

Reducing the Generosity and Increasing the Conditionality of Welfare Benefits for People with Disability: Turning the Supertanker or Squeezing the Balloon?

Barbara Broadway*

Melbourne Institute of Applied Economic and Social Research, University of Melbourne,
Vic 3010, Australia

b.broadway@unimelb.edu.au

&

Duncan McVicar

Queen's Management School, Queen's University Belfast, Belfast BT9 5EE,
United Kingdom

d.mcvicar@qub.ac.uk

This Version: August 2017

JEL: I38, J14

Keywords: welfare reform, disability, propensity score matching

Abstract

This paper uses propensity score matching to examine the impact of a major Australian disability reform on welfare receipt. The reform reduced the generosity and increased the conditionality of welfare payments for people with disability by shifting partially disabled claimants from disability benefits to unemployment benefits. This led to increases in the hazards for switching out of unemployment benefits onto alternative benefits, but also for exiting welfare altogether. Because exiters had a higher probability of subsequently returning to welfare, however, the reform had no impact on the proportion receiving welfare 12 months or 24 months later.

*corresponding author

1. INTRODUCTION

Providing support for individuals with limited earnings capacity on grounds of disability is one of the most important objectives of a modern welfare state. All OECD countries provide some form of disability payment to at least partly replace earnings for those whose work capacity is limited due to disability or long-term health conditions. The dilemma facing policy makers, however, is how to provide adequate out-of-work support for people with disability while maintaining incentives for those who can do so to remain in or re-enter the labour market (see Bound and Burkhauser, 1999; Author and Duggan, 2006). Experience of continued program growth in many countries including the US, despite overall improvements in health, coupled with low employment rates and high poverty rates among people with disability, suggests that policy makers have not yet got this consistently right (e.g. see OECD, 2010).

Among the potential levers to contain program growth which continue to be put forward (e.g. OECD, 2009, 2010; Department of Social Services, 2015) are reductions in payment generosity, more stringent medical screening or re-screening, and introducing activity requirements for continued eligibility. Several studies have found that such reforms impact on program growth in the desired direction (e.g. Gruber, 2000; Staubli, 2011; de Jong et al., 2011), although there are also examples where the program growth impacts of changes to disability benefit characteristics are negligible (e.g. Campolieti, 2004). Among other things, the impacts of such reforms appear to depend on other characteristics of the disability program itself and on the wider welfare system and labour market context (see Burkhauser et al., 2014).

There is also a crucial difference between restricting program growth by displacing people with disability onto other benefits such as sickness or unemployment benefits – squeezing the

balloon – and restricting program growth by retaining (returning) people with disability who can work in (to) stable employment. Existing evidence from studies of disability benefit reforms that have examined this benefit displacement effect – what Borghans et al. (2014) calls ‘social support substitution’ – is mixed. For example, Karlström et al. (2008) (Sweden, tighter eligibility conditions for older workers), Staubli (2011) (Austria, tighter eligibility conditions for older workers) and Borghans et al. (2014) (Netherlands, reduced payment generosity) all find that tightening disability benefits displaces at least some claimants onto other welfare payments. In contrast, de Jong et al. (2011) (Netherlands, tougher screening) finds no such displacement onto unemployment insurance and *positive* spillovers, i.e. reduced applications, for long-term sickness benefits. Autor et al. (2016) (US, relaxation of eligibility criteria for disability payments to armed services veterans) also finds evidence of positive spillovers to Disability Insurance (DI), likely related to the typically long waiting period before DI benefits are awarded in the US.

In this paper we examine the impacts of a 2006 package of reforms introduced in Australia which, by redirecting new disability benefit claimants deemed only partly disabled onto unemployment benefits, combined a reduced level of payments with the introduction of work-related activity requirements. We examine the impact of these reforms on welfare benefit exits (with welfare benefits defined broadly to include any income-replacement benefit payment), but also on switching between welfare payments and on re-entry to welfare benefits for those exiting. In the spirit of Bound (1989) we compare outcomes for applicants who were accepted for disability benefits prior to the reforms with outcomes for matched post-reform applicants who would have been accepted for disability benefits had they applied prior to the reforms but in the event were rejected for disability benefits post-reform and instead granted unemployment benefits. These groups differ in terms of the conditionality

and generosity of the benefits they were awarded upon application but, in contrast to Bound (1989), not in terms of their assessed health or any other observable characteristics.

