State dependence in youth labor market experiences and the evaluation of policy interventions

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Abstract: We investigate the extent and type of state dependence in labor market outcomes for young low-skilled Australians. Our model allows for three labor force states, employment, unemployment and out of the labor force, and for observed and unobserved heterogeneity. We find evidence of occurrence dependence but no lagged duration dependence. A past employment spell increases the probability of employment in the future, but the length of the spell does not matter. A past spell of unemployment undoes the positive benefits from a spell in employment. Interpretations of these effects and implications for labor market policies are discussed.

Keywords: Transition data, event history analysis, state dependence, unobserved heterogeneity, policy evaluation.

J.E.L. Classification Numbers: C33, C41, J64, J68.

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Executive summary

This paper analyses the magnitude and form of state dependence in labour force outcomes for young low-skilled Australians. The presence of causal relationships between past labour market experiences and future outcomes has serious implications for the design of policies aimed at reducing youth unemployment. Existing program evaluations have concentrated on before-and-after comparisons and longer term effects have been mostly ignored.

There are still very few studies that characterize the form of state dependence across labor market states and these mostly deal with the "scarring" effects of unemployment. We look at general forms of state dependence across three labour force states: employment, unemployment and out of the labor force. Also contrary to most papers in the area, this study uses an event history framework. The resulting model is more general in the type of state dependence allowed and more precise in the measurement of the timing and duration of labour force spells.

Results suggest that the form of state dependence is complex. For youth without post-secondary education, estimates suggest significant occurrence dependence but no lagged duration dependence is found. Having experienced an employment spell in the past increases the probability of future employment and similarly, unemployment spells raise the probability of future unemployment; however the duration of past spells does not matter. For example, simulations suggest that one additional spell of unemployment raises the future unemployment rate for this group by 13 to 16 percentage points. Also, the magnitude of the effects of past employment and unemployment spells is similar which means that the beneficial effects of an employment spell can be easily undone if the spell is followed by unemployment.

The presence of occurrence dependence suggests that keeping an attachment to the labor market is valuable in terms of future outcomes; young people who hold jobs can more easily find jobs in the future. This could be interpreted as benefits accruing from networking or other skills in finding employment. Alternatively, employers could be more willing to hire people who have held previous jobs regardless of the length of the em-

ployment spells. The lack of lagged duration dependence suggests that on-the-job human capital acquisition is limited or that it is not transferable across employment experiences.

Although men and women experience significant differences in some aspects of their labour market histories, the form and magnitude of the state dependence are similar and overall results apply to both groups. Preliminary results for youths with post-secondary education suggest different forms of state dependence for this group. There is evidence of lagged duration dependence; that is, the length of employment and unemployment spells matters in determining future outcomes.

Overall, our results suggest that employment spells, even short ones are beneficial for future employment probabilities and ignoring these effects can lead to an underestimation of labour market policies. Unfortunately, previous spells of unemployment are also important and can easily undo the beneficial effects of job spells. Finally, the effects are complex and flexible modeling of state dependence is needed to isolate and measure the causal relationships between past and future labour market experiences.

1 Introduction

Joblessness occurs more frequently among young people than in the rest of the labor force and youth unemployment has received much attention among researchers as well as in public debate. The higher unemployment rates are partly explained by the learning processes faced by both sides of the labor market. New entrants to the labor market need to discover their own skills and preferences as well as the opportunities available to them, and employers need to assess the potential productivity of new entrants. This results in "job-shopping" (e.g. Topel and Ward, 1992) and higher mobility among young people. Moreover, temporary jobs and spells of unemployment often accompany investments in education or training (e.g. Wolpin, 1987).

Recently trends in youth unemployment have been the cause of renewed concern. Youth employment is believed to be particularly sensitive to the state of the economy; yet, youth unemployment has remained at twice or more the adult rate of unemployment in most OECD countries despite the continued period of expansion (e.g. OECD, 2002). Increased levels of education and the aging of the workforce have also failed to solve the problem.

To combat youth unemployment, many countries have implemented a wide range of policies targeted at unemployed and disadvantaged youth. There is now a large and growing literature estimating the effects of employment policies and training programs on the subsequent employment experience of the participants. For recent surveys on the evaluation of active labor market policies (ALMP) in general see Heckman, Lalonde, and Smith (1999), Kluve and Schmidt (2002) and Kluve (2006), and for policies targeted at the youth labor market specifically see Blanchflower and Freeman (2000). The findings for ALMP aimed at young people are not encouraging; in many if not most cases, these programs lead to a reduction in employment probabilities.[1] Kluve (2006, page 28) concludes: "It might also be the case that active labor market policies are not at all the appropriate policy for this group, and public policy should therefore focus on measures that prevent

[1]There is however substantial heterogeneity in the impacts of policies; and recent findings concerning the New Deal for Young People suggest that the multistage approach adopted in the UK has generated positive employment effects (e.g. Blundell, Meghir, and Van Reenen, 2004; De Giorgi, 2005).

the very young from becoming disadvantaged in the labor market in the first place."

With few exceptions, the existing literature on evaluations of ALMP has two major shortcomings: the equilibria are partial and only short-term effects are considered (Kluve, 2006). A few recent studies of social and labor market policies consider general equilibrium effects such as spillovers, crowding-out and responses in prices (Blundell, Costa Dias, and Meghir, 2003; Lise, Seitz, and Smith, 2005; Angelucci and De Giorgi, 2007); an earlier example is Davidson and Woodbury (1993). Although tentative, the results suggest that general equilibrium effects could be considerable (e.g. Lise, Seitz, and Smith, 2005). Also, see Bloom, Orr, Bell, Cave, Doolittle, Lin, and Bos (1997) for a more extensive welfare analysis of the costs and benefits of a job training program.

Researchers recognize that the short-term horizons used in most of the policy evaluations can lead to an underestimation of the policy impacts and to misleading conclusions about the relative effectiveness of programs (e.g. Hotz, Imbens, and Klerman, 2006). However, data limitations have generally led to comparisons of outcomes before and after intervention with horizons of less than a year for the post-intervention outcomes. Exceptions to this are few (e.g. Card and Sullivan, 1988; Bloom, Orr, Bell, Cave, Doolittle, Lin, and Bos, 1997; Lechner and Wunsch, 2005; Hotz, Imbens, and Klerman, 2006). One finding to emerge from these studies is that extended training programs can have substantially larger effects on employment than policies aimed at placing people in jobs in a short period of time.

It is surprising that the employment spells experienced as part of ALMP do not have stronger impacts on future employment probabilities, even in the short time horizons considered. This suggests that state dependence from past employment spells is weak, at least in the case of subsidized jobs. To date the policy impacts have either been measured at a specific point in time in the future or cumulated over time, with limited attempts to separate direct effects of policies (finding people a job placement) from the effects of employment on future labor force transitions (state dependence).[2] In addition,

[2]Histories of labor market experiences have been used to construct better counterfactuals in policy evaluations following Card and Sullivan (1988) and Heckman and Smith (1999), who argue that labor force dynamics play a central role in the selection process into ALMP. For recent examples see Heckman and Smith (2004) and Kluve, Lehmann, and Schmidt (2005).

existing results on longer term policy effects are based on samples spanning all age groups. Stronger effects from state dependence are expected for young people who can benefit more from job shopping and on-the-job human capital acquisition; hence they should be treated separately in the analysis.

In this paper the presence and form of state dependence are estimated in a general and flexible model of labor force transitions for young people. The estimates are based on observed histories of up to six years after the person leaves secondary school. Various employment and unemployment interventions are simulated based on the parameter estimates and the immediate or direct impact of the shock is distinguished from future state dependence effects. We do not analyze a specific policy and our simulation results can be interpreted as best case scenarios. For example, a finding of limited state dependence in the case of a "real" job suggests that such effects are unlikely to be important in the case of subsidized employment (e.g. Gerfin, Lechner, and Steiger, 2004) and lends validity to existing before-and-after comparisons of policy effects.

Early papers on the estimation of state dependence include for example Heckman and Borjas (1980) and Ellwood (1982). Recent contributions focus on the scarring effects of unemployment and the implications for policy interventions (e.g. Arulampalam, Booth, and Taylor, 2000; Gregg, 2001; Mroz and Savage, 2006).[3] Most studies, with Heckman and Borjas (1980) as a notable exception, have approached the problem using autoregressive models for panel data. That is, they have compared either labor force status at distinct points in time or the proportion of time within given periods spent in each state. State dependence is represented by lagged dependent variables in this framework.

In contrast, this paper applies event history methods (e.g. Heckman, Lalonde, and Smith, 1999). The event history approach emphasizes the timing of transitions between distinct states such as employment and unemployment and is particularly well suited for this kind of research, given the availability of high-quality longitudinal data and the delicate econometric issues involved such as missing data (left- and right-censoring), and the influence of unobserved heterogeneity. Recent advances in event history methods have

[3]Like the present paper, these studies do not evaluate particular programs but estimate state dependence relative to the policy environment in place during the analysis period.

allowed the modelling of current duration dependence along with endogenous program participation (e.g. Abbring and van den Berg, 2003; Abbring, van den Berg, and van Ours, 2005), but to date, state dependence across spells has yet to be incorporated when evaluating policy interventions.

We estimate the extent of state dependence in transitions across three labor force states, namely employment, unemployment and out of the labor force. Using three states complicates the analysis considerably but the resulting model takes into account movements in participation and allows for differential effects of spells with and without search on future employment probabilities. In his review of the literature on school-to-work transitions, Ryan (2001) points out the lack of evidence on the structure of dependence across labor force states other than the current duration dependence in unemployment. A few recent studies have incorporated multiple labor force states in duration analysis (e.g. D'Addio and Rosholm, 2002a,b; Addison and Portugal, 2003; Frijters and van der Klaaw, 2006). This study also contributes to the existing literature on labor market transitions in estimating these cross-state effects.

The main data source is the Australian Youth Survey 1989–1994, and the model analyzes respondents' weekly history of labor force spells after they have left secondary school. In addition to the personal past history of transitions, we consider explanatory variables representing personal demographic characteristics as well as external environmental factors such as the business cycle. Also the model includes random effects to capture unobserved heterogeneity. People are analyzed separately depending on their post-secondary education levels. Most of the results presented below concern people with no education following secondary school over the duration of the survey. The resulting sample is fairly homogeneous and this group is of most interest for policy interventions. Results are presented for the group with post-secondary education for comparison. Also, in order to account for the importance of fertility decisions among women, separate estimations are performed for men and women and the results are compared to those involving the whole sample.

Parameterizing state dependence is a challenge in these models, because of the need to summarize the past history and the limited guidance available in the literature on how to achieve this. The parameterization used in this paper is inspired by Heckman and Borjas' (1980) distinction between duration dependence, occurrence dependence and lagged duration dependence. Specifically, we include variables representing both the number of transitions and the time spent in each state prior to the start of the current spell, in addition to elapsed time in the current spell. We experiment with alternative specifications of state dependence, including for example models in which past experience is discounted. The estimation results suggest that occurrence and lagged duration dependence have different impacts and should be modelled separately. No evidence of discounting was found; longer histories may be needed for such effects to emerge.

