specification, 'point-in-time' definition: Monthly and Annual model

	ANNUAL MODEL		BIENNIAL MODEL	
$y_{(t-1)}$	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				
y_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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* p<0.10, ** p<0.05, *** p<0.01

note: standard errors in parentheses. The dependent variable measures receipt of welfare benefits in December of the given year for both the annual and biennial model. 'Wooldridge controls' include the outcome variable in the initial period y_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 A.2: The standard Markov specification, 'point-in-time' definition: Annual and Biennial model

	ANNUAL MODEL		BIENNIAL MODEL	
$y_{(t-1)}$	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				
y_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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* p<0.10, ** p<0.05, *** p<0.01
note: standard errors in parentheses. The dependent variable measures receipt of welfare benefits at any time during the given one-year / two-year period for the annual and biennial model, respectively. 'Wooldridge controls' include the outcome variable in the initial period y_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 A.3: The standard Markov specification, 'benefit year' definition: Annual and Biennial model

	monthly model with higher-order lags	
$y_{(t-1)}$	1.402***	(0.007)
higher-order lags		
$y_{(t-2)}$	0.562***	(0.008)
$y_{(t-3)}$	0.247***	(0.008)
$y_{(t-4)}$	0.157***	(0.008)
$y_{(t-5)}$	0.082***	(0.009)
$y_{(t-6)}$	0.090***	(0.009)
$y_{(t-7)}$	0.047***	(0.009)
$y_{(t-8)}$	0.035***	(0.009)
$y_{(t-9)}$	0.035***	(0.009)
$y_{(t-10)}$	0.024***	(0.009)
$y_{(t-11)}$	0.069***	(0.009)
$y_{(t-12)}$	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		
y_0	0.275***	(0.036)

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Table A.4 – continued from previous page –

	MONTHLY MODEL WITH HIGHER-ORDER LAGS	
y_1	0.229***	(0.039)
y_2	0.108***	(0.040)
y_3	0.087**	(0.038)
y_4	0.158***	(0.036)
y_5	0.220***	(0.037)
y_6	0.133***	(0.039)
y_7	0.006	(0.042)
y_8	0.099**	(0.044)
y_9	0.177***	(0.045)
y_{10}	-0.016	(0.048)
y_{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 measures receipt of welfare 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 y_0 to y_{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 A.4: The higher-order Markov specification: Monthly model