We show that the reforms led to increased welfare exits among partially disabled welfare recipients but also to increases in switching from payments conditioned on participation in work-related activity to unconditional, passive payments. The reforms also led to increased re-entry to welfare for those having previously exited welfare. Overall the reform had no impact on the probability of being on welfare (i.e. of being in receipt of any income-replacement benefit payment) 12 months or 24 months later. This result echoes the earlier finding of Karlström et al. (2008) that tightening eligibility criteria for disability benefits in Sweden merely shuffled people between welfare payments and had no overall impact on welfare dependency. The reform studied here, however, covers not just 60-64 year olds – who we might imagine would be harder to re-integrate into employment than younger workers – but workers of all ages. The lack of *any* positive reform impact on welfare dependency is also starker than the partial benefit displacement findings of Staubli (2011) and Borghans et al. (2014), who find some shift into employment alongside displacement onto other welfare payments. An additional contribution is that this is the first evaluation of this major package of Australian disability reforms to estimate impacts against a defined counterfactual, complementing an earlier ‘before and after’ evaluation carried out in-house by the relevant government department (DEEWR, 2008) and a more recent descriptive study of reform impacts on the time path of the disability benefit roll in Australia (McVicar and Wilkins, 2013).

The rest of the paper is set out as follows. Section 2 describes the pre and post-reform benefit system for people with disability in Australia. Section 3 describes our administrative data covering the relevant population of benefit recipients pre and post-reform and describes the matching of accepted disability benefit recipients with those rejected for disability benefits

but receiving unemployment benefits. Section 4 sets out our approach to estimation using this matched sample. Section 5 presents and discusses our results and Section 6 concludes.

2. INSTITUTIONAL BACKGROUND AND THE 2006 WELFARE TO WORK REFORM

Over the period of interest here, both pre and post-reform, the Australian welfare system offered support for those with insufficient income due to disability primarily via one of two means-tested Income Support (IS) payments: the ‘Disability Support Pension’ (DSP) (the nearest Australian equivalent to US DI) and ‘New Start Allowance’ (NSA) (unemployment benefit). The key differences between the two payments were first that DSP was paid at a higher rate than NSA, and second that eligibility for NSA typically required ongoing engagement in job search / job preparation activity, although NSA claimants with limited work capacity on the grounds of disability could be granted temporary exemptions from some of these activity requirements. Other differences are summarised in Table 1 and explained in more detail below.

Table 1 around here

In practice an individual who experienced physical or mental impairments that might make him or her eligible for DSP would submit an application, with supporting evidence from their doctor, to Centrelink, the government agency that is responsible for processing all IS applications. Most individuals making a new claim for DSP were initially placed on NSA (assuming they met the eligibility requirements for NSA), perhaps with temporary exemption from activity requirements, pending an assessment of their work capacity known as a Job Capacity Assessment (JCA).¹ JCAs were conducted by trained and government-employed

¹ Immediate access to DSP was granted to those deemed by Centrelink assessors to have a manifest disability at the time of application, e.g. cases of terminal illness or blindness, subject to meeting the other eligibility requirements. A JCA was not conducted in such cases.

health professionals, usually face-to-face, on average a few months after the initial claim. The JCA then determined whether the claim for DSP was successful or not, with DSP payments backdated for successful applicants.² Those deemed ineligible for DSP as a result of a JCA could remain on NSA, with the outcome of the JCA informing exemption status for activity requirements. There have been further reforms since 2006, outside the period we study here, but the characteristics of these payments and the DSP application process remain largely unchanged today.

2.1. Eligibility for DSP Pre-reform

JCAs assessed both the level of impairment and the work capacity based on those impairments. For DSP purposes only permanent conditions were taken into account.³ The health assessor first determined the lack of functional capacity in various physical or mental functions, resulting from diagnosed conditions, and assigned a point-rating for each function's impairment according to the corresponding 'Impairment Table' defining a number of 'impairment points' for given levels of impairments per function. A total of twenty or more 'impairment points' across all Impairment Tables was a necessary, but not sufficient, condition for eligibility for DSP. The assessor then determined the maximum number of weekly working hours the individual could be expected to perform, given the level of impairment and any available support. To that end, any barriers to finding or maintaining employment, as well as any interventions that might assist in improving the individual's future work capacity, were taken into account. In addition to the minimum of twenty impairment points, eligibility for DSP required the individual's working capacity after interventions and assistance to be less than 30 hours per week for at least the next two years.