The most striking result is that of no lagged duration dependence in any of the three states. Jointly and individually, the coefficients on these variables are insignificant and the results of simulated interventions confirm this finding. Either on-the-job human capital acquisition is limited or it is not transferable across employment experiences. The more positive aspect of this finding is that the duration of past unemployment spells does not matter for future unemployment probabilities. Thus the problem of people having recurring and increasingly entrenched unemployment experiences is not yet apparent in this age group.

There is evidence of occurrence dependence. An additional spell of employment (unemployment) increases the probability of being employed (unemployed) in the future. Furthermore, the size of the effects of past employment and unemployment spells are similar. Simulated interventions in which people are placed in employment spells of fixed duration followed by spells of unemployment do not have lasting effects because of the counteracting impacts of the employment and unemployment spells. When we isolate the effect of one additional spell of unemployment (without a corresponding employment spell) we find a medium-term impact of 4 additional days per month spent in unemployment or an increase of 13 to 16 percentage points in the unemployment rate for the group who experienced the spell.

The employment spells are fairly short for the low-skill group. Interventions where unemployed persons are placed in "real" jobs (i.e. jobs with the same transition intensities as observed employment spells) do not produce lasting effects since so few remain in employment. Men and women have different durations of employment spells due to different baseline transition intensities. Controlling for previous experiences, women tend to have longer employment spells than men. This is consistent with findings for French youths (e.g. D'Addio and Rosholm, 2002a). However, the findings on state dependence are very similar for men and women and separate estimations yield virtually identical results to a model that allows only for limited differences between men and women; specifically, dummy variables for females interacted with the presence of children and partners capture most of the variation by sex.

Findings from the literature on scarring effects of unemployment suggest some limited effects of unemployment spells on future probabilities of being unemployed (e.g. Gregg, 2001, and other papers in the same issue). Our results are consistent with these findings but also suggest that the impacts come from the presence and number of previous spells rather than the duration of time spent in the states. There is value in keeping an attachment in the labor market; young people who hold jobs can more easily find jobs in the future. This could be interpreted as benefits accruing from networking or other skills in finding employment rather than the human capital acquired on the job. Alternatively, employers could be more willing to hire people who have held previous jobs regardless of the length of the employment spells.[4]

The paper is organized as follows. The next section includes a summary of policy implications from this research. Section 3 discusses the data as well as sample selection and censoring issues. This is followed by a presentation of the econometric model in Section 4. Section 5 proceeds to discuss the estimation results and concludes by summarizing the fit of the model. Section 6 presents simulation results on the effects of various stylized policy interventions or shocks to labor force spells. Section 7 offers concluding comments.

[4] We have controlled for the presence of permanent and unobserved individual-specific effects; however, we cannot fully rule out the possibility that impermanent, individual-specific and unobserved characteristics are responsible for the state dependence. See for example Mroz and Savage (2006) for a more extensive modelling of unobserved effects.

2 Policy Discussion

This paper addresses an important issue that has not received much attention in the literature: the role of state dependence in youth labor market outcomes and the implications for public policy. State dependence refers to a causal link between previous outcomes and current and future outcomes. For example, past periods of unemployment may affect the probability of experiencing unemployment in the future through "scarring", irrespective of personal characteristics such as education and motivation and of external environmental factors such as the business cycle.

The extent of state dependence has serious implications for the effectiveness of labor market programs. If personal characteristics are the main determinants of future labor force states, then policy should target these characteristics. The timing of the intervention may not matter much. On the other hand, if state dependence is significant, policy should aim at preventing unfavorable outcomes from occurring early in a person's career. Disentangling the effect of personal characteristics and the environment from the effect of the previous labor market outcomes is therefore critical for the design of effective labor market policies. To date, most policy evaluations have consisted of before-and-after comparisons with short time horizons for the measurement of the outcomes. Furthermore, existing program evaluations have yet to separate immediate policy effects from longer term impacts through state dependence.[5] In the presence of strong state dependence, this approach will underestimate the total policy impact.

The paper is close in spirit to the literature on the scarring effects of unemployment. However, unlike the other studies in the area, this research uses event history methods and looks at state dependence across three states: employment, unemployment and out of the labor force. The resulting model is more general in the type of state dependence allowed and more precise in the measurement of the timing and duration of spells. For example, we consider the effects of past unemployment experiences on the likelihood of dropping

[5]Notable exceptions are Ham and LaLonde (1996) and Eberwein, Ham, and LaLonde (1997) that use experimental data to separate the effects of job training on the probability of employment and the duration of subsequent spells of employment and non-employment. In these papers at most one new spell is considered.

out of the labor force in the future and histories of continuous longer term employment are compared to experiences with frequent short-term employment spells.

The data concern young Australians making the transition from school to work. We focus attention on a fairly homogeneous group who are especially relevant for policy, namely those with no post-secondary education over the length of the survey. Estimates are provided for the group with post-secondary education for comparison.

There is no evidence of lagged duration dependence in the model estimates. Short-term employment spells are as beneficial in increasing the probability of future employment as long spells. Hence we do not find effects consistent with the acquisition of on-the-job human capital that is transferable across jobs and raises one's employability. Since previous studies suggest that real jobs are more successful in improving future labor market outcomes than the standard subsidized employment (e.g. Gerfin, Lechner, and Steiger, 2004), these results imply that interventions in the form of subsidized jobs are unlikely to have long-term impacts that depend on the length of the job.

There is evidence of occurrence dependence. Those who maintain attachments in the labor market are more likely to be employed in the future. However, spells of unemployment in one's past can counteract the presence of job experiences. Hence the beneficial effect of a subsidized job may be completely undone by a following unemployment spell. We cannot distinguish between several explanations for the occurrence dependence; people who retain attachments to the labor market in the form of employment spells may be developing skills in finding jobs or networks, or employers may be hiring people based on previous histories (e.g. Manning, 2000). It is also possible that the results reflect more complex forms of unobserved heterogeneity than those allowed for in our models.

In summary, the results suggest that programs placing people in employment may have a beneficial effect if the job is perceived as a real job. This is true even for short-term employment. However, the people who move from employment back to a spell of unemployment may lose all beneficial effects from the job since even short-term unemployment spells result in worse labor market outcomes in the future.

3 Data

This section discusses the data set. A description of the institutional and policy environment in Australia at the time of the survey can be found in Doiron and Gørgens (2005). We mention but a few salient facts concerning the income support programs in place during the analysis period 1989–1994. There is no maximum duration for the receipt of government transfers and the size of payments does not depend on the duration of time in the program or on previous earnings. This applies to unemployment assistance and welfare schemes. Hence, the policy incentives are fairly straightforward. Also, no major policy changes were implemented during this time period.

3.1 Australian Youth Survey

The main data source is the Australian Youth Survey 1989–1994 (AYS). The AYS was designed to be representative of the Australian population of young people, with the exception of those living in sparsely populated areas. The AYS consists of six cohorts. The initial cohort were between 16 and 19 years of age in September 1989. Additional cohorts of 16-year-olds were added to the sample in each of the years 1990–1994. Face-to-face interviews were carried out annually from 1989 to 1994.[6]

The AYS is a rich data set which contains detailed demographic and economic information about each respondent. Topics covered by the questionnaire include basic demographic characteristics, family background, secondary school and post-school education, as well as labor market experience and job information. At each interview the respondents were asked to provide detailed week-by-week information about jobs and job search. From the job and job search histories it is possible to determine the labor force status of each person in each week, starting in January of the year of the first interview and ending in the week of the last interview. These weekly labor force status histories form the core data for the analysis in this paper.

To simplify the analysis, at any point in time a person is placed in one and only one

[6]No additions were made to the sample after 1994, but telephone interviews were conducted in 1995 and 1996 for those already selected. Our analysis is restricted to the years 1989–1994, because the information on labor force status collected in the 1995 and 1996 interviews is less detailed.

of three labor force states: out of the labor force (O), unemployed (U), or employed (E). A person is classified as employed if he or she reported having a job, no matter how short the hours and how low the salary. This is standard in the literature (e.g. Ryan, 2001).[7] A person is classified as unemployed if he or she is not employed and is looking for work.[8] Finally, persons not employed and not unemployed are classified as being out of the labor force.

A person's history begins when he or she leaves secondary school, and the event of leaving secondary school is treated as exogenous. The exclusion of experiences while the person is in secondary school is made on the grounds that these labor market profiles in most cases are completely determined by schooling and the type of work performed by school students is likely to be different from other work. We focus attention on the group of people who do not undertake any post-secondary education over the survey period. This group is more homogeneous and of greater policy relevance. The model is also re-estimated on the sample of people who invest in and complete post-secondary education during the sample period and the results on state dependence are compared.

In the AYS data, histories are left-censored if the person left secondary school before the year of the first interview. In addition, all histories are naturally right-censored at the time of the last interview.[9] A few histories have censored periods in the middle. This happens when a person misses a scheduled interview, but participates in the survey again in a following year. The results presented in this paper are based on analysis which excludes all left-censored histories and which right-censor histories at the time of any gap (i.e. we use only the first part of a history with a gap in the middle). This follows the standard approach in the literature (e.g. Mroz and Savage, 2006).[10]

[7]We study transitions between labor force states rather than jobs; transitions from one job to another with no interruption in employment are not modelled.

[8]This is less stringent than the official definition of unemployment used by the Australian Bureau of Statistics. To be counted as unemployed in the official figures, a minimum level of active search effort must be reported and the person must be available to start work within a short period of time. Consequently, the unemployment rates in the analysis sample are slightly higher than official estimates. For example, the unemployment rate observed in our main sample (men and women with no post-secondary education) is over 21% while the Australian Bureau of Statistics national unemployment rate over the same period for people 15–24 years of age and not attending post-secondary institutions is about 19%.

[9]Left-censoring means that states and transitions are not observed during the beginning of the history. Similarly, right-censoring means that states and transitions are unknown after some point in time.

[10]D'Addio and Rosholm (2002b) examine the effects of the exclusion of left-censored spells.

In addition to endogenous variables representing previous labor market experience (discussed in Section 4.3), we consider exogenous explanatory variables representing personal demographic characteristics as well as external environmental factors. While some of our exogenous variables are time-invariant, such as sex and language background, most are time-varying. Age is measured on a monthly scale, and changes are assumed to occur between successive calendar months. Information about the place of residence and marital status is collected for the time of each interview. For these variables, it is assumed that any change occurs on the day following the previous interview.

To capture differences in the external environment, local unemployment rates are included. The AYS does not contain information about local labor markets, but it does have detailed information about the respondents' residence at the time of the interview (either census collector's districts or postcodes). Using this information, it is possible to merge local unemployment rates published by the Australian Bureau of Statistics (ABS) with the AYS data. The ABS figures are available monthly by sex, age group and statistical region.[11] A local unemployment rate is assigned to each person in each month. Finally, some specifications include dummies for calendar year and month, state/territory, and the degree of urbanization.

3.2 Data overview

There are altogether 11431 persons in the AYS. Of these, 5439 have left secondary school at the time of the last interview and have non-missing values for all explanatory variables used in the analysis.[12] One quarter of the sample or 1363 people do not undertake any post-secondary education during the sample period and form the main analysis sample. Table 1 shows that the average length of time under analysis for this group is 632 days. Of the time under observation, 65 days (10% of the total time) are spent out of the labor

[11] Australia was divided into 65 statistical regions during the analysis period. The ABS unemployment rates for people 15–24 years of age are used in the main model presented below. Alternative specifications including models with lagged unemployment rates were also estimated and the results on state dependence were unaffected.

