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STATE DEPENDENCE IN WELFARE BENEFITS IN A NON-WELFARE CONTEXT

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This study investigates state dependence in social assistance benefits in Turkey, where benefit receipt and persistence rates have significantly increased over the past decade. We estimate state dependence through dynamic random-effects probit models, controlling for observed and unobserved heterogeneity, and endogenous initial conditions. In particular, we employ Wooldridge's estimator to achieve consistent and correct estimates of state dependence and compare the results with estimates from Heckman's reduced-form approach as a sensitivity check. Both estimators enable us to disentangle true state dependence from its spurious components and address the potential bias due to the short panel length. Our results suggest that the receipt of benefits in the last year increases the likelihood of benefit receipt in the current year, namely the structural state dependence, by 17.2–19.5 percentage points.

JEL Codes: I38, J18, C23

Keywords: social assistance benefits, state dependence, endogenous initial conditions, dynamic random-effects models

1. INTRODUCTION

There is an ongoing debate in the welfare economics literature on benefit dependency. The discussions revolve around countries with generous social assistance schemes, such as Canada, Germany, the United Kingdom (U.K.), and the Scandinavian countries. The central question in this literature is whether the generosity of the social assistance system creates dependence in benefit receipt; in other words, whether the receipt of benefits in the current period makes the

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beneficiary more likely to receive future benefits. In technical terms, the debate attempts to ascertain the *structural (genuine) state dependence* in benefit receipt, net of observed and unobserved individual characteristics. Empirical evidence suggests considerable state dependence in the aforementioned countries that are considered for discussion in this matter (Andren and Andren, 2013; Cappellari and Jenkins, 2014; Hansen *et al.*, 2014; Königs, 2014), with the exception of Riphahn and Wunder (2016).

The related literature from developing countries, mostly from Latin America and Africa, mainly focuses on the evaluation of anti-poverty social transfer programs (see, Baird *et al.*, 2011; Duflo, 2003; Edmonds and Schady, 2012; Manacorda *et al.*, 2009). To the best of our knowledge, none of the studies in this sparse literature has attempted to investigate the dynamics of social assistance benefits. This could be partly because state dependence is not expected to be an issue in developing countries, given the short spell of the benefits, and partly because of the unavailability of longitudinal data. Our study contributes to the literature by analyzing the dynamics of social assistance benefits within a state-dependence framework in the context of a developing country.

Turkey is an interesting case for the analysis of welfare benefits, because the role of social assistance in the welfare and political arena has increased over the past decade.[1] According to Ministry of Finance records, social expenditures in Turkey financed by public sources have increased 15-fold since 2002 and reached 32.9 billion Turkish liras (about 10.1 billion euros) in 2014. Nevertheless, the ratio of social expenditures to gross domestic product (GDP) (of 1.73 percent in 2014) is still below the OECD average (of 2.3 percent) (OECD, 2014). The share of family-based social transfers in public expenditure is only 0.2 percent in Turkey, while this ratio reaches 3.9 percent in the United States (U.S.) (Immervoll, 2010).[2] Despite Turkey's relatively ungenerous welfare regime, the high dependence in benefit receipt is an observed phenomenon. As of 2015, 3 million households, accounting for 15.6 percent of the total number of households, receive some type of social transfer.[3] The welfare participation rate has been steadily increasing, as shown in Figure 1, contrary to the downward trend in developed countries such as Canada, the U.K., and the U.S.[4] This increase is associated with a remarkably high rate of persistence, around 80 percent annually, despite a relatively low level of and constant trend in the entry rate (see Figures 1 and 2).[5]

In this study, we aim to quantify the degree of *structural state dependence* in Turkey so that we can determine the extent to which the high persistence rate

[1]The literature on Turkey also focuses on the role of social assistance benefits in the alleviation of poverty. See Buğra (2009) and Şeker and Dayioğlu (2015).

[2]It is also reported that total public expenditure as a part of GDP is 13.7 percent in Turkey, which is clearly below the OECD average, of 20.6 percent, and the share in the U.S., of 15.9 percent (Immervoll, 2010).

[3]For reference, see http://www.maliye.gov.tr/KonusmaSunumlari/SunumMerkezi/index.html?kt-p=2015YBSK, retrieved on November 24, 2015.

[4]See Hansen *et al*. (2014), Cappellari and Jenkins (2008), and Scholz *et al*. (2009) for Canada, the U.K., and the U.S., respectively.

[5]The persistence rate is defined as one minus the exit rate. Note also that Figure 1 shows the total share of the working-age population in receipt and the shares by benefit type. The shares by benefit type exceed the total share because some of the beneficiaries receive different types of benefits at the same time.

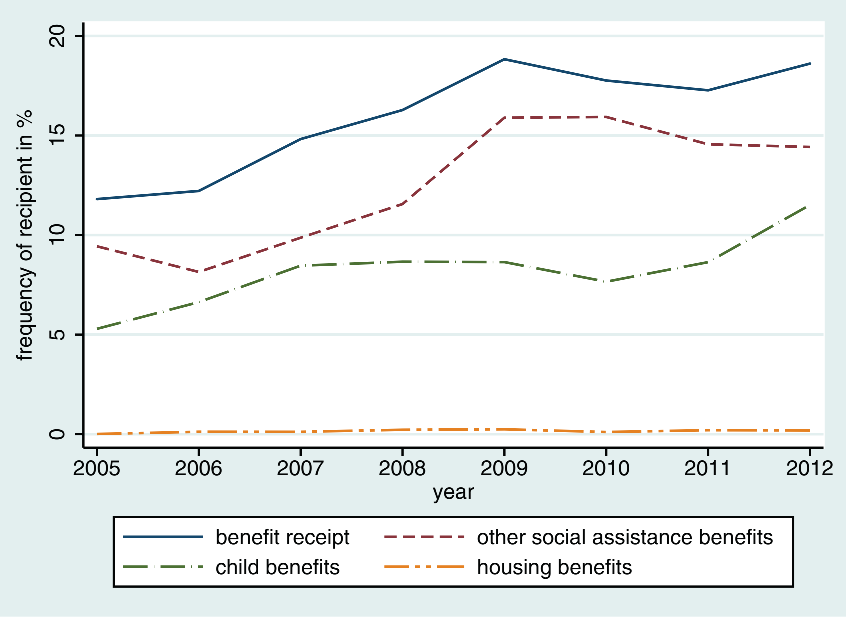

Figure 1. The Rate of Benefit Recipients of the Working-Age Population (Age 15–64)
Notes: The rate of benefit recipients refers to the share of working-age individuals from a benefit-receiving household. It is calculated using individual sampling weights based on micro data from the SILC. [Colour figure can be viewed at wileyonlinelibrary.com]

observed can be explained by the *genuine* component. Accordingly, the hypothesis we attempt to test is that the high persistence rate in social assistance benefit receipt in Turkey can be partly attributed to structural state dependence. In other words, observed and unobserved individual characteristics cannot completely account for the high rate of persistence in benefit receipt. If, however, the high dependence arises from observed and unobserved characteristics of individual factors, policies would be less effective in inducing exits from social assistance and subsequently reducing persistence and state dependence (Hansen and Lofstrom, 2009). To test the hypothesis, we employ a series of dynamic random-effects probit models that facilitate controlling for unobserved heterogeneity. We use annual longitudinal data from the Survey of Income and Living Conditions for the period 2006–12. Identification of structural state dependence emphasizes the need to handle endogenous initial conditions, which, if undetected, could lead to bias in parameter estimates. We address this problem by using two empirical methods, proposed by Wooldridge (2005) and Heckman (1981), respectively. We also implement an alternative specification of Wooldridge's estimator, as proposed by Rabe-Hesketh and Skrondal (2013), and test whether our results are biased due to the short time span of the panel.