	MONTHLY MODEL				ANNUAL MODEL			
	entry equation		persistence equation		entry equation		persistence equation	
individual characteristics								
female	-0.179***	(0.018)	-0.038*	(0.020)	-0.128***	(0.024)	-0.078*	(0.046)
immigrant	0.328***	(0.037)	0.248***	(0.034)	0.256***	(0.047)	0.229***	(0.071)
age	0.155***	(0.027)	-0.423***	(0.045)	0.406***	(0.074)	-0.573***	(0.155)
age^2	-0.376***	(0.058)	0.910***	(0.096)	-0.865***	(0.161)	1.269***	(0.334)
years of education	0.022***	(0.008)	-0.003	(0.008)	0.132***	(0.013)	0.062***	(0.019)
years of education^2	-0.004***	(0.000)	-0.001***	(0.001)	-0.014***	(0.001)	-0.008***	(0.001)
household characteristics								
single, with children	-0.090***	(0.013)	-0.044**	(0.021)	0.089**	(0.039)	0.057	(0.071)
couple, no children	-0.233***	(0.038)	0.010	(0.066)	-0.247*	(0.127)	0.143	(0.221)
couple, with children	-0.288***	(0.017)	-0.019	(0.028)	-0.065	(0.052)	-0.039	(0.100)
child aged 0-5 years	-0.113***	(0.013)	-0.123***	(0.020)	-0.157***	(0.034)	-0.150**	(0.065)
household size	0.060***	(0.005)	-0.005	(0.008)	0.063***	(0.015)	0.007	(0.028)
spouse characteristics								
immigrant	-0.034	(0.060)	0.062	(0.088)	-0.049	(0.141)	-0.121	(0.254)
age	-0.003	(0.005)	-0.006	(0.008)	0.004	(0.013)	-0.023	(0.024)
age^2	0.005	(0.013)	0.007	(0.019)	-0.005	(0.032)	0.045	(0.061)
years of education	0.050***	(0.015)	0.029	(0.023)	0.029	(0.036)	0.067	(0.066)
years of education^2	-0.005***	(0.001)	-0.003**	(0.002)	-0.004*	(0.002)	-0.004	(0.004)
year dummies								
1995	0.191***	(0.066)	0.061	(0.082)	-0.025	(0.115)	0.031	(0.232)
1996	0.205***	(0.061)	0.083	(0.076)	0.086	(0.107)	0.021	(0.214)
1997	0.210***	(0.058)	0.063	(0.073)	0.039	(0.103)	0.099	(0.204)
1998	0.168***	(0.055)	0.029	(0.070)	-0.086	(0.100)	0.033	(0.198)
1999	0.090*	(0.052)	-0.000	(0.068)	-0.104	(0.098)	0.120	(0.195)
2000	0.090**	(0.049)	-0.004	(0.066)	-0.100	(0.096)	0.197	(0.192)
2001	0.078**	(0.045)	0.020	(0.063)	-0.087	(0.092)	0.195	(0.185)
2002	0.116***	(0.043)	0.001	(0.060)	-0.071	(0.089)	0.247	(0.181)
2003	0.117***	(0.040)	0.002	(0.058)	-0.031	(0.087)	0.338*	(0.177)
2004	0.141***	(0.038)	-0.011	(0.056)	0.009	(0.085)	0.340*	(0.174)
2005	0.126***	(0.036)	-0.036	(0.055)	-0.034	(0.085)	0.320*	(0.173)
2006	0.111***	(0.035)	0.012	(0.054)	-0.002	(0.085)	0.325*	(0.172)
2007	0.073**	(0.034)	0.039	(0.054)	-0.039	(0.086)	0.150	(0.172)
2008	-0.023	(0.036)	0.001	(0.056)	-0.176*	(0.094)	0.090	(0.181)
Wooldridge controls								
y_{11}	1.379***	(0.033)	0.751***	(0.029)	0.949***	(0.041)	0.695***	(0.058)
single, with children	-0.173***	(0.044)	-0.155***	(0.047)	-0.402***	(0.065)	-0.330***	(0.117)
couple, no children	-0.938***	(0.120)	-0.424***	(0.138)	-1.215***	(0.203)	-1.117***	(0.407)
couple, with children	-0.895***	(0.055)	-0.486***	(0.064)	-1.008***	(0.085)	-0.706***	(0.162)
child aged 0-5 years	1.118***	(0.039)	0.438***	(0.045)	0.849***	(0.058)	0.194*	(0.106)
household size	-0.059***	(0.015)	-0.006	(0.017)	-0.054**	(0.023)	0.023	(0.044)
local unempl. rate	4.720***	(0.783)	-0.573	(0.967)	3.480***	(1.304)	-0.972	(2.640)
local unempl. rate	1.981***	(0.312)	2.721***	(0.520)	2.743***	(0.914)	3.014	(1.891)
constant	-4.347***	(0.330)	4.802***	(0.525)	-6.704***	(0.843)	5.583***	(1.757)
σ_a	0.947***	(0.010)	0.590***	(0.012)	0.791***	(0.018)	0.654***	(0.032)
ρ	0.473***	(0.005)	0.258***	(0.008)	0.385***	(0.011)	0.300***	(0.021)
τ		0.714***	(0.014)			0.700***	(0.048)	
log Likelihood		-170,903.910				-23,511.214		

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Table A.5 – continued from previous page –

	MONTHLY MODEL		ANNUAL MODEL	
	entry equation	persistence equation	entry equation	persistence equation
# of observations	3,279,708		272,878	
# of individuals	30,899		30,899	

* p<0.10, ** p<0.05, *** p<0.01
note: standard errors in parentheses. The dependent variable measures receipt of welfare 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 y_{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 (22)-(23), respectively, while τ is their correlation.