[12] Of the 11431 respondents, 2733 are in school at their last interview, 3222 have left-censored histories, 21 have missing background variables, and 16 have missing or invalid information about educational qualifications. Of the remaining 5439 respondents, 1130 leave the survey before wave 6 (attrition) and 4309 complete their wave 6 interview.

force, 135 days (21%) unemployed and 433 days (68%) employed. The longest observed history is 2048 days or 5.6 years.

The table also shows summary statistics for the spells. In total there are 4718 spells, of which 20% are out of the labor force, 35% are unemployment and 44% are employment. The fact that unemployment accounts for only 21% of total analysis time but 35% of the spells, while employment accounts for 44% of the spells and 68% of the time reflects the fact that spells of unemployment tend to be much shorter than spells of employment.

The majority of the AYS respondents (912 or 67%) were employed at the time of their last interview. The next three rows of the table show the transition matrix from one spell to the next. From unemployment, substantially more spells end with transition into employment than to out of the labor force (1151 versus 211). The transitions from employment to unemployment are more frequent than out of the labor force (836 versus 346) and there are more transitions from out of the labor force into unemployment than into employment (465 versus 346).

The bottom panel of Table 1 shows quantiles of the distribution of spells. For example, the first entry shows that 10% of spells out of the labor force are shorter than 6 days and that 90% are longer than 6 days. The median length of a period spent out of the labor force is 36 days, or just over one month. The median length of unemployment and employment spells are 72 days and 244 days respectively. The shortest spells are out of the labor force while the longest spells are in employment. This is true at all deciles of the distributions.

4 Econometric framework

4.1 Outcome and explanatory variables

The outcome (or dependent) variable for person i is his or her history; that is, the transition times and destination states. Let $T_{i,0}$ and $S_{i,0}$ denote the point in time when history begins and the initial state. In this application, $T_{i,0}$ is the (calendar) time when person i leaves secondary school and $S_{i,0}$ indicates whether he or she is out of the labor force (O), unemployed (U) or employed (E) immediately after leaving school. Let $T_{i,j}$ and $S_{i,j}$ for

$j = 1, 2, \ldots$ denote subsequent transition times and destination states (with $T_{i,j-1} < T_{i,j}$ and $S_{i,j-1} \neq S_{i,j}$). Person i's history is observed over the period $(T_{i,0}, C_i]$, where C_i is a random variable representing the time of the last interview. Let N_i denote the number of transitions during $(T_{i,0}, C_i]$.

It is necessary to distinguish between exogenous and endogenous explanatory variables in the notation. Let $X_i(t)$ denote a vector of exogenous explanatory variables at time t for person i, and let $\mathbf{X}_i(t)$ denote the path of the explanatory variables from the beginning of time until time t.[13] Let $\mathbf{Y}_i(t, s)$ denote the history of outcomes from the beginning of time until time t; that is, $\mathbf{Y}_i(t, s) = \{T_{i,j}, S_{i,j}\}_{j=0}^{J_i(t)}$, where $s = S_{i,J_i(t)}$ and $J_i(t)$ is the maximal integer such that $T_{i,J_i(t)} \leq t$.[14]

A main aim of this paper is to disentangle the effect of previous labor market outcomes (state dependence) from other factors such as personal characteristics and the external environment (heterogeneity). Since any heterogeneity not accounted for is likely to induce spurious state dependence, it is important to minimize the effect of unobserved heterogeneity. To this end, we follow the literature and include random effects in the model. Let V_i be a random vector representing unobserved personal and environmental characteristics. It is assumed that V_i is person-specific, time-invariant, and independent of observed and time-variant exogenous personal characteristics and environmental factors.

4.2 Transition intensities and the likelihood function

Assuming continuous measurement of time, let $h(t, s|\mathbf{y}(\tilde{t}, \tilde{s}), \mathbf{x}(t), v)$ denote the conditional transition intensity to state s at time t given that the current spell in state $\tilde{s}$ began at time $\tilde{t}$, and given the history, $\mathbf{y}(\tilde{t}, \tilde{s})$, the path of exogenous variables, $\mathbf{x}(t)$, and value of unobserved characteristics, v.

Conditional on $\mathbf{X}_i(C_i) = \mathbf{x}_i(c_i)$ and $V_i = v_i$, the contribution to the likelihood function

[13] To simplify the exposition, we assume that the exogenous explanatory variables are "external", as defined by Kalbfleisch and Prentice (1980, p123).

[14] The argument s in $\mathbf{Y}_i(t, s)$ is redundant, but we find it helps to clarify the discussion.

of person i's history can be expressed as the product of the contributions of each spell,

$$\begin{aligned} &L\big(\mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), c_i | \mathbf{x}_i(c_i), v_i\big) \\ &\qquad = L\big(c_i | \mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), \mathbf{x}_i(c_i), v_i\big) \Bigg(\prod_{j=1}^{n_i} L\big(t_{i,j}, s_{i,j} | \mathbf{y}_i(t_{i,j-1}, s_{i,j-1}), \mathbf{x}_i(t_{i,j}), v_i\big) \Bigg) \\ &\qquad\qquad \times L\big(s_{i,0} | t_{i,0}, \mathbf{x}_i(t_{i,0}), v_i\big) L\big(t_{i,0} | \mathbf{x}_i(t_{i,0}), v_i\big), \end{aligned} \tag{1}$$

where lowercase letters are used for realized values of random variables. In this paper, we focus on the transitions and do not model the decision to leave school nor the first labor force state.[15] In terms of (1), this means that the last two terms are omitted and the contributions are effectively conditioned on $T_{i,0}$ and $S_{i,0}$.[16]

Conditional on $\mathbf{Y}_i(t_{i,j-1}, s_{i,j-1}) = \mathbf{y}_i(t_{i,j-1}, s_{i,j-1})$, $\mathbf{X}_i(t_{i,j}) = \mathbf{x}_i(t_{i,j})$ and $V_i = v_i$, the contribution to the likelihood of the event of person i moving to state $s_{i,j}$ at time $t_{i,j}$ is

$$\begin{aligned} L\big(t_{i,j}, s_{i,j} | \mathbf{y}_i(t_{i,j-1}, s_{i,j-1}), \mathbf{x}_i(t_{i,j}), v_i\big) &= h(t_{i,j}, s_{i,j} | \mathbf{y}_i(t_{i,j-1}, s_{i,j-1}), \mathbf{x}(t_{i,j}), v_i) \\ &\times \exp\Bigg(- \sum_{\substack{k=\mathrm{O,U,E} \\ k \neq s_{i,j-1}}} \int_{t_{i,j-1}}^{t_{i,j}} h(u, k | \mathbf{y}_i(t_{i,j-1}, s_{i,j-1}), \mathbf{x}_i(u), v_i)\, du \Bigg). \end{aligned} \tag{2}$$

The right-hand side of (2) is similar to the "hazard function times survivor function"-expression familiar from the analysis of single-spell duration data. The first term is the intensity of moving to state $s_{i,j}$ at time $t_{i,j}$ and the second, exponential term is the probability of no events taking place between time $t_{i,j-1}$ and time $t_{i,j}$.

Assume that the end time, C_i, is independent of the transition process and of observed and unobserved heterogeneity.[17] Then C_i is uninformative about parameters of interest. Ignoring the distribution of C_i in the likelihood function, the contribution of the last

[15] This simplification is common in the literature, see for example Gritz (1993), Bonnal, Fougère, and Sérandon (1997), and D'Addio and Rosholm (2002a).

[16] The predictions presented later in the paper do not condition on $S_{i,0}$; they are based on the simplest possible model of the conditional distribution of $S_{i,0}$ given $T_{i,0}$, $\mathbf{X}_i(t_{i,0})$ and V_i, namely the unconditional distribution of $S_{i,0}$, estimated by the sample frequencies.

[17] This is a standard, and in most cases unproblematic assumption, since C_i is simply the time of the 1994 interview for most AYS respondents, and the time of the 1994 interview is exogenous. The assumption may be violated for respondents who are lost to the survey before 1994 to the extent that attrition from the panel is correlated with changing labor force status.

right-censored time period becomes

$$L\big(c_i|\mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), \mathbf{x}_i(c_i), v_i\big) = \exp\bigg(-\sum_{\substack{k=\mathrm{O,U,E}\\ k\neq s_{i,n_i}}} \int_{t_{i,n_i}}^{c_i} h(u, k|\mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), \mathbf{x}_i(u), v_i)\, du\bigg). \tag{3}$$

This is simply the probability that no events took place between t_{i,n_i} and c_i.

After integrating out the random effect, V_i, the contribution to the likelihood for person i is (using a Stieltjes integral[18])

$$L_i = \int_{-\infty}^{\infty} L\big(\mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), c_i|\mathbf{y}_i(t_{i,0}, s_{i,0}), \mathbf{x}_i(c_i), v\big)\, dA^*(v), \tag{4}$$

where A^* is the marginal distribution of V_i at time $T_{i,0}$ in the population.

Following common practice in this literature, V_i is assumed to take only a small number of different values. These values are often thought of as different "types" of persons. Let the discrete support of V_i be $\{\nu_1, \ldots, \nu_M\}$ and let the corresponding probability function be $\pi_m = \Pr(V_i = \nu_m)$. Then (4) becomes

$$L_i = \sum_{m=1}^{M} L\big(\mathbf{y}_i(t_{i,n_i}, s_{i,n_i}), c_i|\mathbf{y}_i(t_{i,0}, s_{i,0}), \mathbf{x}_i(c_i), \nu_m\big)\pi_m. \tag{5}$$

Allowing for more types yields a more flexible distribution of unobserved heterogeneity. In practice, researchers have usually estimated models with a small number of types. In the main model presented below there are three types.

4.3 Parameterization and estimation

In general, the transition intensities may depend on the entire paths of the exogenous and endogenous variables, $\mathbf{X}_i(\cdot)$ and $\mathbf{Y}_i(\cdot,\cdot)$. However, in practice it is not possible to condition on every aspect of the past history. Regarding the exogenous variables, it is assumed that $X_i(\cdot)$ is sufficiently rich that only contemporaneous values affect the

[18]The Stieltjes integral allow for both discrete and continuous V_i. If V_i is continuous, the integral can be written in the more familiar form $\int(\cdots)a^*(v)\,dv$, where a^* is the density corresponding to A^*. The discrete case is given in (5).

transition intensities. This assumption is standard in the literature and can usually be satisfied by including variables which represent current information about previous times in $X_i(\cdot)$. Regarding the endogenous variables, it is assumed that the transition intensities depend on the path, $\mathbf{Y}_i(t,s)$, through a (finite-dimensional) random vector, $Y_i(t)$, whose components summarize $\mathbf{Y}_i(t,s)$. (This vector does not depend on s as the effect of the current state is captured by variation in parameters.) Again, this is standard.

Three sets of variables are included in $Y_i(t)$: the presence and type of the previous spell (i.e. the spell preceding the current spell), the number of previous spells in each labor force state (i.e. all spells before the current spell), and the cumulative duration in each state prior to the current spell. Note that these variables are time-varying, but constant within each spell. The model also includes elapsed time in the current spell to capture dynamics within spells.