We find significant evidence of state dependence in social assistance benefit receipt, even after controlling for unobserved heterogeneity and endogenous initial conditions. Benefit receipt in the current year is found to increase the likelihood of receiving benefits next year by an average of 17.2–19.5 percentage points. This finding

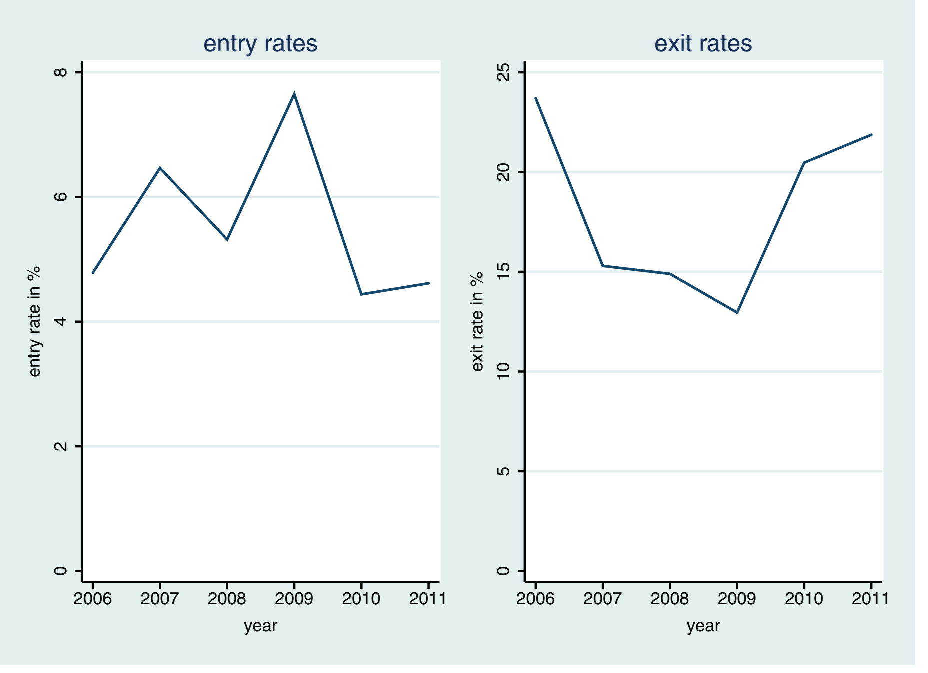

Figure 2. Benefit Transition Rates of the Working-Age Population (Age 15–64)

Notes: The *entry rate* is defined as the number of recipients at time t among those who were not in receipt at time $t-1$ divided by the total number of individuals not in receipt at time $t-1$. The *exit rate* is the number of non-recipients at time t among those who were in receipt at time $t-1$ divided by the total number of individuals in receipt at time $t-1$. The rates are calculated using individual sampling weights based on micro data from the SILC. [Colour figure can be viewed at wileyonlinelibrary.com]

is at least 3 percentage points higher than the estimated state dependence (14.4 percent) in the U.K. and 4 percentage points higher than the estimated state dependence (13 percent) in Germany (Cappellari and Jenkins, 2014; Königs, 2014). The persistence rate is also estimated to be higher, whereas the study finds a substantially lower entry rate in Turkey relative to the U.K. and Germany. The results are quite similar across the estimators of Wooldridge (2005) and Heckman (1981). The consistent evidence of state dependence independent of the choice of estimator ensures the feasibility of a state-dependence analysis based on a short panel, which is particularly important for developing countries that lack long panel data. Taken together, the strong evidence of structural state dependence in benefit receipt in Turkey points out the potential for successful policy reform that would reduce the persistence rate.

2. Data

2.1. *Data and Sample Selection*

The data for the analysis of state dependence in social assistance benefit receipt are obtained from the Survey of Income and Living Conditions (SILC), a representative longitudinal survey of households in Turkey. The panel was initiated in 2006 and the latest survey was made available in 2012. The survey is designed as a rotating panel in which the sample of households and corresponding individuals are tracked annually for four consecutive years. The panel's structure design facilitates replacement of one fourth of the sample by a new group each year; thus three fourths of the sample remain unchanged with respect to the previous year.

The SILC involves detailed information on demographic (e.g. age, gender, education, and marital status), labor force (e.g. employment status, previous work information, and income), and household characteristics. All the members in a sample household are individually interviewed and one of the household members (i.e. the *reference person*) fills out an additional questionnaire regarding household characteristics. This household-level survey provides relevant information related to social assistance benefits. We conduct an individual-level regression analysis based on the reference persons, extracting the benefit receipt information from the household's recipient status. Households are used as the unit of analysis in comparable studies by Hansen *et al*. (2014) and Riphahn and Wunder (2016).

Our outcome variable of interest indicates whether the reference person within a household receives benefits. We focus on social assistance schemes aimed at income maintenance rather than income replacement. In particular, we exclude contribution-based social assistance schemes such as unemployment benefits, maternal benefits, sickness allowance, and retirement pension from the analysis. Therefore, to construct the outcome variable, we examine the questions regarding non-contributory social transfers received by households, including family and child allowances, housing benefits, and other social benefits in cash and kind.

The panel used for our analysis, beginning in 2006, consists of seven waves. However, as mentioned above, every individual can at the most be observed for four consecutive years. As a focus of the state-dependence analysis, the study examines reference persons who were observed for at least two consecutive years during the sample period. The sample is restricted to the working-age population (aged 15–64) in order to rule out complications regarding entry into the labor market and the old-age pension scheme. The analysis also excludes individuals in full-time education. We end up with a final sample of 3,450 individuals (10,239 observations) in a *balanced panel* in which we can observe each individual over a 4-year period. On the other hand, we have a final sample of 14,383 individuals (25,222 observations) in an *unbalanced panel* in which each individual can be observed either two, three, or four consecutive years.

Our main specifications rely on the balanced panel for the methodological reasons discussed in Section 4, although we also present, in the Appendix (in the Online Supporting Information), the estimation results based on the unbalanced panel. We check the extent to which the balanced panel is representative of the population by first comparing the observable characteristics of individuals between the balanced and unbalanced samples. The summary statistics presented in the upper panel of Table 1 show that the two samples are very similar in terms of observed covariates and that the differences in the means of the covariates are not statistically significant.[6]

[6]We also compare the transition rates between the balanced and unbalanced samples to reaffirm the representativeness of the balanced panel. The transition rate from being a non-recipient to a recipient is about 17 percent in the balanced panel and 18 percent in the unbalanced panel. The results are available upon request.

TABLE 1
SUMMARY STATISTICS FOR BALANCED VERSUS UNBALANCED PANEL AND ATTRITERS VERSUS NON-ATTRITERS

	Balanced				
	Obs.	Mean	SD	Min.	Max.
Benefit recipient	10,239	0.18	0.386	0	1
Age	10,239	45.41	9.534	24	64
Female	10,239	0.01	0.101	0	1
Years of schooling	10,234	7.51	4.404	0	16
Spouse's education	10,192	5.32	4.446	0	16
Number of children	10,239	1.67	1.550	0	12
Household size	10,239	4.59	1.940	1	19
Health restriction	10,234	0.24	0.427	0	1
Non-employed	10,239	0.21	0.407	0	1
	Unbalanced				
Benefit recipient	25,222	0.18	0.380	0	1
Age	25,222	44.97	9.975	17	64
Female	25,222	0.02	0.128	0	1
Years of schooling	25,205	7.64	4.480	0	16
Spouse's education	25,079	5.50	4.568	0	16
Number of children	25,222	1.64	1.561	0	18
Household size	25,222	4.48	1.981	1	30
Health restriction	25,205	0.24	0.427	0	1
Non-employed	25,222	0.21	0.405	0	1

	Non-attriters			Attriters		
	Obs.	Mean	SD	Obs.	Mean	SD
Benefit recipient	14,539	0.18	0.383	10,683	0.17	0.376
Age	14,539	44.99	9.765	10,683	44.94	10.254
Female**	14,539	0.02	0.122	10,683	0.02	0.136
Years of schooling**	14,527	7.59	4.432	10,678	7.72	4.543
Spouse's education***	14,465	5.41	4.506	10,614	5.63	4.647
Number of children	14,539	1.65	1.548	10,683	1.63	1.579
Household size***	14,539	4.52	1.948	10,683	4.43	2.023
Health restriction	14,527	0.24	0.425	10,678	0.24	0.428
Non-employed	14,539	0.21	0.404	10,683	0.21	0.405

Notes: The top and middle panels of the table show respective summary statistics for balanced- and unbalanced-panel samples, based on which regression analyses are conducted. The bottom panel of the table displays the statistics for the sample of individuals who stay in the survey over the 4-year panel period (called "non-attriters") and those who are not observed across the complete panel period (called "attriters"). The individuals with missing data, the so-called "attriters," might appear in the panel in either 2 or 3 years over the observation period. The asterisks (*) by the variable names denote the observable characteristics that differ significantly and the level of significance of the difference:
***p-value < 0.01; **p-value < 0.05; *p-value < 0.1.