Table A.5: The standard DRCP specification: Monthly and Annual model

	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				
$dur2$	-0.528***	(0.014)	0.188***	(0.013)
$dur3$	-0.700***	(0.016)	0.284***	(0.016)
$dur4$	-0.803***	(0.017)	0.396***	(0.019)
$dur5$	-0.889***	(0.018)	0.459***	(0.022)
$dur6$	-0.980***	(0.020)	0.502***	(0.024)
$dur7$	-1.030***	(0.021)	0.527***	(0.027)
$dur8$	-1.051***	(0.022)	0.575***	(0.031)
$dur9$	-1.006***	(0.023)	0.604***	(0.034)
$dur10$	-1.050***	(0.024)	0.656***	(0.037)
$dur11$	-1.061***	(0.025)	0.583***	(0.039)
$dur12+$	-1.429***	(0.011)	0.789***	(0.020)
occurrence variables				
$occ1$	0.368***	(0.014)	0.051***	(0.017)
$occ2$	0.418***	(0.018)	0.033*	(0.019)
$occ3$	0.419***	(0.020)	0.041*	(0.022)
$occ4$	0.453***	(0.023)	0.046*	(0.024)
$occ5$	0.448***	(0.025)	0.079***	(0.026)
$occ6$	0.442***	(0.028)	0.059**	(0.029)
$occ7$	0.452***	(0.030)	0.096***	(0.031)
$occ8$	0.444***	(0.033)	0.070**	(0.034)
$occ9$	0.477***	(0.036)	0.054	(0.037)
$occ10$	0.456***	(0.038)	0.066	(0.041)
$occ11+$	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)

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Table A.6 – continued from previous page –

	MONTHLY MODEL			
	entry equation		persistence equation	
1999	0.011	(0.039)	-0.007	(0.060)
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				
y_{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)
$dur2_{11}$	-0.011	(0.043)	-0.047	(0.048)
$dur2_{11}$ * y_{11}	0.055	(0.060)	0.088	(0.065)
$dur3_{11}$	-0.075*	(0.041)	-0.058	(0.047)
$dur3_{11}$ * y_{11}	0.209***	(0.064)	0.075	(0.069)
$dur4_{11}$	-0.116***	(0.039)	-0.108**	(0.045)
$dur4_{11}$ * y_{11}	0.289***	(0.064)	0.293***	(0.067)
$dur5_{11}$	-0.048	(0.043)	-0.083*	(0.050)
$dur5_{11}$ * y_{11}	0.117	(0.077)	0.263***	(0.081)
$dur6_{11}$	-0.154***	(0.052)	-0.221***	(0.066)
$dur6_{11}$ * y_{11}	0.234***	(0.082)	0.327***	(0.092)
$dur7_{11}$	-0.172***	(0.066)	-0.216***	(0.084)
$dur7_{11}$ * y_{11}	0.185*	(0.098)	0.329***	(0.110)
$dur8_{11}$	-0.113*	(0.066)	-0.016	(0.079)
$dur8_{11}$ * y_{11}	0.330***	(0.108)	0.219*	(0.115)
$dur9_{11}$	-0.085	(0.071)	-0.285***	(0.092)
$dur9_{11}$ * y_{11}	0.360***	(0.119)	0.546***	(0.129)
$dur10_{11}$	-0.128	(0.093)	-0.224*	(0.126)
$dur10_{11}$ * y_{11}	0.267**	(0.125)	0.519***	(0.151)
$dur11_{11}$	-0.311***	(0.111)	-0.087	(0.156)
$dur11_{11}$ * y_{11}	0.553***	(0.138)	0.239	(0.176)
$dur12+_{11}$	-0.268***	(0.043)	-0.227***	(0.045)
$dur12+_{11}$ * y_{11}	0.523***	(0.059)	0.579***	(0.058)
$occ1_{11}$	0.022	(0.031)	0.021	(0.032)
$occ2_{11}$	0.089**	(0.036)	0.061	(0.038)
$occ3_{11}$	0.161***	(0.050)	0.054	(0.052)
$occ4_{11}$	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)