² Note the average time to decision on DSP eligibility is considerably shorter than the equivalent for DI in the US (see Autor et al., 2015).

³ A permanent condition in the sense of the law is 'fully diagnosed, treated and stabilised'. Essentially this is a requirement that the condition leading to the impairment is expected to last for more than two years, and any significant functional improvement is unlikely within that timescale, with or without medical treatment.

If both criteria were met the individual was considered to have a ‘continuing inability to work’ and was therefore eligible for DSP.

If one or other criteria was *not* met the individual was not deemed eligible for DSP but may have qualified for NSA. In this case the outcome of the JCA would be used to inform activity requirements, which could be reduced so that they did not exceed the recipient’s assessed weekly work capacity.⁴

2.2. Eligibility for DSP Post-reform

Post-reform – i.e. for DSP claims made on or after 1 July 2006 – eligibility for DSP still required a total of twenty or more impairment points across all Impairment Tables. However, in order for an individual to be eligible for DSP, their weekly working capacity was no longer allowed to exceed 15 hours per week (instead of the previous cut-off of 30 hours). In other words those DSP applicants with 20+ impairment points and a working capacity of between 15 and 29 hours per week were no longer eligible for DSP, but instead may have qualified for NSA with reduced activity requirements.

Note that claims made before or on 10 May 2005 were fully grandfathered, i.e. assessed under the old 30-hours-rule, and not affected by future reforms. Claims made between 11 May 2005 and 30 June 2006 were first assessed under the old rules but payments were granted only temporarily until a re-assessment – a new JCA – under the *new* hours rule, which was expected to take place about two years after their initial JCA (but in many cases took place considerably later). Claims made after 1 July 2006 were assessed under the new rules from the day of application.

⁴ Note for NSA recipients the assessment of a ‘partial capacity to work’ could additionally take account of health conditions that did not meet the expected two-year duration restriction. If future work capacity was expected to improve this did not affect a person’s *current* eligibility for exemptions from activity requirements.

2.3. Disability-related Welfare Payments

Once a person's qualification for either DSP or NSA with reduced activity requirements had been assessed, their income (and, if applicable, also spousal income) determined the level of payment. In 2006, the maximum rate of DSP paid to a single person was A\$256.34 per week, and the maximum rate paid to a member of a married couple was A\$211.85. This payment was equivalent to 48% or 40% of the minimum wage for a full-time employed adult at the time. NSA payment rates were (and still are) lower than DSP payment rates: twenty per cent lower for singles (A\$205.30 per week) and twelve per cent lower for a member of a married couple (A\$185.25). For both payment types, only low additional earnings were permissible, and earnings above the income threshold reduced the payment. NSA tapered off at a faster rate than DSP, in particular in partner income.⁵ Relative payments rates for DSP and NSA, means-testing and taper rates remained essentially unchanged during the reform process. However, the change in the hours rule meant that applicants with a working capacity of 15 to 29 hours per week received substantially lower payments at higher activity requirements than would have been the case had they applied prior to the reform

3. DATA AND MATCHING

The primary data source used here is the *Research and Evaluation Database* (RED), which contains detailed administrative records for the full population of welfare recipients in Australia. For each individual who has ever received any form of welfare over the period of study (and long before and after) the exact history of welfare spells with beginning and end of the spell as well as payment type and amount is recorded on a daily basis. The records were

⁵ For DSP, earnings beyond A\$64 (A\$114) per week for singles (couples) reduced the total payment by 50 cents (25 cents) per dollar. NSA was reduced by 50 cents per dollar for earnings in excess of A\$62 and by 60 cents per dollar of earnings in excess of A\$250; if the applicant's partner has earnings in excess of the amount beyond which NSA would not be payable to the partner, payments to the NSA recipient were reduced by 60 cent per dollar of partner's earnings beyond this threshold.

extracted – and thus the maximum period of observation ends – as of 30 June 2012. Additional information recorded in the RED includes gender, age, number of children, the results of the JCA, the type of their primary health impairment in broad categories (such as psychological impairment or muscular/skeletal impairment), and the number of ‘impairment points’ (greater than / less than 20). Temporary suspensions of payments are also recorded.