The difference between the unbalanced- and balanced-panel samples is composed of those who dropped out of the sample, the so-called "attriters." An inspection of Table 1 reveals that attriters account for about 40 percent of the unbalanced sample. This finding is unsurprising given the rotating panel design of the dataset. The bottom panel of Table 1 displays statistics for both attriters and non-attriters, that is, those without missing data.[7] The difference in observables between these

[7]Note that the unbalanced-panel sample is composed of attriters and non-attriters, whereas the balanced panel corresponds to the set of non-attriters.

two subsamples is statistically insignificant for many of the variables, such as the benefit recipient dummy, age, number of children, and health restrictions, while significant for others, such as the gender dummy, education, and household size.

2.2. *Institutional Background and Descriptive Statistics*

Social assistance schemes in Turkey are mainly coordinated by the central government, local authorities appointed by the central government, or municipalities. The key governmental institution responsible for the coordination is the Social Assistance and Solidarity Fund (SASF). The SASF was established to work in conjunction with regional associations that are located in each subprovince. There are currently 973 local associations that receive a regular monthly budget from the SASF (Aytaç, 2014; Metin, 2011). The selection of beneficiaries is under the responsibility of these local associations. The benefits are allocated on the basis of "neediness," which is determined through a proxy-means test. The details of the proxy-means test (namely, the poverty-scoring formula) are not disclosed by the SASF. Individual criteria are applied by every association to determine the neediness of beneficiaries. The executive committees formed under every association of the subprovince execute their decisions independently. The autonomy exercised by centrally appointed bureaucrats of the local executive committees leaves ample room for discretion, particularly for political preferences, in determining eligibility for the benefits (Adaman *et al*., 2007; Aytaç, 2014).

While these committees do not adhere to the norms in determining the neediness of beneficiaries, the law provides a tacit definition for the term "needy." The individuals who are not covered by any social security institution and do not have monthly income, or those with a per capita income lower than one third of the net minimum wage, are considered as needy (Law 3294, 1986). This threshold is *de jure* the eligibility criterion for free-health-care beneficiaries (namely, green-card holders). However, a nationalized and binding poverty-scoring formula based on a settled threshold does not exist for other social transfers. While applicants with scores below a certain threshold (determined by local committees) become officially eligible, applicants with poverty scores above the threshold are not automatically excluded from consideration, and they can still be regarded eligible at the discretion of the executive committee (Aytaç, 2014).

The SILC does not reveal information on the providers of benefits, but it does reveal the types of benefits. This means the information on benefits exploited in our analysis could refer to either public or private social assistance. We group the benefits into three categories. The first category is *child benefits*, comprising cash and in-kind transfers related to children's health care and education. The second category is *housing benefits*, which involve cash allowances related to repair and reconstruction. These benefits play a significant role in certain cases, such as earthquakes, food shortages, and mining accidents. The number of respondents reporting the receipt of housing benefits is negligible in our sample (less than 1 percent; see Figure 1). The third category comprises all *other social assistance benefits*, in cash and in kind, that are not counted in the first two categories. The incidence of other social assistance benefits is clearly the highest of all three types. Figure 1 also presents the overall trend of the share of recipients in the total working-age population from 2005 to 2012. The rate of social assistance benefit recipients steadily increases until 2009 and has shown a relatively constant trend since then. It peaks at 18.8 percent just after the global economic crisis in 2008 and does not fall significantly in the post-crisis period.[8]

The summary statistics presented in Table 2 reveal substantial variation in the amount of annual social transfers across households, from 15 to 20,520 Turkish liras. The ratio of social transfers to net household income is about 10 percent, on average, with a sizable standard deviation. Household size and the number of children in the household are notably higher among benefit recipients than among non-recipients. The personal characteristics of benefit recipients and non-recipients also differ significantly. Female and unemployed household heads are more likely to receive social transfers. In line with expectations, the educational level of household heads and their spouses is lower among recipient households relative to non-recipient households. The share of individuals whose daily life is restricted due to health problems constitutes about 39 percent of the recipients, whereas it is only 22 percent among non-recipients.

Lastly, Figure 2 displays the annual transition rates into and out of benefit receipt. The entry and exit rates exhibit opposite trends over the period. The pattern is more apparent during the recovery period of the 2008 crisis, that is, a decline in the entry rate is accompanied by an increase in the exit rate after 2009. The observed transition rates provide evidence about "raw" state dependence in social assistance receipt, namely, the difference between persistence rate (i.e. 1− exit rate) and the entry rate. The persistence rate of around 80 percent and the entry rate of around 5 percent indicate that three out of every four recipients in a given year continue to receive benefits the next year.

The raw state dependence could be due to observed and unobserved characteristics as well as structural features of the social assistance system. The main objective of this paper is to analyze the extent to which the raw state dependence is structural. In this regard, a regression analysis is conducted in the following section to disentangle the structural state dependence from its spurious components.

3. Empirical Method

A dynamic random-effects probit model, which is largely cited in recent empirical work, is employed in the current study to analyze state dependence in social assistance benefit (see, Andren and Andren, 2013; Cappellari and Jenkins, 2014; Hansen *et al.*, 2014; Königs, 2014). The model has also been applied to other binary outcomes, such as poverty, labor-force participation, and unemployment (see, Arulampalam and Stewart, 2009; Biewen, 2009; Chay and Hyslop, 2014; Stewart, 2007). This section introduces the model mainly on the basis of these cited studies.[9]

[8]It is worth noting that the period denoting an upward trend in benefit recipients coincides with positive economic growth, except for 2009.

[9]We are aware of the limitations of non-linear models in dealing with unobserved heterogeneity. An alternative would be the use of an Arellano–Bond estimator in a linear regression framework. However, our data limitations due to the usage of a short panel restrict its feasibility. More specifically, given the panel span of 4 years, we only have enough observations to check the differenced errors for the first-order autocorrelation and are unable to check the second-order autocorrelation.

TABLE 2
SUMMARY STATISTICS FOR BENEFIT RECIPIENTS VERSUS NON-RECIPIENTS

	(1) Mean	(2) SD	(3) Min.	(4) Max.	(5) Obs.
Benefit recipient rate	0.18	0.38	0	1	15,351
Annual benefit in Turkish liras (at household level)	644	943	15	20,520	2,758
Benefit share in net household income	0.10	0.22	0	5.96	2,758
Individual Characteristics of Benefit Recipients					
Age	44.2	9.36	19	64	2,758
Female	0.12	0.33	0	1	2,758
Married	0.89	0.31	0	1	2,758
Years of schooling	4.46	3.34	0	16	2,750
Spouse's education	2.65	3.10	0	16	2,390
Household size	5.57	2.45	1	19	2,758
Number of children	2.75	1.94	0	12	2,758
Health restriction	0.39	0.49	0	1	2,750
Non-employed	0.28	0.45	0	1	2,758
Individual Characteristics of Non-recipients					
Age	45.5	9.72	19	64	12,593
Female	0.08	0.28	0	1	12,593
Married	0.91	0.29	0	1	12,593
Years of schooling	7.98	4.53	0	16	12,586
Spouse's education	5.90	4.48	0	16	11,285
Household size	4.07	1.74	1	19	12,593
Number of children	1.32	1.31	0	11	12,593
Health restriction	0.22	0.43	0	1	12,593
Non-employed	0.24	0.43	0	1	12,593

Notes: Statistics are produced on the basis of an appended sample of two balanced panels of 2006–9 and 2009–12. *Benefit recipient rate* refers to the share of social assistance beneficiaries in the working-age population (aged 15–64). *Net household income* refers to the total household income minus social assistance benefits. Individual characteristics belong to reference persons in households.