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Table A.6 – continued from previous page –

	MONTHLY MODEL			
	entry equation		persistence equation	
ρ	0.111***	(0.006)	0.092***	(0.005)
τ	0.784***	(0.026)		
log Likelihood	-158,370.707			
# 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 measures receipt of SEA benefits in a given month for the monthly model and in December of the given year for the annual model. The variables labelled $dur*$ and $occ*$ 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 y_{11} and longitudinal averages of the time-varying variables, as well as the values of the duration and occurrence dummies in the initial period and their interactions with outcome variable in the initial period. 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 (25)-(26), respectively, while τ is their correlation.

Table A.6: The extended DRCP specification with duration and occurrence dependence: Monthly model

A.3 Alternative sample design

As discussed in Section 2, the data we use for the analysis presented in this article are selected based on a cohort-based sampling approach. More specifically, we use a random sample of individuals who turn 18 years in January of the years 1993 to 2008. We then follow these individuals for a period of eleven years unless in cases where they leave the data set earlier due to emigration or death. For brevity, we refer to this sample as the 'cohort sample'. The advantage of selecting the sample like we do is that we observe all individuals from the start of their 'welfare careers', which permits us to calculate their total number of previous welfare benefit spells at any point in time. The disadvantage of this approach is that this sample is clearly not representative of the overall Norwegian population and that our sample selection criteria differ from those used in the earlier work on welfare benefit dynamics.

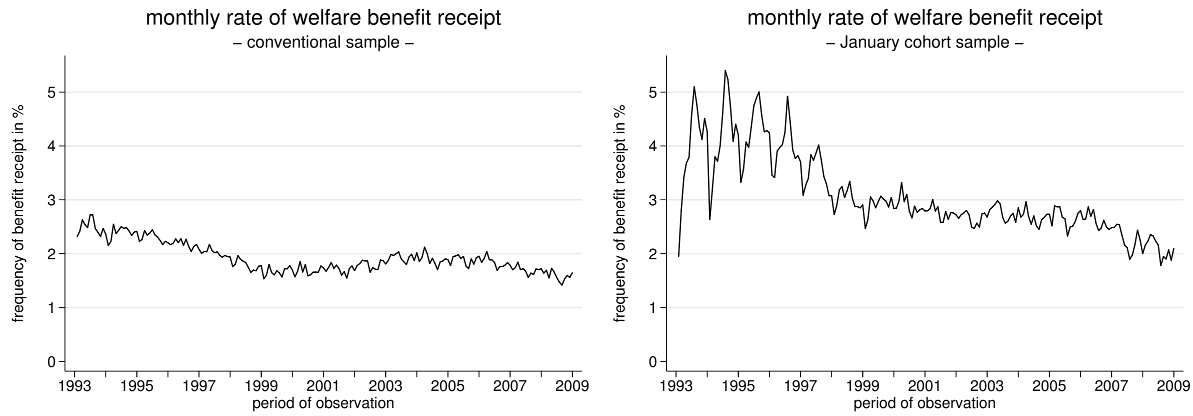
To illustrate that our main results are not driven by these non-standard sample selection criteria, we replicate the analysis presented in Sections 4.1 and 4.2 using a standard 5% random sample of 25-60 year-olds from the entire Norwegian population. We refer to these data as the 'conventional sample'. In Table A.7, we present descriptive statistics of the main variables used in our analysis for these two samples. Figures A.1 to A.5 provide further descriptive evidence on the welfare dynamics for the two samples. In Tables A.8 and A.9, we then give the results of a replication of our earlier time aggregation exercise.