Because RED is not a random sample but contains the *full population* of interest, the number of available observations is very high, which allows us to restrict the analysis to DSP applicants assessed as having 20+ impairment points and a work capacity of 15-29 hours per week, in a relatively small time-window either side of the reform. There were a total of 9,162 such applications for DSP between 1 July 2005 and 30 June 2007 that resulted in a spell of DSP or NSA receipt. Of those, we exclude 1,920 individuals where information regarding the date, result of their assessment, the type of payment they receive after their assessment, or the regulations under which they were assessed does not allow us to unambiguously identify whether they are subject to the regulations of the before- or after-period.⁶ Of the remaining 7,242 applications, 3,019 were lodged after the change in regulations (i.e. between 1 July 2006 and 30 June 2007), and 4,223 before (i.e. between 1 July 2005 and 30 June 2006).

A disadvantage of the RED data, as with other similar administrative data sets, is a lack of universal information on socioeconomic characteristics. But we are able to augment the RED data with linked information from the *Job Seeker Classification Instrument* (JSCI). This is an interview that is typically conducted at the time when an individual first claims a benefit, or registers with the administering office (Centrelink), and is used to classify individuals according to their distance to the labour market. The JSCI contains predominantly self-reported

⁶ The data contain a flag indicating whether an individual was assessed under the transition rules or under the rules in place after full implementation of the 2006 reform, the date of lodgement and assessment of their claim, the result of their assessments and the type of payment they receive. In some cases, those entries do not yield a consistent picture, and are therefore omitted.

information on an individual's education, language skills, and their recent work experience. The postcode of the individual's residential address is used to provide some additional information such as the remoteness of the area of residence. The information from a JSCI interview is not updated unless any changes in an individual's life circumstances warrant a change in their service arrangements. Some changes in time-varying characteristics will thus not be picked up. We treat information on highest education level and English language proficiency as time-invariant until there is an update by a new interview. Geographic location and recent work experience, on the other hand, are used only if the last JSCI interview was conducted no more than one year ago, and are treated as 'missing' otherwise. We drop observations for which no JSCI information is available, leaving a total sample of 5,921 observations.

Table 2 reports mean characteristics of applicants who applied for DSP before (column 1) and after (column 3) the reform, at the time of their application. The two groups are broadly similar but there are some differences. For example, applicants after the reform are more likely than pre-reform applicants to be aged between 30 and 59 years, to be married or divorced rather than single, and have somewhat lower education levels. A higher proportion post-reform also report poor English language proficiency. Simply comparing outcomes for these two groups – a straight 'before and after' approach – will therefore confound impacts of the disability reforms with these and any other pertinent differences between the two groups.

To wash out these differences we therefore match those who apply for DSP after the reforms with DSP applicants who are observationally most similar prior to the reforms. Specifically, we apply a matching procedure which combines exact matching on age group with propensity

score kernel-matching on all other characteristics in Table 2.⁷ All observations are first separated by age category. Within each age category, we then estimate the propensity for an observation to be an applicant after the reform. Figure 1 shows the distribution of propensity scores aggregated over all age groups. The two distributions of propensity scores show common support for treated and control observations, and are very similar in both groups. They are nearly centred, with similar means and comparable spread. Only at the very top end of propensity scores is the support for treated observations somewhat slim. Finally, we match each applicant after the reform (the treatment group) with applicants within the same age category before the reform (column 2 in Table 2) (the control group) using kernel-matching. This results in a sample of 5,921 observations with a total weight of 5,623, who are well balanced in all characteristics.

Figure 1 around here

The last two columns of Table 2 show measures of balanced-ness for the samples after matching: the standardised bias and the p-value of a means-comparison test. The absolute values of the standardised differences are very low, ranging from <0.1% to a maximum of 5.4% and thus well below the common threshold for balanced samples of 25%. There are no statistically significant differences in means. All remaining analysis is conducted using this matched sample. This approach identifies reform impacts separately from compositional differences between post-reform and pre-reform applicants under a standard conditional independence assumption or CIA, i.e. under the assumption that there are no relevant *unobserved* differences between the two groups (see Rosenbaum and Rubin, 1983). We assess the reasonableness of this assumption in the following section.

⁷ We have also applied radius-caliper matching and nearest-neighbour-matching with various numbers of matching partners, all with similar results. Kernel matching, however, provided the best matching quality in terms of balancing the sample and using as many different individuals as possible. The results of other matching procedures are available from the authors on request.