The latent equation for the binary outcome variable for receiving social assistance is specified as follows:

$$
\begin{aligned}
y_{it} &= 1\{y_{it}^{*} > 0\} \\
&= 1\{\beta_0 + \beta_1 y_{it-1} + X_{it}'\Omega + \alpha_i + u_{it} > 0\}, \quad \text{for } i = 1, \dots, N; t = 2, \dots, T,
\end{aligned}
\tag{1}
$$

where y_{it} is the observed binary outcome variable indicating whether the individual is a benefit recipient and 1(.) is an indicator function equal to one if the latent variable $y_{it}^{*} > 0$ and zero otherwise. In other words, each individual i is observed to be a benefit recipient in year t if the indicator function is equal to one and not a recipient if it is zero. The latent variable, to be interpreted as the potential utility from receiving social assistance, depends on the lagged dependent variable (y_{it-1}), observable characteristics (X_{it}), unobserved individual-specific random effects (α_i), and a white-noise error term (u_{it}). The vector X_{it} includes the reference person's characteristics, such as gender, age, age squared, own and spouse's years of schooling, health restrictions, employment status, number of children, and household size.

The white-noise error term is assumed to be serially uncorrelated[10], independent of X_{it} and y_{it-1}, and normally distributed. Even if the errors u_{it} are assumed to be serially uncorrelated, the composite error term, $v_{it} = \alpha_i + u_{it}$, would be correlated over time due to the individual-specific time-invariant α_i terms. The correlation between the composite error terms from any two different periods t and s is assumed to be the same: $\rho = \mathrm{Corr}(v_{is}.v_{it}) = \sigma_\alpha^2/(\sigma_\alpha^2 + 1)$ for $t, s = 2, \ldots, T$; $t \neq s$ and $\sigma_u^2 = 1$. It is further assumed that the two error components, α_{it} and u_{it}, have zero mean and are uncorrelated with each other, the dynamic structure of benefit receipt is approximated by a first-order Markov model, and the covariates (X_{it}) are strictly exogenous.

Under these conditions, the probability that individual i receives social assistance at time t (t>1), conditional on y_{it-1}, X_{it}, and α_i, is as follows:

$$\Pr(y_{it} = 1 | y_{it-1}, X_{it}, \alpha_i) = \Phi(\beta_0 + \beta_1 y_{it-1} + X_{it}'\Omega + \alpha_i), \tag{2}$$

where $\Phi(.)$ is the standard normal cumulative distribution function.

The standard random-effects model assumes α_i to be uncorrelated with X_{it}. Alternatively, the *Mundlak–Chamberlain* approach is employed, which allows for correlation between the unobserved individual-specific effect α_i and observed characteristics X_{it} in the model. This correlation is obtained by supposing a relation between α_i and either time-averaged characteristics, also known as Mundlak averages, or a combination of the variables' lags and leads. Several of the aforementioned studies, such as those of Cappellari and Jenkins (2008) and Königs (2014), use time averages ($\bar{X}_i$), describing $\alpha_i = \bar{X}_i' a + \zeta_i$, where $\zeta_i \sim N(0, \sigma_\zeta^2)$. The individual characteristics that are left in ζ_i are supposed to be independent of X_{it} and u_{it} for all i, t.

The coefficient estimate of the lagged dependent variable β_1 is the parameter of interest. To obtain the structural state dependence, one must distinguish it from the spurious components that are induced by observed and unobserved characteristics. Failure to control for unobserved heterogeneity, such as unobserved labor-market ability or individualistic preferences, could lead to spuriously high state dependence, that is, overestimation of β_1 (Königs, 2014). The implementation of controls for observed and unobserved heterogeneity (via X_i and α_i, respectively) eliminates the spurious components and yields the structural state dependence.

Estimation of the structural state dependence requires an additional assumption about the initial conditions: the need to specify the relation between the individual-specific effect α_i and the dependent variable in the initial period y_{i1}, which typically cannot be treated as exogenous. Unless the start of the process coincides with the start of the observation period for each individual—and this is not the case—a correlation exists between α_i and y_{i1}. This would cause the lagged dependent variable to be correlated with the composite error term, leading to a bias in parameter estimates. In particular, the estimator of a standard random-effects

[10]Following the previous studies using a similar method, we assume that the error term is not correlated with its past values (see, Cappellari and Jenkins, 2014; Hansen and Lofstrom, 2009; Hansen *et al.*, 2014; Königs, 2014). There have also been extensions of the model that drop this assumption (see, Hyslop, 1999; Stewart, 2007).

probit model that assumes the absence of correlation between the initial conditions and α_i will be inconsistent, which also leads to overestimation of β_1 in equation (1) (Stewart, 2007).

We deal with the problem of endogenous initial conditions using the conditional maximum likelihood estimator suggested by Wooldridge (2005). We also employ an alternative specification of Wooldridge's estimator proposed by Rabe-Hesketh and Skrondal (2013) to address potential bias in the initial conditions due to the short panel length. We compare the results with those of Heckman's (1981) reduced-form approach as a sensitivity check. Heckman's estimator is introduced prior to the discussion of Wooldridge's estimator to facilitate the understanding of the empirical discussion.

3.1. *Heckman's Estimator*

Heckman (1981) specifies a linearized approximation to the reduced-form equation for the initial value of the latent variable. Specifically, the latent variable in the initial year y^*_{i1} can be written as follows:

$$y^*_{i1} = \pi_0 + Z'_{i1}\pi_1 + \theta_1\alpha_i + u_{i1}, \qquad i = 1, \ldots, N, \tag{3}$$

where Z_{i1} represents a vector of exogenous covariates including explanatory variables observed in the first wave (X_{i1}) and pre-sample characteristics that are deemed instruments (Akay, 2012; Arulampalam and Stewart, 2009; Pasaribu, 2016). The explanatory variables in vector X_{i1} include the same observed characteristics considered in the baseline regression, equation (1). The pre-sample characteristics, on the other hand, are considered a proxy for poverty and include the ability to afford bills, rent, and credit card payments and unemployment status over the past year, prior to the initial sample period.

The study assumes the composite error term, $v_{i1} = \theta_1\alpha_i + u_{i1}$, to be correlated with α_i but uncorrelated with u_{it} for $t \geq 2$.[11] The standard assumptions regarding u_{it} and α_i being normally distributed—the former with variance one and the latter with variance σ^2_α—are considered, as before. Given these normalizations, the model can be estimated with maximum likelihood techniques (Stewart, 2007).

Equations (1) and (3) together specify a complete model for $(y_1, \ldots, y_T)$. In this model, the contribution to the likelihood function for individual i is given by the following:[12]

$$L_i = \int \left\{ \Phi[(Z'_{i1}\pi_1 + \theta_1\alpha)(2y_{i1} - 1)] \prod_{t=2}^{T_i} \Phi[(\beta_1 y_{it-1} + X'_{it}\Omega_1 + \theta_t\alpha)(2y_{it} - 1)] \right\} g(\alpha)\mathrm{d}\alpha,$$

where $\theta_T = 1$ for identification (of σ^2_α), $g(\alpha)$ is the probability density function of the unobserved individual-specific effect, and Φ is the standard normal cumulative

[11]A test of $\theta = 0$ provides a test of exogeneity of the initial conditions in this model. The hypothesis of exogeneity of the initial condition ($\theta = 0$) is strongly rejected in Heckman's reduced-form model in equation (3). Rather, the estimate of θ is about one, as reported in Table 4.

[12]To simplify the notation, the intercepts β_0 and π_0 in equations (1) and (3) are not explicitly shown in the likelihood function.

distribution function. The covariates are considered in the same way as described above. Longitudinal averages of time-varying variables $\bar{X}_i$ (i.e. number of children, household size, health, and employment status) are also included in the regression analysis to allow for the correlation between observed characteristics and unobserved individual heterogeneity. For the sake of brevity, $\bar{X}_i$ is subsumed in X_{it}. As in common practice, the integral is evaluated using Gaussian–Hermite quadrature based on the assumption that α is normally distributed (Arulampalam and Stewart, 2009).

3.2. *Wooldridge's Estimator*

Wooldridge (2005) proposes proposes a conditional maximum likelihood estimator in which one does not need to find the density of $(y_{i1}, \ldots, y_{iT})$, given the exogenous variables. Specifically, the author specifies an approximation for the density of α_i conditional on the initial observation y_{i1} and either the set of explanatory variables $X_i = (X_{i2}, \ldots, X_{iT})$ or averages of the X variables over t as regressors in the model.