Table A.7: Descriptive statistics for the two samples

	Conventional sample	Cohort sample
% of welfare recipients	2.0 (13.9)	3.0 (17.1)
% of females	47.9 (50.0)	48.7 (50.0)
% of migrants	7.5 (26.3)	4.0 (19.7)
age (in years)	41.5 (9.8)	22.9 (2.9)
years of education	11.8 (3.9)	12.2 (2.4)
household size	2.9 (1.4)	3.1 (1.5)
% of singles without children	25.6 (43.6)	24.0 (42.7)
% of singles with children	9.3 (29.0)	16.0 (36.7)
% of couples without children	10.4 (30.5)	1.9 (13.7)
% of couples with children	54.7 (49.8)	58.0 (49.4)
# of individuals	157,818	33,134
# of observations	14,330,704	3,653,064

Note: Mean values have been calculated across all observations in the data sets; standard deviation in parentheses.

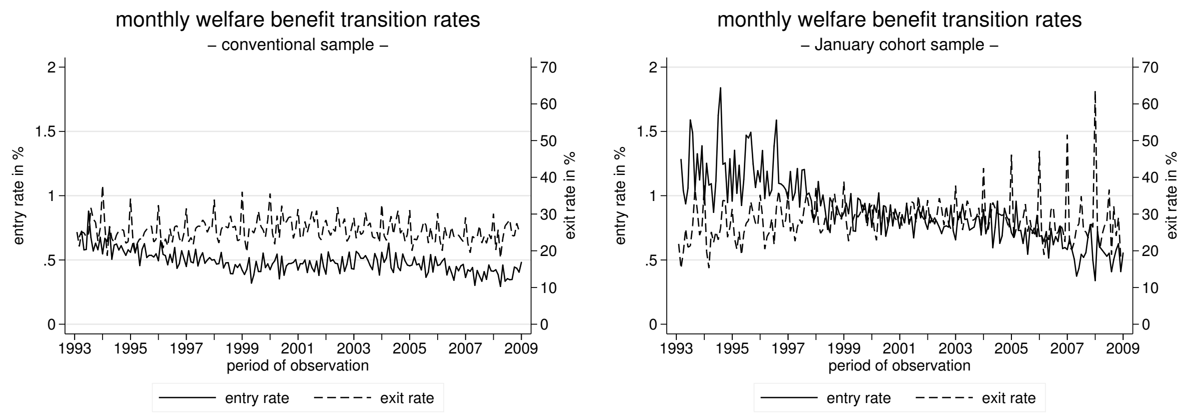
The numbers presented in A.7 suggest that the two samples are generally relatively similar in terms of personal and household characteristics. By construction, individuals in our 'cohort sample' are much younger on average, and we find that they have a one percentage-point higher probability of receiving welfare benefits. The proportion of migrants in this sample is lower, which likely reflects the fact that many immigrants come to Norway only as adults and are therefore not included in the restricted sample. Individuals in the 'cohort sample' are much less likely to live in a coupled household without children.



Source: FD-Trygd 1993-2008; calculations by the authors based on *(a)* a 5% random sample of 25-60 year-olds (conventional sample) and *(b)* the population of individuals who turn 18 years old in January of the years 1993 to 1998 (cohort sample)

Figure A.1: rates of welfare benefit receipt for the two samples

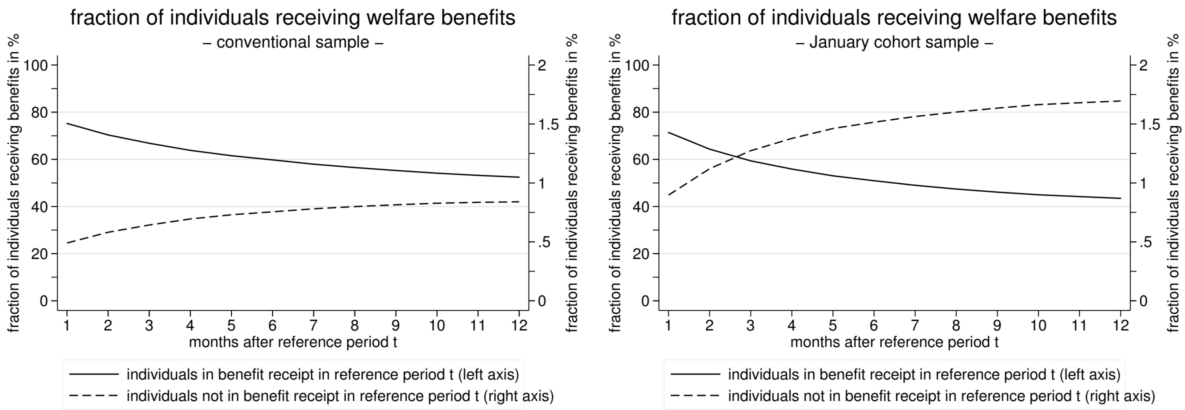
Rates of benefit receipt plotted in Figure A.1 confirm the result from Table A.7 that the frequency of benefit receipt is about 50% higher in the 'cohort sample' compared to the 'conventional sample'. The high volatility of welfare benefit receipt rates for the January-borns at the beginning of the observation period reflects seasonal fluctuations in benefit receipt among under-25 year-olds.