In preliminary work we allowed for "depreciation" of the previous history of outcomes. Let ρ_s denote the state-specific depreciation rate ($\rho_s \leq 0$). The discounted cumulative number of previous spells (or spell-endings) in state s from time 0 to time $\tilde{t}$ is

$$e^{\rho_s \tilde{t}} \sum_{j=1}^{J_i(\tilde{t})} 1(S_{i,j-1} = s) e^{-\rho_s T_{i,j}}. \tag{6}$$

The discounted cumulative duration in state s from time 0 to time $\tilde{t}$ is

$$e^{\rho_s \tilde{t}} \sum_{j=1}^{J_i(\tilde{t})} 1(S_{i,j-1} = s) \int_{T_{i,j-1}}^{T_{i,j}} e^{-\rho_s u}\, du. \tag{7}$$

The null hypothesis that $\rho_s = 0$ for all s was not rejected and the discounting parameters are set to zero in all specifications discussed in the paper.

The vector of unobserved heterogeneity, V_i, is assumed to have six components, one for each transition. Thus, each point in the distribution of V_i, that is, $\{\nu_1, \ldots, \nu_M\}$, is a six-dimensional vector. No prior assumptions are made about the location of the support points. In particular, the correlation between components representing different transitions is not restricted. In the main analysis, $M = 3$, which results in 20 unknown parameters for the distribution of V_i. Of these, 18 relate to the support and 2 to the probability function.

While the support of V_i is defined in terms of M six-dimensional vectors, it is convenient to represent it as six M-dimensional vectors. Accordingly, let $\nu_{\tilde{s},s}$ denote the M-dimensional vector obtained by stacking the particular components of $\nu_1, \ldots, \nu_M$ which represents the transition from $\tilde{s}$ to s. Moreover, let $z(v) = \big(1(v = \nu_1), \ldots, 1(v = \nu_M)\big)'$ be an M-dimensional vector function indicating the support point. Then $z(v)'\nu_{\tilde{s},s}$ is the component of the support of V_i corresponding to the transition from $\tilde{s}$ to s for type v.

The transition intensities are modelled with a "proportional hazards" specification.[19] Specifically, the conditional transition intensity is[20]

$$h(t, s|\mathbf{y}(\tilde{t}, \tilde{s}), \mathbf{x}(t), v) = \lambda_{\tilde{s},s}(t - \tilde{t}; \alpha_{\tilde{s},s}) \exp\big(x(t)'\beta_{\tilde{s},s} + y(\tilde{t})'\delta_{\tilde{s},s} + z(v)'\nu_{\tilde{s},s}\big),$$
$$t \geq \tilde{t},\ s \neq \tilde{s},\ v \in \{\nu_1, \ldots, \nu_M\}, \quad (8)$$

where $\lambda_{\tilde{s},s}(\cdot; \alpha_{\tilde{s},s})$ is the "baseline" transition intensity from state $\tilde{s}$ to state s and $\alpha_{\tilde{s},s}$, $\beta_{\tilde{s},s}$ and $\delta_{\tilde{s},s}$ are additional parameter vectors. The baseline intensity is parameterized as a product of a Weibull function and a piecewise constant function,

$$\lambda_{\tilde{s},s}(t - \tilde{t}; \alpha_{\tilde{s},s}) = \alpha_{1,\tilde{s},s}(t - \tilde{t})^{\alpha_{1,\tilde{s},s}-1} \exp\left(\sum_{k=2}^{K_{\tilde{s},s}} \alpha_{k,\tilde{s},s} 1(\tau_{k-1} < t - \tilde{t} \leq \tau_k)\right), \quad (9)$$

where $\tau_1 = 0$, $\tau_{k-1} < \tau_k$ and $\tau_{K_{\tilde{s},s}} = \infty$. In the main model, $K_{\tilde{s},s}$ varies between 4 and 8 depending on the transition. (Specific values of τ_k are given below.) This is based on experimentation with various specifications. Finally, $\alpha_{K_{\tilde{s},s},\tilde{s},s}$ is normalized to equal zero.

The unknown parameters $\alpha_{\tilde{s},s}$, $\beta_{\tilde{s},s}$, $\delta_{\tilde{s},s}$, $\nu_{\tilde{s},s}$ and $\pi_1, \ldots, \pi_M$, are estimated by the method of maximum likelihood.

[19]See e.g. Kalbfleisch and Prentice (1980, chapter 7).

[20]A normalization of each transition intensity is necessary to identify the parameters in (8), such as fixing the scale of the baseline intensity $\lambda_{\tilde{s},s}$ or the location of the $\nu_{\tilde{s},s}$. We choose the former.

5 Estimation results and fit of the model

5.1 Estimated transition intensities

Table 2 presents estimates for a model containing a fairly parsimonious set of variables other than the state dependence variables. Since each variable affects six transition intensities, the model still includes a large number of parameters, namely 212. More general specifications including dummies for state/territory, rural/urban area, calendar month, and year were also estimated and although these dummies were statistically jointly significant, the results on state dependence are unaffected.[21]

The estimates presented in Table 2 are based on a combined sample of males and females. Interactions of a female dummy variable with variables representing household composition are included to capture variations by sex. Separate models for males and females yield similar overall results on state dependence. We indicate the main differences in the separate male and female estimations in the discussion below and include simulation results based on the separate models for comparison in the next section.

As mentioned, the main model includes many parameters for state dependence: current duration dependence as captured by the shape of the baseline transition intensity, proportional differences in the baseline intensity caused by the presence and type of a previous spell, the cumulative number of previous spells by type (occurrence dependence) and the cumulative duration of spells by type (lagged duration dependence). Although the parameters are individually mostly insignificant, jointly the parameters for each kind of state dependence are significant, except for lagged duration dependence. We keep the large number of parameters to get a good fit for the model and simulation results that represent well the patterns found in the event history data.[22]

Consider first the effect of elapsed time in the current spell; that is, current duration dependence.[23] As mentioned, the baseline transition intensity is modelled as a product of

[21]These and other results referred to in this section are available from the authors.

[22]As described above, models incorporating depreciation of past history were estimated and no statistically significant evidence of depreciation was found. In addition, models including separate lagged durations by type for the spell preceding the current spell were estimated; again, these effects were quantitatively and statistically insignificant.

[23]In the following, analysis time is measured in days, and previous durations are measured in months.

a Weibull function and a piecewise constant function. The parameter labelled "Weibull $\ln \alpha$" indicates the long-run slope of the baseline intensity; if $\ln \alpha$ equals zero, the baseline intensity is eventually constant. The parameters labelled "Elapsed *a*–*b*" indicate a deviation from the Weibull relationship which applies *a*–*b* days into the spell. The implied shapes of the baseline transition intensities are shown in Figure 1. The curves are plotted from 0 to the 99th percentile of the observed uncensored transitions.

The hypothesis of constant baseline intensities (by transition) cannot be rejected for transitions between unemployment and out of the labor force and vice versa. There is a significant decrease in the transition intensity from unemployment into employment over the first month of the spell, however the long-run intensity is increasing; that is, movements into employment are more likely the longer the person remains unemployed. This is contrary to what is usually found for the general population, but it is consistent with the findings for youth in other countries. For example, D'Addio and Rosholm (2002a) found increasing transition intensities for French youths. As they pointed out, this could be evidence of a declining reservation wage among the young cohorts. A similar pattern is found for transitions from out of the labor force into employment.

The baseline intensities for transitions out of employment also have similar shapes for the two exit states. In both cases, there are large decreases in the baseline intensities for the first year into the employment spell, a possible indication of the occurrence of good matches or firm-specific human capital acquisition. The long-run transition intensity has a significant positive slope for exits into unemployment.

The remaining estimates represent approximate proportional effects of each explanatory variable on the transition intensities. We begin with the effects of previous labor market outcomes. It is important to keep the interrelation between variables in mind when interpreting these results. The estimates for the cumulative previous number of spells indicate the effect of having had an additional spell in the past, holding the total time in each state constant. Essentially these variables are capturing the difference between having had many short spells as opposed to a few long spells. The estimates for cumulative previous duration indicate the effect of an additional month spent in a particular state in the distant past, holding the number of spells in each state constant; that is, the effect of a spell of a given type being one month longer. The variables for type of previous spell capture the influence of recent history, over and above the effects captured by the cumulative variables. The coefficients on the previous spells must be added to those on the cumulative number of spells to obtain the total impact of the occurrence of a previous spell.

The most striking result in Table 2 is the lack of any lagged duration dependence. The parameters on cumulative previous durations are very small, and they are jointly and individually insignificant. In particular, more time spent in employment does not affect the probability of entering or leaving employment in the future. If on-the-job human capital acquisition occurs, it does not have a lasting effect on the likelihood of future employment spells. When separate models are estimated for males and females there is limited evidence of lagged duration dependence in that the joint hypotheses of zero coefficients are rejected for both groups. However the effects are quantitatively very weak and will be illustrated in simulations further below.

There is evidence of occurrence dependence. Parameters on the presence and type of a previous spell are jointly statistically significant as are the parameters on the cumulative number of spells by type. Although some of these parameters are quantitatively important, none of them are individually significant except for the effect of the occurrence of previous employment spells which raises the transition intensities from unemployment into employment. The estimates suggest that previous spells of employment make transitions into employment more likely for those who are not employed. Conversely, previous spells of unemployment tend to lower the transition intensities into employment as well as raise the transition intensities from employment to unemployment. The magnitude of these effects will be explored using simulations. One finding that we will return to when presenting simulation results is the opposite effects of additional spells of unemployment versus employment. This result also holds for the separate male-female models.

We now turn briefly to the effects of demographics, education and local labor market conditions. Local unemployment rates have expected effects and the coefficients are jointly statistically significant. The main effect of high unemployment is to reduce the transition intensity into employment. Although not individually statistically significant, it is also interesting to note that the employed are less likely to leave their jobs in slack labor markets. This is consistent with the theory proposed by Hall (2005) for the general population that during slack periods, unemployment rises mainly due to low hiring rates rather than increased separations.[24]

Previous work based on aggregate statistics shows a strong positive relationship between education and employment probabilities for Australia and other OECD countries (e.g. Blanchflower and Freeman, 2000). Variation in highest educational qualifications in the model is limited to comparisons across people with 10, 11 or 12 years of schooling but these comparisons generally support that finding. Persons with more years of education are more likely to enter employment and less likely to leave an employment spell. The same is generally true of older people.

The coefficients on unpartnered females without children are jointly insignificant compared to equivalent males.[25] Partnered women are less likely than unpartnered women to begin searching if out of the labor force and more likely to drop out of the labor force. The presence of children reduces transition intensities into employment and increases transition intensities out of the labor force for women. The effects of partnering and children are much smaller in magnitude and jointly statistically insignificant for men. These results are consistent with general findings on the effects of household composition on the labor supply of men and women (e.g. Blau and Kahn, 2000). Neither the variables for language background nor country of birth are jointly significant.[26] The estimates imply that disabled persons are more likely to spend time not employed and not searching.[27]

Finally, the estimated parameters of the distribution of unobserved heterogeneity are

[24] A recent study by Elsby, Michaels, and Solon (2007) finds evidence of both countercyclical inflow rates and procyclical outflow rates for the general population.