Wooldridge's estimator has practical advantages over Heckman's estimator in that the initial dependent variable does not need to be jointly modeled with the subsequent dependent variables and that estimation can be obtained using standard random-effects probit software. On the other hand, a recent study by Akay (2012) claims that the parameter estimates from Wooldridge's estimator could be biased in applications that rely on panel data containing a small number of periods. In response, Rabe-Hesketh and Skrondal (2013) suggest including initial-period explanatory variables in the auxiliary model (for the individual-specific effect) as additional regressors, besides the longitudinal averages and the lagged dependent variable.[13] Rabe-Hesketh and Skrondal (2013) also reveal that Wooldridge's original auxiliary model, in which the individual-specific effect is conditioned on the lagged dependent variable and explanatory variables at periods $t = 2, \ldots, T$, serves as a favorable outcome. Following their proposal, we exclude the initial-period characteristics from the covariates and from their longitudinal averages but include them only as additional regressors in our last specification, in equation (6).

We begin the analysis with Wooldridge's original model and assume the following auxiliary model:

$$\alpha_i = \varsigma_0 + \varsigma_1 y_{i1} + X_i' \varsigma_2 + a_i, \tag{4}$$

where $X_i' = (X_{i2}', \ldots, X_{iT}')$. The correlation between y_{i1} and α_i is handled by the use of equation (4), providing another unobservable individual-specific heterogeneity term a_i that is uncorrelated with the initial observation y_{i1}. Here and henceforth,

[13]Rabe-Hesketh and Skrondal (2013) indicate that the problem with the "overly constrained model" suggested by Akay (2012) is that the authors includes initial-period explanatory variables in the longitudinal averages. Since the conditional distribution of the unobserved effect depends more directly on the initial-period explanatory variables than on the explanatory variables in the other periods, the coefficients of the initial-period explanatory variables should not be constrained to equal the coefficients in the other periods.

a_i is assumed to be normally distributed with mean zero and variance σ_a^2, given the covariates in each specification.

Next, we employ a specification for the individual-specific effect following the Mundlak–Chamberlain approach described above:

$$\alpha_i = \varsigma_0 + \varsigma_1 y_{i1} + \bar{X}_i' \varsigma_2 + a_i, \tag{5}$$

where $\bar{X}_i = \frac{1}{T-1} \sum_{t=2}^{T} X_{it}$ includes time-varying explanatory variables that are correlated with the unobservable α_i.

In the last specification, we add the initial-period explanatory variables (X_{i1}) to the auxiliary model, as suggested by Rabe-Hesketh and Skrondal (2013). The new specification for the individual-specific effect α_i can be written as follows:

$$\alpha_i = \varsigma_0 + \varsigma_1 y_{i1} + \bar{X}_i' \varsigma_2 + X_{i1}' \varsigma_3 + a_i, \tag{6}$$

where X_{i1} is a vector of explanatory variables in the initial year and all the other variables are in equation (5).

The probability of benefit receipt is obtained by substituting each of these three auxiliary models into equation (2), separately. To illustrate, as for equation (5), the probability of benefit receipt becomes

$$\Pr(y_{it} = 1 | a_i, y_{i1}) = \Phi[\beta_0 + \beta_1 y_{it-1} + \varsigma_1 y_{i1} + \bar{X}_i' \varsigma_2 + X_{it}' \Omega + a_i], \qquad t = 2, \dots, T,$$

where the constant term ς_0 is subsumed into β_0. In this model, the contribution to the likelihood function for individual i is as follows:

$$L_i = \int \left\{ \prod_{t=2}^{T} \Phi[(\beta_0 + \beta_1 y_{it-1} + \varsigma_1 y_{i1} + \bar{X}_i' \varsigma_2 + X_{it}' \Omega + a)(2y_{it} - 1)] \right\} g(a) \mathrm{d}a,$$

where $g(a)$ is the normal probability density function of the new unobserved individual-specific effect a_i, specified in equation (5). The likelihood function is maximized by evaluating the integral over a, using Gaussian–Hermite quadrature, which is based on the assumption that a is normally distributed.

4. Results

4.1. *Main Results*

This section presents the estimation results from the specifications described in the previous section. Given the non-linearity of the models, the magnitudes of the coefficient estimates provide little information about the size of the effects of the observable characteristics and, hence, the degree of state dependence. The level of state dependence is assessed through the measurement of the average partial effect (APE) of benefit receipt. The next section elaborates on this issue.

Table 3 presents the estimation results of the dynamic random-effects probit model based on the Wooldridge estimator. These results are based on a balanced panel in which the individuals are observed across a 4-year period. The first

TABLE 3
DYNAMIC RANDOM-EFFECTS PROBIT MODEL: WOOLDRIDGE'S ESTIMATOR—BALANCED SAMPLE

	(1)	(2)	(3)	(4)
Benefit receipt at t−1	2.318***	1.371***	1.344***	1.317***
	(0.053)	(0.119)	(0.119)	(0.119)
Benefit receipt at t=1		1.751***	1.783***	1.840***
		(0.230)	(0.230)	(0.233)
Personal Characteristics				
Age	−0.031	−0.058*	−0.054	0.163
	(0.019)	(0.033)	(0.034)	(0.159)
Age squared	0.023	0.048	0.043	−0.159
	(0.022)	(0.038)	(0.039)	(0.172)
Female	−0.044	−0.084	−0.189	−0.212
	(0.205)	(0.322)	(0.330)	(0.412)
Years of schooling	−0.055***	−0.089***	−0.089***	−0.052
	(0.008)	(0.012)	(0.013)	(0.071)
Spouse's education	−0.048***	−0.072***	−0.071***	−0.066
	(0.007)	(0.013)	(0.013)	(0.056)
Number of children	0.061	0.306***	0.103	0.068
	(0.080)	(0.044)	(0.096)	(0.099)
Household size	0.060	−0.031	0.071	0.091
	(0.067)	(0.032)	(0.083)	(0.084)
Health restriction	0.096	0.227***	0.133	0.142*
	(0.069)	(0.067)	(0.086)	(0.086)
Non-employed	−0.132	0.052	−0.134	−0.112
	(0.099)	(0.083)	(0.122)	(0.123)
Time Averages				
Avg: number of children	0.151*		0.250**	0.260
	(0.085)		(0.106)	(0.159)
Avg: household size	−0.098		−0.126	−0.179
	(0.069)		(0.089)	(0.123)
Avg: health restriction	0.182*		0.244*	0.299*
	(0.094)		(0.140)	(0.177)
Avg: non-employed	0.217*		0.317*	0.490**
	(0.118)		(0.162)	(0.225)
First-Wave Characteristics				
Fst: age				−0.228
				(0.159)
Fst: age squared				0.221
				(0.177)
Fst: years of schooling				−0.037
				(0.071)
Fst: spouse's education				−0.006
				(0.057)
Fst: number of children				0.042
				(0.138)
Fst: household size				0.023
				(0.107)
Fst: health restriction				−0.041
				(0.118)
Fst: non-employed				−0.230
				(0.166)
Year Dummies				
2008	0.288***	0.420***	0.416***	0.363***
	(0.078)	(0.093)	(0.094)	(0.084)
2009	0.187**	0.422***	0.414***	0.327***
	(0.074)	(0.095)	(0.098)	(0.105)
2010	0.344***	0.490***	0.498***	0.500***
	(0.074)	(0.103)	(0.104)	(0.104)

TABLE 3 *Continued*

	(1)	(2)	(3)	(4)
2011	0.077	0.258**	0.253**	0.211**
	(0.078)	(0.107)	(0.109)	(0.087)
2012	−0.028	0.109	0.082	
	(0.078)	(0.104)	(0.109	
Constant	−0.599	−0.517	−0.644	−0.519
	(0.410)	(0.699)	(0.711)	(0.754)
Number of observations	10,239	10,239	10,239	10,156
Number of individuals	3,450	3,450	3,450	3,400
σ_α	0.001	1.010	1.037	1.059
	(30.009)	(0.108)	(0.109)	(0.110)
ρ	0.000	0.505	0.518	0.528
	(0.070)	(0.054)	(0.052)	(0.052)
Log likelihood	−2,135.274	−2,089.683	−2,083.688	−2,061.984
Predicted Probabilities				
Entry	0.038***	0.015***	0.014***	0.013***
	(0.005)	(0.003)	(0.003)	(0.003)
Persistence	0.708***	0.210***	0.195***	0.185***
	(0.025)	(0.053)	(0.051)	(0.050)
APE (%)	67.0	19.5	18.1	17.2

Notes: Estimation is based on the appended sample of two balanced panels of 2006–9 and 2009–12. Robust standard errors clustered at individual level are in parentheses.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

column provides estimates assuming that the initial conditions are exogenous and columns (2)–(4) display the results obtained from the specifications indicated in equations (4)–(6), respectively. The coefficient estimates of the lagged recipient status, namely, state dependence, lie in the narrow range between 1.37 and 1.32 and are all strongly statistically significant. This range is calculated according to the three specifications that allow for endogenous initial conditions. The magnitude of the coefficient estimate decreases as the longitudinal averages (of time-varying variables) and the initial-period explanatory variables are added to the regression.