Source: FD-Trygd 1993-2008; calculations by the authors based on *(a)* a 5% random sample of 25-60 year-olds (conventional sample) and *(b)* the population of individuals who turn 18 years old in January of the years 1993 to 1998 (cohort sample)

Figure A.2: welfare benefit transition rates for the two samples

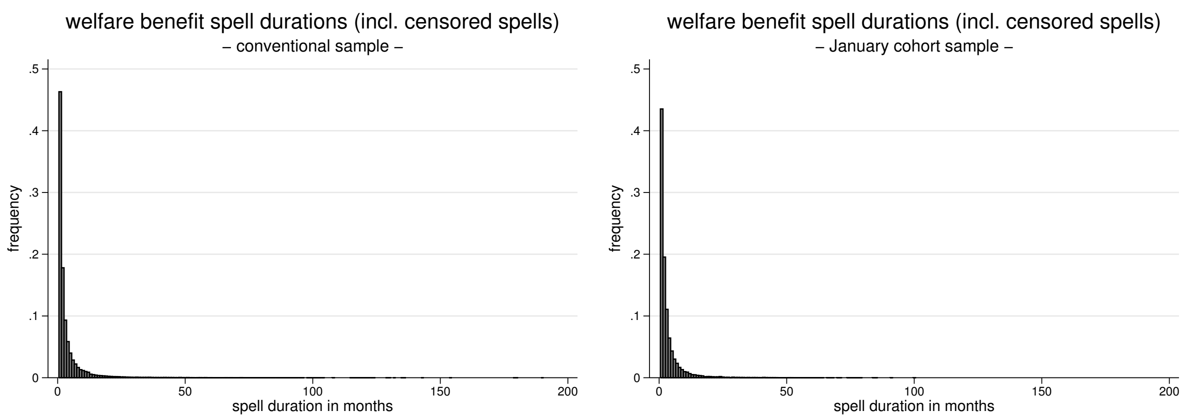
Figure A.2 gives on average slightly higher monthly benefit entry and exit rates for the sample of January-borns compared to the conventional sample. Unconditional ('raw') state dependence is 73.9 percentage points for the 'cohort sample' and 70.7 percentage points for the conventional sample. The higher volatility of benefit transition rates for the January-born sample is due to the smaller sample size and the stronger seasonality in benefit receipt.



Source: FD-Trygd 1993-2008; calculations by the authors based on *(a)* a 5% random sample of 25-60 year-olds (conventional sample) and *(b)* the population of individuals who turn 18 years old in January of the years 1993 to 1998 (cohort sample)