[25] A person is partnered if he or she is married or in a de-facto relationship. A de-facto couple consists of two people who live together in the same household, who are not registered as married to each other, and who report being de facto.

[26] Language background is derived from the question asked in the first interview: Which language or languages did you first speak? A total of 1069 respondents do not mention English. Of these, 420 were born in Australia and 649 abroad (in decreasing order: Vietnam, Hong Kong, Lebanon, Philippines, Poland, Malaysia, Germany, Chile, Yugoslavia etc.). Virtually all Aborigines mention English.

[27] Disability is self-reported and includes physical and mental health problems.

presented. There are three mass points in the model presented, and their estimated probabilities are almost equal. Models with four mass points have also been estimated but the results are virtually unaffected.[28]

5.2 Model fit and simulation methods

Before proceeding, we check that the model fits the main characteristics of the data. There is no simple test available for this purpose; instead informal checks are conducted based on simulations. For a given set of exogenous variables, simulations are done dynamically over time. The first step is to draw a value of the random effect from the estimated distribution of types and probabilities. The second step is to determine the labor force state immediately after leaving secondary school. This is done using the marginal distribution of first states. Then a transition time and a destination state are drawn in accordance with the time path of the exogenous variables, the random effect (now fixed), and the estimated model. After the transition takes place, the endogenous explanatory variables representing past history are updated to reflect the type and duration of the first spell. Then the second transition time and destination state are drawn using updated values for the endogenous variables. This process is continued over the period of observation of the exogenous variables. The end result of this process is a random history which is statistically compatible with the given path of the exogenous explanatory variables. All simulation results presented in this paper are averaged over the distribution of unobserved heterogeneity.

In order to assess how well the estimated model fits the data, 10 realizations are simulated for each person in the analysis sample. Summary statistics are computed for the simulated outcomes and compared to the corresponding values based on the raw data. Selected results are shown in Table 3. Figure 2 shows empirical and simulated distribution functions for spells in each state as well as nonparametric estimates of the unconditional

[28] A likelihood-ratio test rejects the three-point against the four-point model with p-value of 0.0%, but the probability of the fourth mass point is only 2% and the parameter estimates are virtually unchanged. One-point (no random effects) and two-point models are strongly rejected against the preferred three-point model with p-values of 0.0%. In these latter cases, parameter estimates are substantially affected.

transition intensities.[29] The table and figure show that the model generally fits the data well, although the predicted number of short employment spells is somewhat low. Of the separate models for males and females, the former performs slightly worse and the latter slightly better in this respect.

6 Effects of interventions

The presence of occurrence dependence suggests that the medium- to long-term effects of policy interventions may differ markedly from the short-term impacts. In this section we investigate the implications of the estimated parameters for persistence in labor force states by simulating the effects of interventions that force or prevent transitions between labor force states at certain times in a person's history.

To focus on the effect of state dependence, outcomes are simulated for representative persons living in a stationary environment. Specifically, simulations are performed for a person, male or female, who leaves secondary school after completing grade 10 on the day he or she turns 16 and does not pursue further education during the nine-year simulation period. The representative person is born in Australia, from an English-speaking background, does not live with his/her parents, has no disability, and remains unpartnered and child-less during the simulation period. The local unemployment rates are fixed at their average sample values. The age of the person is progressed as appropriate.

In all cases, the intervention is applied only to persons who are 90 days or more into an unemployment spell on their 20th birthday. The fraction of persons who are eligible for the intervention vary depending on the model that is simulated, but is typically around 30%.[30] The results presented below are based on 10000 eligible persons.

The simulation results are presented graphically as days spent in each state by age, measured on a monthly scale. The graphs show the treatment effect on the treated; that is, the outcomes for the group affected by the intervention minus the outcomes they would

[29]The nonparametric estimates are computed using local linear "external" smoother with a boundary correction at 0 proposed by Müller and Wang (1994). The second-order "bi-weight" kernel was used with a bandwidth of 60 days.

[30]For the model presented in Section 5, the fraction of males and females treated is 0.33 and 0.24, respectively.

have had had the intervention not taken place.

Figure 3 shows simulation results for the model presented in Section 5. In the top left-hand panel, the intervention consists of an employment spell lasting 30 days. When the job ends, the person rejoins the group of unemployed, the job search clock is restarted and the person's history is updated to reflect an additional spell of employment of 30 days, as well as the original unemployment spell. During the 30-day employment period, transitions to unemployment and out of the labor force are prohibited. This intervention can be thought of as a subsidized employment program, where the subsidized job is short but it is considered as a typical job in providing employment experience. The experiment is labelled "30 day program E".

The middle left-hand graph illustrates a shock to unemployment. A person who has been unemployed for 90 days or more when they reach their 20th birthday is prevented from leaving unemployment for a period of 30 days. This can be thought of as a training program such that trainees are still unemployed when finishing the program and there is no direct effect of the program on future employment probabilities. Accordingly, after finishing the program the unemployment clock is not reset and the time spent in the program is subsequently treated as time spent unemployed. Thus, persons participating in the program are nominally worse off, because they are prevented from accepting a job during the training period. This experiment is labelled "30 day program U".

The bottom left-hand panel illustrates the effects of an intervention in which those unemployed for 90 days or more on their 20th birthday are taken out of the labor force for a period of 30 days. After the 30 day period, they are replaced in the unemployment queue and the job search clock is restarted. This can be thought of as a training program during which participants do not search for jobs. Compared to the previous experiment, workers and employers now consider the training program as time out of the labor force. This experiment is labelled "30 day program O".

These interventions are designed to have meaningful interpretations while illustrating the differences in the amount of state dependence across the three labor force states. Comparisons between corresponding left- and right-hand graphs show the magnitude of duration dependence. Specifically, the right-hand panels represent similar interventions on employment, unemployment and out of the labor force, the only difference being that the interventions are 180 days in length instead of 30.

The lack of duration dependence is evident in the comparisons between the left- and right-hand graphs. The 180-day intervention has no more lasting effect than the 30-day intervention. The middle panels also represent duration dependence effects from unemployment since this intervention acts as an extension of the existing unemployment spell.[31]

The impact of occurrence dependence is more complex and medium-term effects are only evident in the bottom panels. In the top panels, unemployment is interrupted by a spell of employment which is of fixed duration. At the end of this spell, the histories have been augmented by one spell of employment and one spell of unemployment. As seen in Table 2, these have counteracting effects and there is hardly any net impact of this intervention. The bottom panels show the effects of the addition of a spell of unemployment and a spell out of the labor force. The unemployment experience is not counteracted by an employment spell in this case and we see a lasting effect of about 4 additional days spent in unemployment per 30 days. This represents an increase of 13 to 16 percentage points in the unemployment rate for this group. The increased time in unemployment corresponds to a reduction in the time spent in employment and there is is no discernable effect on time spent out of the labor force.

Figure 4 presents additional results based on the main model. The top panels correspond to an intervention similar to that in the bottom panels of Figure 3; that is, we place the eligible unemployed in a spell out of the labor force lasting 30 days after which they return to unemployment. In the left-hand panel, the coefficients on the cumulative number of previous spells are set at zero. In the right-hand panel, the coefficients on both the cumulative number of previous spells and the cumulative lagged durations are set to zero. The two graphs are virtually identical and there is no lasting effect from the intervention. These experiments confirm that the state dependence is based on the

[31] The increase in time spent unemployed is small since the counterfactual group also tends to stay unemployed in the absence of any intervention.

number of spells by type in one's history.

The bottom panels illustrate a fourth type of intervention. Here, people who are unemployed for at least 90 days on their 20th birthday are placed in an employment spell which is indistinguishable from "real" jobs in the sense that transition intensities into unemployment or out of the labor force are the same and accumulated employment is the same. This intervention can be thought of as a successful job search program for those who are medium-term unemployed. This experiment is labelled "Real job". The left-hand panel shows the effect of the treatment on the treated for males and the right-hand graphs for females. Women have lower transition intensities out of employment and this generates stronger effects of the job placement. This is consistent with the findings of D'Addio and Rosholm (2002a) for young males and females in France.

Figure 5 presents simulations based on separate models for males and females. The specification of these models is the same as that discussed in Section 5, except that female dummy variables and their interactions are excluded and slightly fewer parameters are included in the baseline transition intensities.[32] The panels correspond to the three right-hand panels in Figure 3; that is, interventions of 180 days. We see slightly stronger effects from the employment spell for women, and weaker effects of the extended unemployment spell due to the lower transition intensities out of unemployment for the counterfactual group. Overall, state dependence results are very similar for males and females, and separate estimation does not change the conclusions discussed above.

Finally, Figure 6 presents simulations based on a model estimated on the sample of people who are observed to undertake post-secondary education and who finish their program of study over the sample period. There are 1528 such persons. The specification of the transition intensities is similar to that presented above except for the addition of variables representing intensity of study (part time, full time, apprenticeship), duration of study interacted with current student status, highest educational achievement, and separate history variables for experience accumulated while the person was studying or after having finished his or her program.[33] Figure 6 presents simulation results for people

[32] A likelihood ratio test rejects the pooled model with a p-value of 0.0%.

[33] The specification of the baseline transition intensities is slightly simplified. The model has a total of

with a one-year certificate after finishing school. We present only selected results to indicate where we believe the differences with the group without post-secondary education are important.[34]

The top panels illustrate the effects of the intervention which consists of placing people who have been unemployed for at least 90 days on their 20th birthday in a "real" job. The left-hand graph shows the results for males and the right-hand graph for females. The gender differences observed previously are not evident for the group with post-secondary education. For both males and females, there are similar lasting effects of job placements in that a similar proportion of people stay in the employment spell over the length of the simulations. The middle two panels show that for males there is evidence of duration dependence in this model. The left-hand graph illustrates the impact of a 30 day extension of the unemployment period (the "30 day program U" described above) while the right-hand graph shows the results of a similar intervention lasting 180 days. As seen in the right-hand panel, there are lasting effects of the longer unemployment spell of around 2 to 3 days per 30 days. Finally, occurrence dependence is less important for this group. The bottom graphs show the effects for males of a 180 day intervention on employment (left-hand panel) and out of the labor force (right-hand panel). In both cases, there is evidence of limited effects in greater (less) time spent in employment (unemployment) of around one day per 30 days. These are due to the break created in the unemployment spell on the person's 20th birthday. The effects are the same for a break caused by an employment spell and a spell out of the labor force. Thus occurrence dependence in employment is not as important for the group with post-secondary education; however, unemployment durations in the past do seem to matter in determining future employment and unemployment probabilities.[35]

326 parameters.

[34]The group with higher education has shorter histories in the data than the low-skilled group and we are less confident in projecting results of state dependence over time.

[35]Since the low-skill group that forms the main analysis sample may have included people who eventually chose to continue their education, the results may have been biased due to a selection effect. However, the presence of duration dependence for the group with higher education suggests that such a bias would have caused a finding of duration dependence in the low-skill group as well.