On the other hand, failure to account for endogenous initial conditions doubles the coefficient estimate of the lagged dependent variable (first row of column (1) of Table 3). The reduction in the coefficient estimate after controlling for endogenous initial conditions coincides with an increase in the estimated standard deviation of the individual-specific effect (σ_α), which is reported toward the bottom of Table 3. The term σ_α is estimated at one, which translates into a cross-period correlation (ρ) in the composite error term of around 0.5. This implies that half of the variance in the composite error term comes from permanent individual unobserved heterogeneity. As presented in the second row of Table 3, the coefficient estimate of the control for the receipt status in the initial period ($t = 1$) is positive and statistically significant. This points out that individuals who received social assistance benefits in the initial period have a higher probability of receiving benefits in following periods. Taken together, our results support the evidence that the estimates based on the exogeneity assumption suffer from initial conditions bias, and that this bias has the potential to overestimate the degree of state dependence.

Table 4 shows the estimation results from Heckman's approach based on a balanced-panel sample. Each column of the table belongs to a separate specification using different subsets of instruments to estimate the equation for the initial

TABLE 4

Dynamic Random-Effects Probit Model: Heckman's Estimator—Balanced Sample

	(1)	(2)	(3)	(4)
Benefit receipt at $t-1$	1.543***	1.543***	1.573***	1.504***
	(0.097)	(0.096)	(0.099)	(0.093)
Personal Characteristics				
Age	−0.032	−0.031	−0.031	−0.032
	(0.032)	(0.033)	(0.032)	(0.033)
Age squared	0.016	0.014	0.015	0.015
	(0.037)	(0.037)	(0.036)	(0.038)
Female	−0.040	−0.055	−0.042	−0.115
	(0.471)	(0.481)	(0.468)	(0.455)
Years of schooling	−0.109***	−0.109***	−0.107***	−0.112***
	(0.014)	(0.014)	(0.014)	(0.014)
Spouse's education	−0.085***	−0.084***	−0.083***	−0.087***
	(0.013)	(0.013)	(0.013)	(0.013)
Number of children	0.112	0.111	0.111	0.112
	(0.093)	(0.093)	(0.092)	(0.093)
Household size	0.060	0.060	0.058	0.063
	(0.077)	(0.077)	(0.076)	(0.078)
Health restriction	0.119	0.121	0.119	0.121
	(0.081)	(0.081)	(0.081)	(0.082)
Non-employed	−0.091	−0.089	−0.090	−0.090
	(0.119)	(0.119)	(0.118)	(0.120)
Time Averages				
Avg: number of children	0.327***	0.327***	0.315***	0.342***
	(0.106)	(0.106)	(0.105)	(0.107)
Avg: household size	−0.145*	−0.144*	−0.140*	−0.150*
	(0.084)	(0.084)	(0.083)	(0.084)
Avg: poor health	0.383***	0.385***	0.375***	0.395***
	(0.135)	(0.135)	(0.133)	(0.137)
Avg: non-employed	0.295*	0.309*	0.290*	0.321*
	(0.162)	(0.162)	(0.160)	(0.164)
Constant	−0.300	−0.332	−0.328	−0.300
	(0.691)	(0.692)	(0.677)	(0.710)
Number of observations	15,352	15,352	15,352	15,352
ρ	0.505	0.504	0.487	0.525
	(0.059)	(0.059)	(0.063)	(0.054)
θ	1.131	1.146	1.161	1.117
	(0.159)	(0.163)	(0.172)	(0.149)
Log likelihood	−3,201.488	−3,194.233	−3,190.143	−3,207.502

Notes: Estimation is based on the appended sample of two balanced panels of 2006–9 and 2009–12. All specifications also include year dummies. Columns (1)–(4) differ according to the instruments used to estimate the initial condition regression presented in Table 5. Robust standard errors clustered at the individual level are in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

conditions. The estimates of the initial conditions regression, indicated in equation (3), are reported in Table 5. Columns (1)–(3) use various pre-sample characteristics, separately or together, as instruments, while column (4) only includes first-wave characteristics in the estimation of the initial conditions equation. The pre-sample characteristics involve information about past unemployment status (1 year prior to the first wave) and past ability to afford bills, rent, and credit card payments. The coefficient estimate on the lagged dependent variable, fluctuating around 1.5, is slightly higher than the results obtained from the Wooldridge estimator. The magnitude of the coefficient estimate is not sensitive to the choice of instrument,

TABLE 5
HECKMAN'S INITIAL CONDITION EQUATION ESTIMATION

	(1)	(2)	(3)	(4)
Personal Characteristics				
Age	0.008	0.013	0.004	
	(0.044)	(0.045)	(0.044)	
Age squared	−0.000	−0.000	−0.000	
	(0.001)	(0.001)	(0.001)	
Female	0.395	0.156	0.293	
	(0.609)	(0.614)	(0.609)	
Years of schooling	−0.127***	−0.123***	−0.123***	
	(0.016)	(0.016)	(0.016)	
Spouse's education	−0.083***	−0.088***	−0.084***	
	(0.016)	(0.016)	(0.016)	
Number of children	0.442***	0.444***	0.437***	
	(0.062)	(0.062)	(0.062)	
Household size	−0.109**	−0.108**	−0.109**	
	(0.045)	(0.046)	(0.046)	
Health restriction	0.500***	0.446***	0.463***	
	(0.099)	(0.100)	(0.100)	
Non-employed	−0.108	0.187	−0.070	
	(0.150)	(0.119)	(0.150)	
Pre-sample Characteristics				
Pre: unemployed	0.738***		0.619***	
	(0.215)		(0.217)	
Pre: poverty1		0.016	0.010	
		(0.127)	(0.126)	
Pre: poverty2		0.209**	0.190**	
		(0.092)	(0.092)	
Pre: poverty3		0.325***	0.307***	
		(0.096)	(0.096)	
First-Wave Characteristics				
Fst: age				0.020
				(0.046)
Fst: age squared				−0.050
				(0.054)
Fst: years of schooling				−0.128***
				(0.016)
Fst: spouse's education				−0.086***
				(0.016)
Fst: number of children				0.451***
				(0.062)
Fst: household size				−0.109**
				(0.046)
Fst: poor health				0.485***
				(0.100)
Fst: non-employed				0.201*
				(0.119)
Constant	−0.682	−0.942	−0.798	−0.861
	(0.889)	(0.902)	(0.891)	(0.948)
Number of observations	15,352	15,352	15,352	15,352

Notes: The initial condition equation estimation corresponds to the main regression results presented in Table 4. The estimation is based on the appended sample of two balanced panels of 2006–9 and 2009–12. All specifications also include year dummies. Robust standard errors clustered at the individual level are in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

changing the coefficients only in small margins (first row of Table 4). The lower coefficient estimates (and hence the APE) of the lagged dependent variable from Wooldridge's estimator relative to Heckmans' implies that the Wooldridge estimates are unlikely to suffer from upward bias due to use of a short panel. This evidence suggests the robustness of the Wooldridge estimator even in the use of a short panel and is in line with the findings of Rabe-Hesketh and Skrondal (2013).