Figure A.3: welfare benefit persistence rates for the two samples

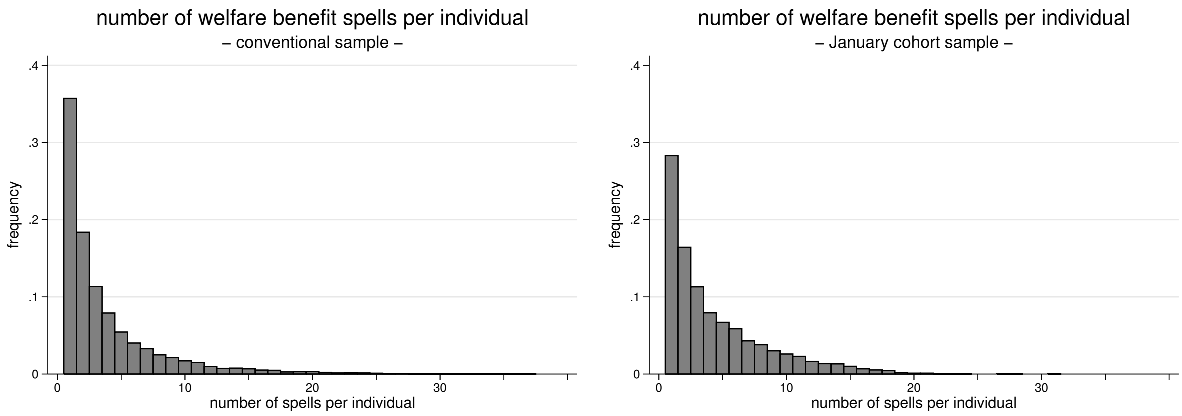
The low benefit transition rates plotted in Figure A.2 imply strong observed persistence in and off welfare benefit receipt. As indicated in Figure A.3, between 40% and 50% of benefit recipients in period t will still (or again) receive benefits 12 months later. By contrast, only about 0.75% and 1.75% of non-recipients in period t in the conventional sample and the sample of January-borns will receive benefit payments 12 months later. So, today's benefit receipt status is a strong predictor of benefit receipt 12 months from now.



Source: FD-Trygd 1993-2008; calculations by the authors based on *(a)* a 5% random sample of 25-60 year-olds (conventional sample) and *(b)* the population of individuals who turn 18 years old in January of the years 1993 to 1998 (cohort sample)

Figure A.4: welfare spell duration for the two samples

Despite the strong observed persistence in benefit receipt status, the large majority of welfare benefit spells is very short. For our sample of January-borns, we calculate a median spell duration of 2 months and a mean spell length of 3.4 months. Median and mean durations in the conventional sample are 2 months and 3.9 months, respectively. Note that these numbers are slight underestimates of true spell durations due to a small share of censored spells.



Source: FD-Trygd 1993-2008; calculations by the authors based on *(a)* a 5% random sample of 25-60 year-olds (conventional sample) and *(b)* the population of individuals who turn 18 years old in January of the years 1993 to 1998 (cohort sample)

Figure A.5: repeat welfare benefit receipt in the two samples

Figure A.5 illustrates that repeat benefit receipt is frequent. In the sample of January-borns, median and mean spell numbers per individual are 3 and 4.4, respectively. For the conventional sample, we calculate a corresponding median of 2 spells and a mean of 3.9 spells. The conventional sample is however strongly unbalanced and many individuals are observed for less than the full 192 monthly waves.

In Section 4, we tested the time aggregation properties of the Markov model for the sample of January-borns. Most earlier studies of welfare benefit dynamics however use a very different sample design. We therefore check whether the standard model's failure to aggregate up over time described in Section 4 is an artefact of our non-standard sample selection. We do so by replicating the analysis presented in the main text for a representative random sample of 32,000 individuals selected from the 25-60 year-olds. Table A.8 presents the results from this exercise.

Table A.8: Time aggregation: Monthly to Annual ('conventional sample')

	Monthly model	Annual model	Monthly-to-annual time aggregations
			Analytical
	(1)	(2)	(3)
Avg. predicted persistence rate (in %)	10.6 (0.2)	6.8 (0.3)	2.7 (0.1)
- Avg. predicted entry rate (in %)	1.3 (0.1)	1.8 (0.0)	2.6 (0.1)
= Average treatment effect (in ppts)	9.3 (0.1)	5.0 (0.3)	0.1 (0.0)

Note: The monthly model (1) and annual model (2) refer to 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 results presented in column (3) were obtained by aggregating up the transitions of the monthly model to the annual level using the analytical approaches described in Section 3.2. Standard errors in parentheses were calculated using 15 bootstrap replications. Coefficient estimates of the two specifications are reported in Table A.9. Results have been calculated based on a 5% random sample of individuals aged 25-59 years from the Norwegian population. * p<0.10, ** p<0.05, *** p<0.01.