7 Concluding remarks

The finding of strong duration dependence in explaining the length of unemployment spells has influenced the design of many labor market policy reforms. However, very little work has been done on more complex effects of labor market experiences and in particular on the causal effects of past outcomes involving other labor force states. In this paper we use longitudinal data to investigate the extent and type of state dependence in labor market outcomes for young Australians with no post secondary education. The model uses transition data for young Australians with no post-secondary education and incorporates observed and unobserved heterogeneity along with a flexible specification for the state dependence. The model includes parameters representing current duration dependence, previous occurrence dependence (recent spells and cumulative number over time) and cumulative lagged duration dependence. Furthermore, three distinct labor force states are considered (employment, unemployment, and out of the labor force), rather than the customary two states (employment and nonemployment).

We find evidence of occurrence dependence but not lagged duration dependence. People who stay active in the job market in the sense that they have employment experiences have higher probabilities of finding a job in the future regardless of the length of time spent in the previous employment spells. The same is true for unemployment spells and it is easy to undo the benefits of previous employment experiences with the addition of unemployment spells.

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Table 1: Data overview

	Origin state			Total
	O	U	E	
Number of histories				
Total				1363
Time under observation (days)				
Average per person	65	135	433	632
Per cent	10	21	68	100
Maximum history length				2048
Number of spells				
Total	956	1668	2094	4718
Right-censored	145	306	912	1363
Uncensored	811	1362	1182	3355
Destination state				
O	0	211	346	557
U	465	0	836	1301
E	346	1151	0	1497
Incidence rates (exits per 365 days)				
Total	3.4	2.7	0.7	
Destination state				
O	0	0.4	0.2	
U	1.9	0	0.5	
E	1.4	2.3	0	
Duration quantiles (days)				
10%	6	7	18	
20%	7	20	41	
30%	14	28	83	
40%	25	47	139	
50%	36	72	244	
60%	59	104	453	
70%	90	160	969	
80%	169	229	1873	
85%	222	284	1930	
90%	308	370		

Legend: O: out of the labor force; U: unemployed; E: employed. *Notes:* Quantiles based on the Kaplan-Meier product limit estimator. The 90th percentile is not identified for E spells due to right-censoring.

Table 2: Parameter estimates

	O→U	O→E	U→O	U→E	E→O	E→U
Elapsed duration						
Wald test (all†=0)	5.64	18.83	2.39	69.97	20.36	66.51
p-value	0.23	0.00	0.88	0.00	0.01	0.00
Wald test (Weibull=0) 54.55 (6) 0.000						
Wald test (Elapsed=0) 234.75 (30) 0.000						
Weibull $\ln\alpha$	−0.13	0.10	0.18	0.19**	0.24	0.26**
	0.23	*0.44*	*0.16*	*0.08*	*0.19*	*0.08*
Elapsed 1–7	−0.45	0.95	0.61	1.01**	2.35**	3.29**
	1.35	*1.83*	*0.94*	*0.36*	*1.11*	*0.48*
Elapsed 8–14	0.16	0.84**	0.12	1.34**	1.73	2.88**
	0.37	*0.39*	*0.53*	*0.41*	*1.54*	*0.49*
Elapsed 15–28	−0.03	−0.05	0.29	0.79**	2.19*	2.47**
	0.53	*1.03*	*0.70*	*0.30*	*1.16*	*0.48*
Elapsed 29–56			0.28	0.32	1.84	2.13**
			0.47	*0.27*	*1.28*	*0.40*
Elapsed 57–112			0.02	0.24	1.62	1.85**
			0.24	*0.18*	*1.06*	*0.32*
Elapsed 113–182					1.22	1.30**
					0.89	*0.25*
Elapsed 183–364					0.68	1.03**
					0.59	*0.19*

Continued next page. †Joint test of "Weibull $\ln\alpha$" and all "Elapsed a–b" parameters by transition.

Table 2 continued

	O→U	O→E	U→O	U→E	E→O	E→U
Previous spell (base: persons with no previous spell)						
Wald test (spell=0) 68.00 (12) 0.000						
Previous O spell			0.17	0.18	0.06	−0.18
			0.71	*0.41*	*0.70*	*0.77*
Previous U spell	0.00	1.84			0.06	0.46
	1.34	*1.40*			*1.13*	*0.31*
Previous E spell	−0.03	0.85	−0.18	0.30		
	0.89	*1.20*	*0.38*	*0.25*		
Cumulative previous spells						
Wald test (spells=0) 102.64 (18) 0.000						
Cum prv O spells	−0.02	−0.05	0.11	−0.10	0.18	−0.04
	0.40	*0.66*	*1.05*	*0.08*	*0.96*	*0.50*
Cum prv U spells	−0.15	−0.17	−0.28	−0.30	−0.12	0.36
	0.91	*0.41*	*0.70*	*0.21*	*0.23*	*0.41*
Cum prv E spells	0.16	0.07	0.22	0.34**	0.19	−0.22
	1.25	*0.68*	*0.97*	*0.11*	*0.19*	*0.43*
Cumulative previous duration (months)						
Wald test (duration=0) 20.96 (18) 0.281						
Cum prv O duration	0.01	0.04	−0.07	0.01	−0.04	0.00
	0.09	*0.09*	*0.19*	*0.19*	*0.05*	*0.39*
Cum prv U duration	0.02	−0.04	0.02	0.00	−0.04	−0.01
	0.07	*0.06*	*0.10*	*0.03*	*0.04*	*0.06*
Cum prv E duration	0.00	−0.01	0.01	0.00	−0.01	−0.01
	0.17	*0.20*	*0.07*	*0.01*	*0.07*	*0.03*

Continued next page.

Table 2 continued

	O→U	O→E	U→O	U→E	E→O	E→U
Local unemployment rates						
Wald test (ABS all=0) 55.44 (18) 0.000						
Wald test (ABS rate=0) 29.38 (6) 0.000						
Wald test (ABS break=0) 17.31 (12) 0.138						
ABS U-rate (per 10)	0.18	−0.06	−0.24	−0.27**	−0.15	−0.09
	0.42	*0.43*	*0.45*	*0.06*	*0.13*	*0.17*
ABS U-break constant	−0.02	0.20	−0.56	−0.10	−0.07	−0.14
	1.13	*1.31*	*0.67*	*0.39*	*0.48*	*0.23*
ABS U-break slope	0.15	0.27	0.52	−0.11	0.15	0.19
	0.67	*0.32*	*1.56*	*0.13*	*0.64*	*0.51*
Highest qualification obtained (base: year 12)						
Wald test (all=0) 25.21 (12) 0.014						
Year 10	−0.08	−0.38	0.00	−0.57**	0.19	0.62**
	1.09	*1.89*	*0.42*	*0.28*	*0.21*	*0.30*
Year 11	0.16	0.07	−0.33	−0.29	−0.14	0.50
	0.39	*0.61*	*0.39*	*0.40*	*0.31*	*0.41*
Age (base: age 18)						
Wald test (all=0) 36.27 (24) 0.052						
Age ≤ 17	−0.05	0.04	0.12	0.26**	0.15	−0.08
	0.15	*0.47*	*0.32*	*0.11*	*0.15*	*0.13*
Age 19	−0.09	−0.07	−0.21	−0.17	−0.17	−0.10
	0.30	*0.39*	*0.21*	*0.11*	*0.41*	*0.17*
Age 20	−0.53	−0.25	−0.30	−0.06	−0.57**	−0.29
	0.57	*1.31*	*0.81*	*0.20*	*0.26*	*0.20*
Age ≥ 21	−0.15	0.21	−0.92	0.12	−0.42	−0.48*
	1.51	*1.60*	*0.71*	*0.35*	*0.56*	*0.27*

Continued next page.

Table 2 continued

	O→U	O→E	U→O	U→E	E→O	E→U
Female (base: male)						
Wald test (all=0) 9.10 (6) 0.168						
Female	−0.15	−0.31	0.32	0.06	−0.16	−0.51
	0.47	*0.81*	*0.27*	*0.25*	*0.26*	*0.41*
Marital status (base: unpartnered male or female)						
Wald test (all M=0) 6.36 (6) 0.384						
Wald test (all F=0) 24.22 (6) 0.000						
Partnered (M)	0.75	0.02	0.37	0.03	−0.10	−0.53
	1.43	*4.40*	*0.91*	*0.72*	*0.71*	*1.46*
Partnered (F)	−0.65	−0.55	0.62	0.09	0.76	0.03
	1.09	*1.92*	*0.81*	*0.26*	*0.61*	*0.44*
Children (base: male or female not living with children)						
Wald test (all M=0) 10.22 (6) 0.116						
Wald test (all F=0) 57.94 (6) 0.000						
Living with children (M)	−0.03	1.76	0.74	−0.10	0.38	0.33
	1.96	*3.94*	*1.54*	*2.03*	*0.95*	*2.09*
Living with children (F)	−1.72	−2.44	2.42	−0.87	1.24**	−0.28
	1.07	*1.63*	*2.98*	*2.60*	*0.43*	*1.58*
Living arrangement (base: not living with parents)						
Wald test (all=0) 13.51 (6) 0.036						
Living with parents	−0.16	0.11	0.24	0.07	0.00	−0.38**
	0.92	*0.74*	*0.46*	*0.22*	*0.45*	*0.14*
Language background (base: English)						
Wald test (all=0) 4.61 (6) 0.595						
Non-English	−0.09	−0.51	0.05	−0.17	0.32	−0.26
	1.17	*0.56*	*2.46*	*0.56*	*0.47*	*0.30*
Birth country (base: Australia)						
Wald test (all=0) 2.79 (6) 0.834						
Foreign born	−0.36	−0.02	0.28	−0.15	0.43	−0.13
	1.17	*0.44*	*0.44*	*0.89*	*1.56*	*0.25*
Health status (base: no disability)						
Wald test (all=0) 32.26 (6) 0.000						
Disabled	−0.79**	−0.99**	0.50**	−0.30	0.83**	0.31
	0.40	*0.34*	*0.22*	*0.25*	*0.39*	*0.57*

Continued next page.

Table 2 continued

	O→U	O→E	U→O	U→E	E→O	E→U
Type 1	-4.25^{**}	-6.03^{**}	-8.71^{**}	-6.73^{**}	-9.78^{**}	-9.00^{**}
	1.81	*1.20*	*1.72*	*0.89*	*0.94*	*1.55*
Type 2	-3.43	-5.55	-7.89^{**}	-5.64^{**}	-10.35^{**}	-9.98^{**}
	3.82	*6.55*	*3.78*	*1.63*	*1.42*	*2.12*
Type 3	-4.58^{**}	-7.62	-7.05	-6.16	-9.21	-10.47^{**}
	1.65	*9.29*	*12.63*	*4.96*	*12.33*	*2.01*
Probability of type 1	0.37					
	0.76					
Probability of type 2	0.46					
	3.09					
Probability of type 3	0.17					
	3.45					
Log-likelihood value	–21817.35405					
Number of parameters	212					

Legend: O: out of the labor force; U: unemployment; E: employment; F: female; M: male; Cum prv X spells: cumulative number of previous spells in state X; Cum prv X duration: cumulative previous duration of X spells in months; U-rate: unemployment rate; U-break constant: dummy for certain regions of Victoria before September 1992; U-break slope: interaction between U-rate and U-break constant. *Notes:* Parameters (except those relating to the baseline intensities) are approximately the proportional change in the transition intensities per unit change in the corresponding explanatory variable. Cluster-robust standard errors in italics (in particular, the standard errors remain consistent in the case of correlation between multiple spells for the same person). One and two stars indicates statistical significance at the 10% and 5% levels using the asymptotic distribution.