The models presented in Tables 3 and 4 consist of covariates including the reference person's characteristics (i.e. sex, age, age squared, marital status, own and spouse's education, health restrictions, and employment status), household characteristics (i.e. number of children and household size), and year dummies. The relations between the personal characteristics and the likelihood of being a benefit recipient are generally in the expected direction. The signs of the estimates of the explanatory variables derived from the Wooldridge estimator do not differ from the Heckman estimator. The probability of receiving social assistance benefits decreases with an increase in age, though the estimate is either of borderline significance or statistically insignificant. As expected, both the respondent's and the spouse's educational attainment are negatively and strongly associated with benefit receipt. On the other hand, having a restrictive health condition makes people more likely to receive benefits. Surprisingly, gender and employment status do not seem to be related with benefit receipt. This finding is, however, consistent with the findings of Königs (2014). As stated by the author, women and men who live in the same household are treated equally as recipients, since the beneficiary unit is defined at the household level. Similarly, the null impact of employment status could be linked to the fact that the regression analysis conditions on the personal characteristics of the household heads (reference persons) who are more likely to be employed (as seen in Table 2) and possibly ineligible to become recipients, whereas the beneficiary unit is the household, such that any (other) member of the household could be an eligible recipient.

Household characteristics, such as the number of dependent children and household size, are not strongly associated with benefit receipt, which could be related to the insufficient time variation in these variables over the sample period. The time averages of these variables, particularly the coefficient estimate of the number of children, are rather statistically significant (see Tables 3 and 4). As illustrated in Figure 1, child allowances account for a considerable share among social assistance schemes and, in relation to this, a household with dependent children increases its likelihood of receiving benefits. Overall, the time averages play an important role in the models. In particular, they help to control for potential correlation between unobserved individual heterogeneity and observed characteristics. The majority of the coefficients of the time-averaged variables are statistically significant and have the same sign as the corresponding variables. The model also captures year-specific effects in benefit receipt with the help of year dummy covariates (see Table 3). We find positive and statistically significant coefficient estimates, except in 2012. This result is consistent with the increasing rate of benefit receipt over most of the sample period, shown in Figure 1.

Lastly, we investigate the variation in benefit dependency with an increase in the amount of benefits received. It is reasonable to expect a positive association between the amount of benefits and the persistence rate, especially within the context of Turkey, which lacks a transparent benefit allocation system and an efficient monitoring mechanism enforcing people to exit the system if their income surpasses a certain level. To investigate heterogeneity in the amount of benefits, we introduce four dummy variables for each quartile of the amount of benefits and interact these dummies with the lagged variable for benefit recipient. The interaction terms thus refer to binary indicators taking the value of one if the respondent is a benefit recipient at $t-1$ as well as the amount of benefits being within the range of the corresponding quartile, and zero if the respondent is either a non-recipient or the amount of benefits falls outside the quartile in question. The interaction terms are additionally included in the regressions, besides the individual- and household-level controls. The estimation results are shown in Table 6 for the Wooldridge estimator and the corresponding results from the Heckman estimator are displayed in Table A.1, in the Appendix. In line with expectations, the increase in benefit amounts is accompanied by an increase in the persistence rate and hence greater state dependence.

4.2. *Degree of State Dependence*

The estimation results from the dynamic random-effects probit model presented in Tables 3 and 4 suggest considerable state dependence in social assistance benefit receipt in Turkey. The coefficient estimates of lagged benefit receipt is always positive and statistically significant, regardless of the specifications. Next, we discuss the average partial effect (APE) of benefit receipt to assess the level of state dependence. The APE simply equals the difference in average predicted probabilities of social assistance receipt across individuals over time conditional on benefit receipt and non-receipt in the previous period, that is, the difference between predicted persistence and entry probabilities (Stewart, 2007). The predicted probabilities are calculated by evaluating the covariates at their mean values.

The bottom panel of Table 3 displays the estimated transition rates (of entry and exit) and APEs calculated based on the estimates in the table. In the case of Wooldridge's original specification, equation (4), presented in column (2), the average probability of benefit receipt at t conditional on receipt at $t-1$ is predicted to be 21 percent (the *persistence rate*) and the average probability of benefit receipt at t conditional on non-receipt at $t-1$ is predicted to be 1.5 percent (the *entry rate*). The APE is thus calculated to be 19.5 percentage points, which decreases to 18.1 percentage points for the model specified in equation (5) (column (3)). This model facilitates the addition of longitudinal averages of time-varying explanatory variables to the regression. The inclusion of additional control variables of first-wave characteristics, as in the case of equation (6), lowers the APE to 17.2 percentage points (column (4)). In line with the higher coefficient estimates from Heckman's approach, we find a higher APE, ranging from 20 to 25 percentage points, depending on the subset of instruments used to estimate the equation for the initial conditions.[14] Furthermore, we examine heterogeneity in state dependence across subgroups of the population by breaking down the results (presented in Table 3) by educational attainment, number of children, and employment status. An inspection of the Table A.2, in the Appendix, makes it clear that the degree of state dependence gets higher for vulnerable groups. For a household with three children

[14]For the sake of brevity, these results are not tabulated here but are available upon request.

TABLE 6
Heterogeneity Across the Amount of Benefits: Wooldridge's Estimator—Balanced Sample

	(1)	(2)	(3)	(4)
Benefit receipt at t−1 × Quartile1	2.007***	1.222***	1.195***	1.169***
	(0.073)	(0.132)	(0.131)	(0.131)
Benefit receipt at t−1 × Quartile2	2.265***	1.420***	1.397***	1.368***
	(0.082)	(0.145)	(0.147)	(0.147)
Benefit receipt at t−1 × Quartile3	2.672***	1.881***	1.852***	1.815***
	(0.108)	(0.180)	(0.181)	(0.184)
Benefit receipt at t−1 × Quartile4	2.718***	1.931***	1.895***	1.868***
	(0.111)	(0.194)	(0.195)	(0.197)
Covariates				
Benefit receipt at t=1	No	Yes	Yes	Yes
Time averages	Yes	No	Yes	Yes
First-wave characteristics	No	No	No	Yes
Number of observations	10,239	10,239	10,239	10,156
Number of individuals	3,450	3,450	3,450	3,400
σ_α	0.002	0.909	0.936	0.958
	(17.031)	(0.110)	(0.110)	(0.111)
ρ	0.000	0.452	0.467	0.478
	(0.058)	(0.060)	(0.059)	(0.057)
Log likelihood	−2,106.434	−2,073.846	−2,068.569	−2,047.428

Notes: Estimation is based on the appended sample of two balanced panels of 2006–9 and 2009–12. The interaction terms refer to binary indicators taking the value of one if the respondent is in benefit receipt at t−1 as well as the amount of benefits being within the range of the corresponding quartile, and zero if the respondent is either a non-recipient or the amount of benefits falls outside the concerned quartile. All specifications also include personal characteristics and year dummies considered in Table 3. Robust standard errors clustered at the individual level are in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

and an unemployed and low-educated head, past receipt is associated with a probability of receipt in the current period that is about 38 percentage points higher compared to the case of no receipt during the last period. On the other hand, the APE is estimated to be only 5.7 percentage points among university graduates, with 16 years of schooling.