We find that predicted transition rates and the resulting average treatment effects for the different specifications are slightly different from those of our preferred sample. For the conventional sample, we calculate slightly lower average predicted persistence and entry rates both at the monthly and annual level, and also somewhat lower average treatment effects. However, the month-to-year time aggregation gives nearly identical results. We therefore conclude that our conclusions are not affected by the sample design. Coefficient estimates for the standard Markov specifications estimated using the conventional sample are provided in Table A.9.

	MONTHLY MODEL		ANNUAL MODEL	
y_{t-1}	1.905***	(0.007)	1.002***	(0.024)
individual characteristics				
female	0.068***	(0.006)	-0.108***	(0.027)
immigrant	1.073***	(0.294)	0.664***	(0.045)
age	-0.099***	(0.023)	0.029***	(0.011)
age^2	0.792***	(0.041)	-0.045***	(0.014)
years of education	-0.032***	(0.005)	0.066***	(0.010)
years of education^2	0.034***	(0.006)	-0.007***	(0.001)
household characteristics				
single, with children	-0.007**	(0.003)	0.024	(0.045)
couple, no children	2.245***	(0.046)	-0.377***	(0.068)
couple, with children	0.099***	(0.017)	-0.118**	(0.059)
child aged 0-5 years	-0.520***	(0.026)	0.003	(0.035)
household size	-0.108***	(0.022)	0.045**	(0.018)
spouse characteristics				
immigrant	0.080***	(0.008)	0.082	(0.055)
age	-0.008***	(0.000)	-0.005	(0.004)
age^2	0.105***	(0.032)	0.014**	(0.007)
years of education	-0.003	(0.002)	0.020	(0.012)
years of education^2	0.008**	(0.003)	-0.003***	(0.001)
year dummies				
1995	-0.038**	(0.015)	-0.151***	(0.037)
1996	-0.057***	(0.016)	-0.146***	(0.040)
1997	-0.065***	(0.017)	-0.217***	(0.045)
1998	-0.121***	(0.020)	-0.207***	(0.052)
1999	-0.167***	(0.023)	-0.249***	(0.055)
2000	-0.201***	(0.023)	-0.278***	(0.054)
2001	-0.202***	(0.023)	-0.300***	(0.055)
2002	-0.216***	(0.025)	-0.242***	(0.059)
2003	-0.181***	(0.025)	-0.252***	(0.055)
2004	-0.204***	(0.024)	-0.237***	(0.052)
2005	-0.258***	(0.024)	-0.282***	(0.054)
2006	-0.278***	(0.026)	-0.361***	(0.058)
2007	-0.315***	(0.028)	-0.399***	(0.061)
2008	-0.365***	(0.029)	-0.453***	(0.063)
Wooldridge controls				
y_0	-0.450***	(0.031)	1.398***	(0.045)
single, with children	0.001	(0.007)	0.186**	(0.072)
couple, no children	-0.001**	(0.000)	-0.561***	(0.094)
couple, with children	0.515***	(0.050)	-0.793***	(0.092)
child aged 0-5 years	-0.288***	(0.058)	-0.015	(0.064)
household size	-0.447***	(0.037)	0.088***	(0.026)

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Table A.9 – continued from previous page –

local unemp. rate	0.112**	(0.046)	2.350**	(0.987)
local unemp. rate	-0.090***	(0.014)	0.906	(0.815)
constant	-2.847***	(0.115)	-3.009***	(0.227)
σ_a	1.250***	(0.013)	0.910***	(0.020)
ρ	0.610***	(0.005)	0.453***	(0.011)
log Likelihood	-110,714.944		-18,751.860	
# of observations	2,903,732		278,737	
# of individuals	32,000		32,000	

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

note: standard errors in parentheses. The dependent variable measures receipt of welfare 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 y_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. Results have been calculated based on a 5% random sample of individuals aged 25-59 years from the Norwegian population.

Table A.9: The standard Markov specification, conventional sample: Monthly and Annual model

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