Table 3: Model fit

	Origin state O	U	E	Total
		Data		
Time under observation				
Per cent	10	21	68	100
Incidence rates (exits per year)				
Total	3.4	2.7	0.7	
Destination state				
O	0	0.4	0.2	
U	1.9	0	0.5	
E	1.4	2.3	0	
Duration quantiles (days)				
10%	6	7	18	
50%	36	72	244	
80%	169	229	1873	
		Model fit		
Time under observation				
Per cent	9	22	69	100
Incidence rates (exits per 365 days)				
Total	3.7	2.5	0.6	
Destination state				
O	0	0.4	0.2	
U	2.3	0	0.4	
E	1.4	2.1	0	
Duration quantile (days)				
10%	5	10	26	
50%	41	83	318	
80%	144	244	1677	

Legend: O: out of the labor force; U: unemployed; E: employed. *Notes:* Quantiles based on the Kaplan-Meier product limit estimator. Predictions calculated by simulating 10 histories for each person in the analysis sample.

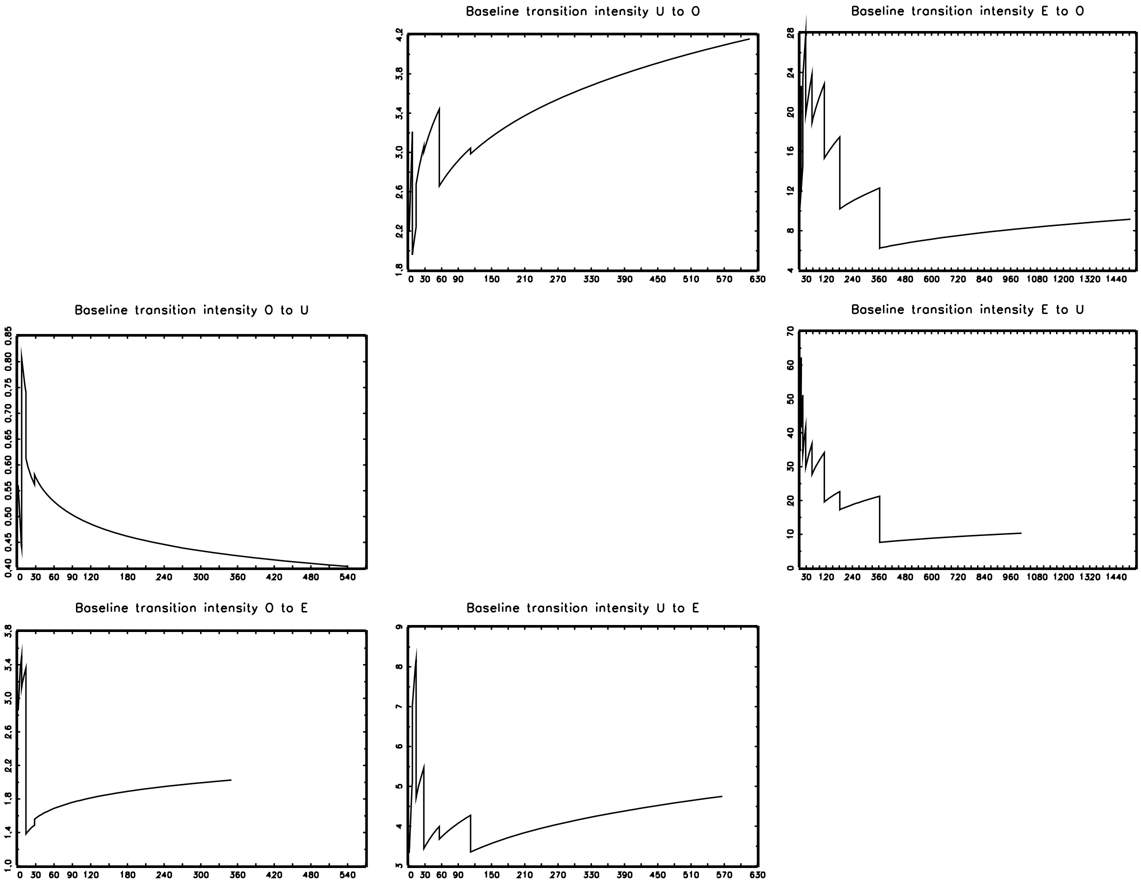

Figure 1: Estimated baseline intensities by type of transition

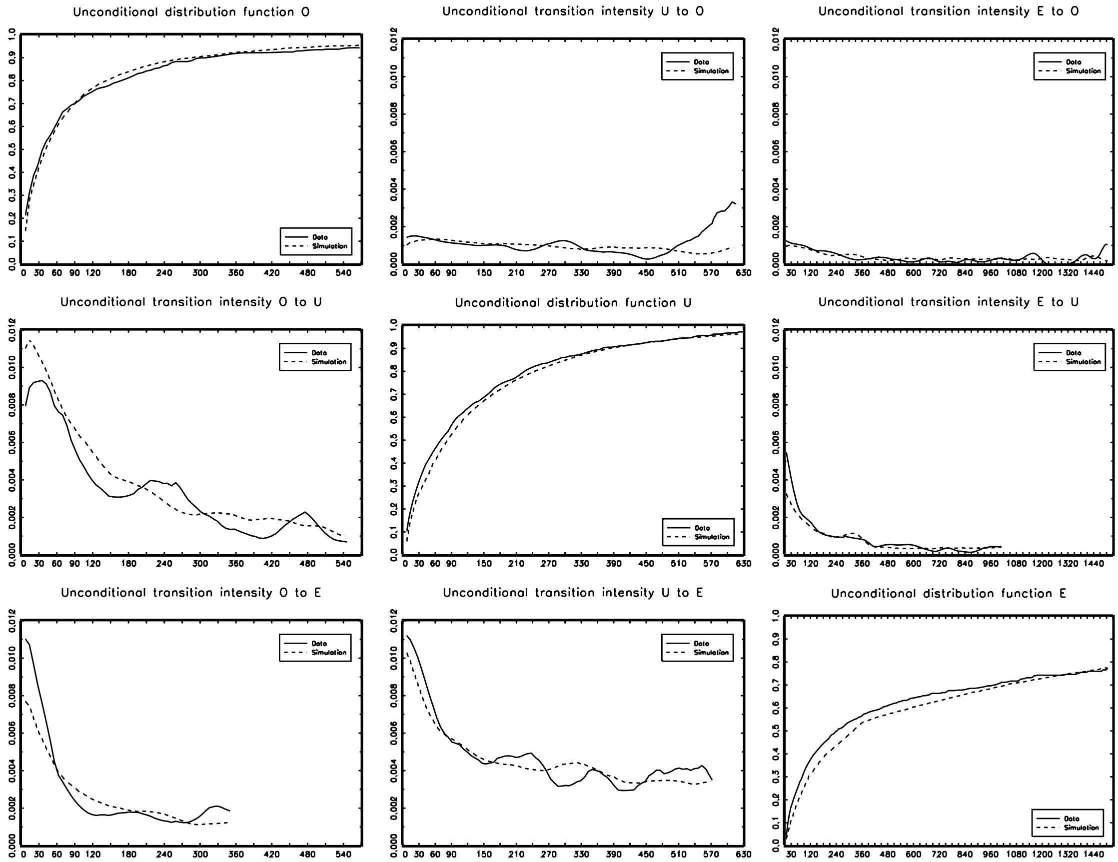

Figure 2: Model fit

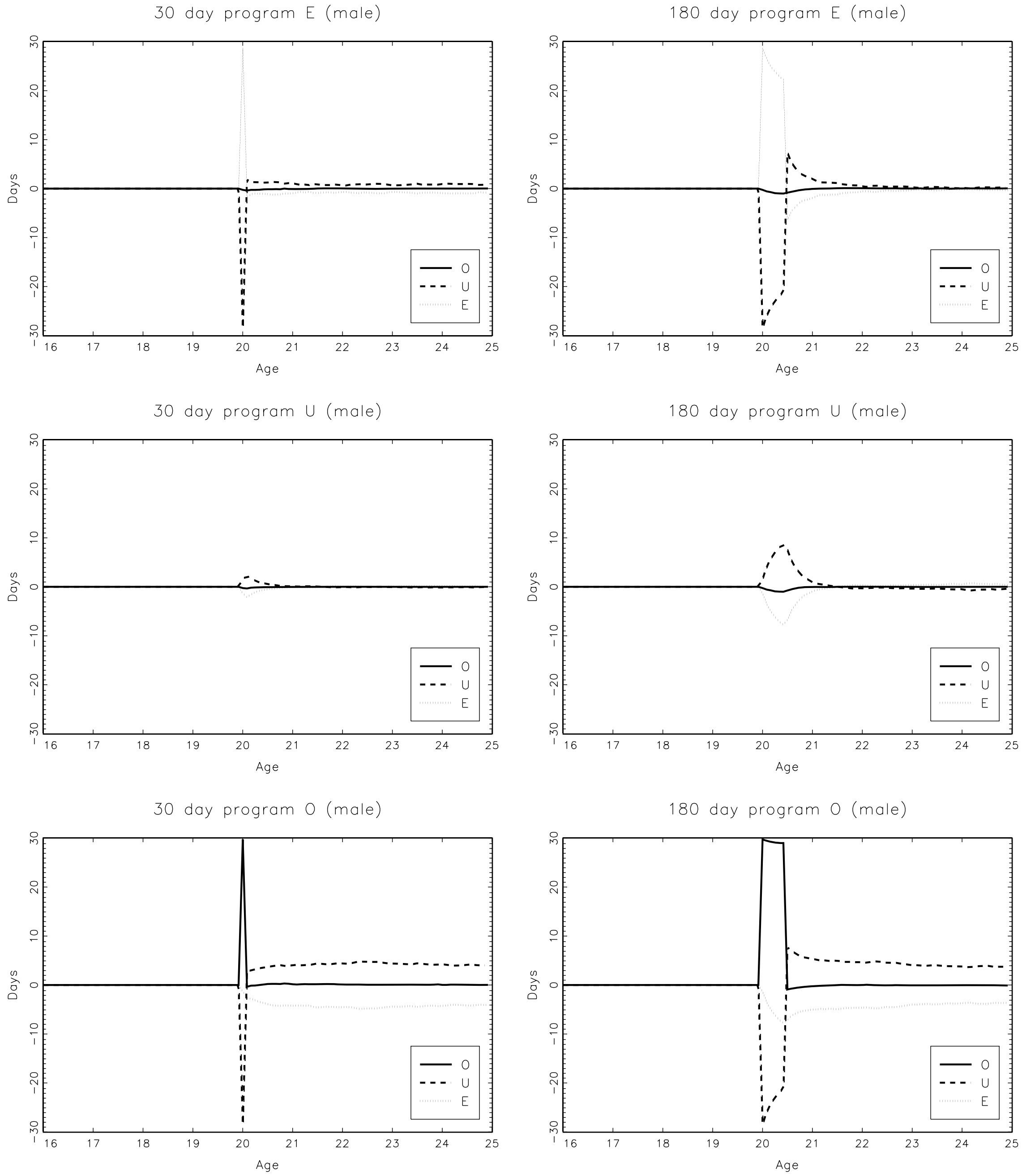

Figure 3: Simulated interventions, joint model for males and females (I)
(Left school after year 10, no further education)