In section 4.1, we show that state dependence is positively associated with the amount of benefits. To evaluate the magnitude of this association, we have predicted the transition probabilities for each quartile of benefit amount based on the estimates presented in Table 6. In particular, the predicted persistence (entry) probability is calculated by conditioning on receiving an amount of benefits within the range of the quartile in question, besides the condition of being a recipient (non-recipient) in the previous year. Table 7 shows the estimated transition rates of entry and exit for each quartile. Given a constant entry rate across quartiles, we find a steady increase in the persistence rate and, thus, greater dependence on the amount of benefits. For instance, considering Wooldridge's original estimator, state dependence ranges from 17 percent among first-quartile benefit recipients to

TABLE 7
HETEROGENEITY ACROSS THE AMOUNT OF BENEFITS: PREDICTED PROBABILITIES FROM THE WOOLDRIDGE ESTIMATOR—BALANCED SAMPLE

	(1) First Quartile	(2) Second Quartile	(3) Third Quartile	(4) Fourth Quartile
Exogenous Initial Conditions				
Entry	0.038***	0.038***	0.038***	0.038***
	(0.004)	(0.004)	(0.004)	(0.004)
Persistence	0.594***	0.690***	0.817***	0.828***
	(0.036)	(0.032)	(0.028)	(0.026)
Number of observations	10,239	10,239	10,239	10,239
Wooldridge's Original Estimator				
Entry	0.017***	0.017***	0.017***	0.017***
	(0.004)	(0.004)	(0.004)	(0.004)
Persistence	0.187***	0.245***	0.409***	0.429***
	(0.051)	(0.062)	(0.088)	(0.094)
Number of observations	10,239	10,239	10,239	10,239
Mundlak–Chamberlain Approach				
Entry	0.017***	0.017***	0.017***	0.017***
	(0.004)	(0.004)	(0.004)	(0.004)
Persistence	0.175***	0.232***	0.391***	0.407***
	(0.049)	(0.061)	(0.088)	(0.094)
Number of observations	10,239	10,239	10,239	10,239
Rabe & Skrondal's Extension				
Entry	0.016***	0.016***	0.016***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)
Persistence	0.164***	0.218***	0.370***	0.390***
	(0.048)	(0.060)	(0.088)	(0.094)
Number of observations	10,156	10,156	10,156	10,156

Notes: Prediction is based on the estimates presented in Table 6, using the appended sample of two balanced panels of 2006–9 and 2009–12. Covariates are evaluated at the mean in calculating the marginal effects. Robust standard errors clustered at the individual level are in parentheses.

***$p < 0.01$, *$p < 0.05$, *$p < 0.1$.

41 percent among beneficiaries in the top quartile. These results are in line with the findings of Anderson and Meyer (1997), which suggest that a 10 percent increase in the weekly benefit amount of unemployment insurance would increase the take-up rate by 2.0–2.5 percentage points.

While the structural state dependence of around 17–20 percentage points is substantial, the value is considerably lower than the difference between the observed persistence and entry rates of about 75 percent, illustrated in Figure 2. This implies that most of the observed state dependence is due to observed and unobserved heterogeneity across individuals. The APEs estimated for Turkey are at least 3 percentage points higher than those reported by Cappellari and Jenkins (2008) for the U.K. (14.4 percentage points) and by Königs (2014) for Germany (14.1 percentage points). While Turkey's estimated persistence rate is comparable with those of these countries, its entry rate is around 4 percentage points lower. The divergence in the degree of state dependence in benefit receipt could be related to the country context, such as distinctive institutional structuring and/or methodological issues including differences in the types of benefits adopted by studies.

4.3. *Sensitivity Checks*

So far, we have discussed the estimation results based on a balanced-panel sample in which only individuals tracked over the entire panel period are kept in the operational sample. The reason behind the choice of a balanced panel is related to the concern that sample dropout is not random. Consequently, the unobservable determinants of non-response or panel attrition could be correlated with unobservables determining benefit receipt. Many previous studies use a balanced panel to avoid potential attrition bias. Only a few studies rely on an unbalanced panel or a weakly balanced sample, mainly due to a huge drop in the number of observations in a balanced panel.[15] However, this is not an issue for our analysis, because a shorter panel is employed for the study. Hence, the sample size remains sufficiently large in our balanced-panel sample.

Nevertheless, we are aware of a potential sample selection problem in balanced-panel data. To illustrate, those recipients who expend the least amount of effort in job search (with a higher tendency to rely on social assistance) could be more likely to drop out from the sample. Therefore, these recipients would not be represented in the balanced-panel sample, which would cause the underestimation of state dependence. To address the issue, we estimate the state dependence based on an unbalanced-panel sample.[16] The results from Wooldridge's estimator and the corresponding predicted probabilities are presented in the Appendix, in Table A.3. The unbalanced-panel sample results in larger coefficient estimates and hence higher APEs, compared to the balanced-panel results presented in Table 3. In other words, omitting the attriters, who are likely to be more dependent on social assistance, from the analysis sample yields a lower level of state dependence. This result implies that the substantial state dependence that we document based on the balanced sample could be even larger when considering the population sample.

One might also be concerned about supply-side–driven factors such as skill requirement changes over time due to a technology shock, which could reduce the job-finding rate, given the same set of skills, and hence increase the persistence rate in benefit receipt. To address this concern, we introduce to our model occupation dummies interacted with a linear time trend. The trend variable runs over a 4-year period. The occupation information in the SILC is structured according to the International Standard Classification of Occupations (ISCO-88) and, using this information, we construct nine occupational dummies. Since the focus of ISCO-88 is on skill levels and the skill specialization required to carry out the tasks and duties of an occupation, our occupation dummies would serve as a proper control

[15]Leading studies using balanced-panel data are those of Andren and Andren (2013), Biewen (2009), Hansen *et al*. (2014), and Stewart (2007). On the other hand, Königs (2014) handles the attrition bias problem by constructing a weakly balanced panel, while Cappellari and Jenkins (2014) rely on the finding from their earlier study that the impact of attrition is small in their sample (Cappellari and Jenkins, 2008).

[16]The implementation of Heckman's estimator is achieved through the Stata program *redprob*, written by Stewart (2006), which is applicable only to balanced panels. Therefore, for this sensitivity check, the unbalanced-panel results are restricted to the Wooldridge's estimator.

to capture skill requirements.[17] The interaction of occupation dummies with the time trend thus captures changes over time in the skill requirement. The estimation results based on the Wooldridge estimator are presented in Table A.4, in the Appendix. The coefficient estimates are overall very similar to our original results, presented in Table 3. The coefficients of lagged benefit receipt are slightly higher than the original ones. As a natural consequence, the predicted probabilities remain very similar: the APE ranges from 21 to 24 percent after controlling for endogenous initial conditions. The similarities in results verify that skill changes over the observation period are not a major issue in our sample data. Considering the short length of the panel (of 4 years), such evidence on the lack of change in skill requirements is reassuring.

5. Discussion and Conclusions

The empirical evidence on the evaluation of the dynamics of social assistance benefits has thus far been limited to developed economies, despite the existence of social transfers in many developing countries. The current study has examined this issue in Turkey over the past decade, within a state-dependence framework. This is the first empirical study to explore state dependence in social assistance benefit receipt in the context of a developing country.

Based on annual panel data from 2006 to 2012, a dynamic random-effects probit model was employed to control for unobserved heterogeneity and initial conditions. To model initial conditions and check for sensitivity, the results from Heckman's two-step estimator were compared with the results from Wooldridge's estimator. We also implemented an alternative specification of Wooldridge's estimator, suggested by Rabe-Hesketh and Skrondal (2013), to test whether the results are biased due to the usage of a short panel. We find that social assistance benefit receipt in the previous year increases the probability of being in receipt in the current year by 17.0–19.5 percentage points, after controlling for observed and unobserved characteristics and endogenous initial conditions. Moreover, the persistence in benefit receipt rises as the amount of benefits increases.

Following the welfare literature, we link the relatively large size of state dependence in Turkey to labor-related mechanisms such as human-capital depreciation due to a long period of inactivity or signaling to employers about the productivity of the worker in benefit receipt. Arguably, an alternative mechanism to explain the source of benefit dependence in Turkey might be specific to the structure of its welfare system. In particular, the dysfunction in the system is likely to have a genuine behavioral effect on the individual's participation behavior. In this respect, more transparent and clear eligibility criteria along with better enforcement and monitoring mechanisms might promote the exit from beneficiary status and thus reduce the persistence rate, while allowing for new entries into the system. The latter is at least as important as reducing the persistence rate in developing countries that suffer from high poverty levels, given the key role of social assistance in poverty. This

[17]For reference, see http://www.ilo.org/public/english/bureau/stat/isco/isco88/index.htm, retrieved on May 17, 2018.

study could instigate research in the dynamics of social assistance benefits in the context of developing countries. The use of data from other developing countries would assure the external validity of our results.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's web site:

Appendix: Tables

Table A.1: Heterogeneity Across the Amount of Benefits: Heckman's Estimator

Table A.2: Heterogeneity Across Subgroups: Predicted Probabilities from the Wooldridge Estimator

Table A.3: Dynamic Random Effects Probit Model: Wooldridge's Estimator

Table A.4: Dynamic Random Effects Probit Model: Wooldridge's Estimator (Including Time Trend Interacted with Occupation Categories)