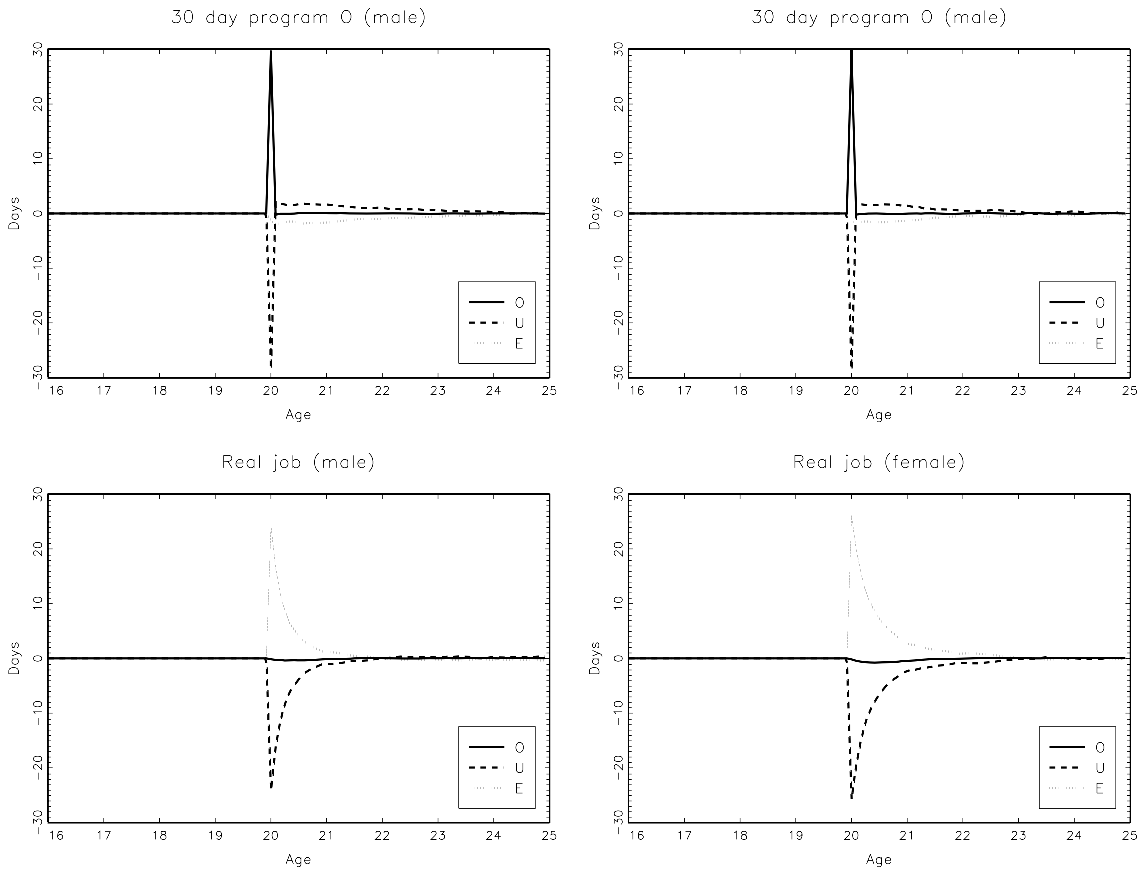

Figure 4: Simulated interventions, joint model for males and females (II)
(Left school after year 10, no further education)

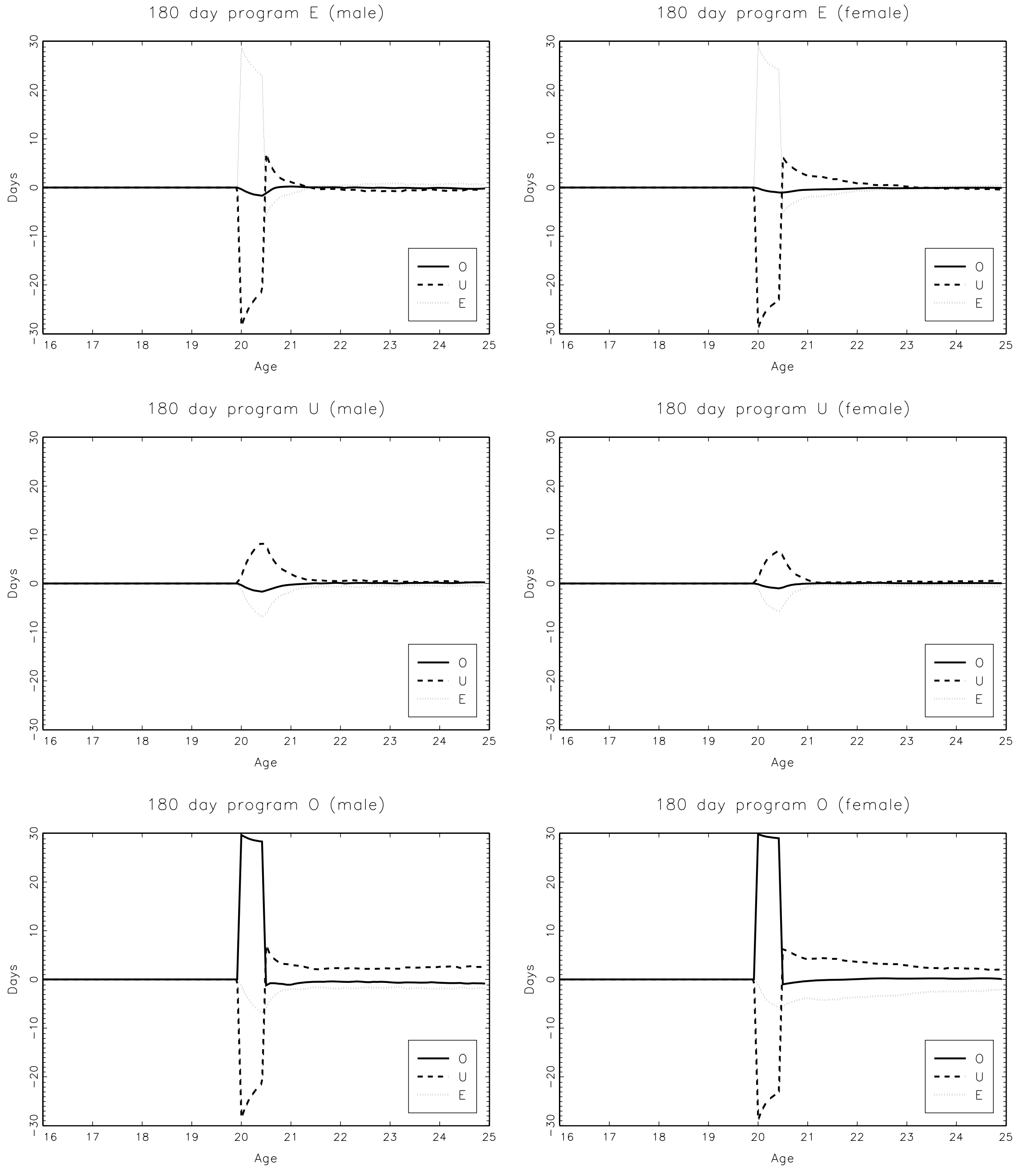

Figure 5: Simulated interventions, separate models for males and females
(Left school after year 10, no further education)

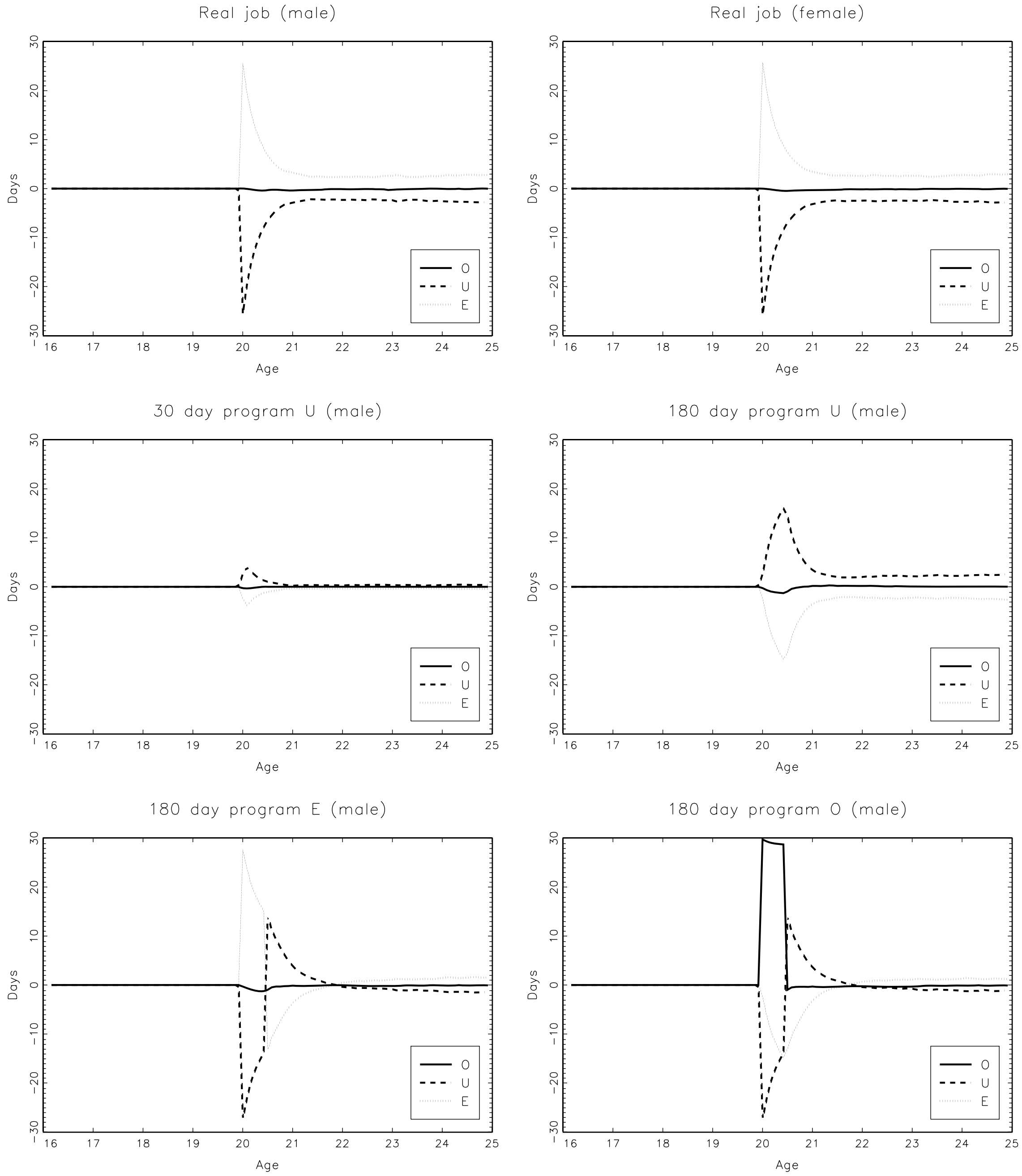

Figure 6: Simulated interventions for people with post-secondary education (Left school after year 12, completed one-year certificate)