Table 2 around here

4. APPROACH TO ESTIMATION

We first estimate the reform's impact on the duration until an individual exits welfare receipt using a continuous-time Cox Proportional-Hazard model (see Van den Berg, 2001). We define an individual as having left welfare if they were not eligible for *any* income-replacement payment for at least one day; mere suspension of payments is not defined as an exit from welfare. The resulting hazard rate for welfare exit is given by:

$$(1) \quad h_i(t) = h_0(t) \cdot \exp(R_i\delta_c + R_it\delta_t),$$

where $i = 1, \dots, N$ is the set of all individuals in our matched sample and t is the time that has elapsed since the beginning of the spell (measured in days and censored at two years). The dummy variable R_i indicates whether an individual belongs to the treatment group, i.e. it takes the value 1 for claimants who applied between 1 July 2006 and 30 June 2007, and the value 0 for claimants who applied between 1 July 2005 and 30 June 2006. The interaction between R_i and t allows the estimated effect of the reform to vary over time, and the estimated impact on the hazard rate for leaving welfare is given by δ_c and δ_t . Finally, $h_0(t)$ denotes the baseline hazard, i.e. the underlying relationship between elapsed duration and the hazard rate. Essentially this model allows the reform to shift the underlying relationship between exit probability and elapsed duration – the baseline hazard – up or down to varying degrees depending on elapsed duration.

4.1. Validity of the Estimator

The key issue here is the extent to which treatment status (being subject to the new hours rule) can be treated as exogenous. In other words, how reasonable is the (untestable) CIA? There are reasons to be confident in its plausibility here. First, we are able to match individuals in the

treatment and control groups on an extensive set of observed characteristics and, as shown in Table 2, the matching performs very well. Second, in the spirit of Bound (1989) the analysis is based on comparing outcomes for two groups of DSP applicants; those successfully applying pre-reform and those applying post-reform who were denied DSP and instead granted NSA, but who would likely have been successful had they applied pre-reform. As Bound (1989), Chen and van der Klaauw (2008), von Wachter et al. (2011) and Giertz and Kubik (2011) all argue, rejected and successful applicants are likely to be much more similar to each other than they are to the general population, both in observed and unobserved factors, thus reducing any potential bias arising from selection on unobservables. The argument is stronger where acceptance or rejection for DSP is driven solely by whether the application was before or after an arbitrary cut-off date, not by whether the applicant was assessed as being above or below a health-related threshold. Using variation in acceptance criteria over time (as in Gruber, 2000; Campolieti, 2004; Mitra, 2009; de Jong et al., 2011, Staubli, 2011), *in conjunction* with a sample that consists of successful or rejected DSP applicants only, means that many of the potential selection issues that might otherwise confound our estimated treatment effects are absent.

Nevertheless, we consider three remaining issues that could limit the acceptability of the CIA here, and that could therefore cause our estimated reform impacts to be biased. First, there may have been differences in labour market context pre and post-reform, or other unobserved time-related differences, that could impact on outcomes. Second, the timing of the DSP application, or whether a DSP application is made at all, may be endogenous. Third, once a DSP applicant was assessed as having a work capacity of <30 hours post-reform, non-permanent health conditions could then also be taken into account in classifying work capacity as being in the 15-29 hours category or the 0-14 hours category. This was not the case pre-reform. In each case, however, we can either sign the bias or rule out substantial biases using robustness checks.

First, consider time effects. In particular, the latest applicants included in the analysis – those who applied in June 2007 – faced a labour market impacted by the global financial crisis (GFC) just over a year after their application. In contrast, the two year time window for the earliest applicants (July 2005) ends well before the GFC. If the GFC affected an individual's chances of exiting welfare, any stimulating effect the reform might have had could be partly offset by general labour market conditions, at least in part, and our estimates would be biased downwards. Note, however, that the labour market and welfare dependency effects of the GFC were comparatively modest in Australia, which did not experience a recession (Kuehnle and Scutella, 2011). Nevertheless, we apply two robustness checks. One is to repeat the estimation restricted to a twelve-month window of observation after application, which means the last month of observation is July 2008, largely preceding any Australian labour market impact of the GFC. The other, in order to wash out general time effects and thus relax the CIA, is to extend the matched sample to include applicants who were assessed with a working capacity of 8–14 hours per week. These applicants received DSP regardless of their application date, and can thus serve as a control group in a difference-in-differences (DID) extension of our main PSM approach.⁸

Second, the timing of one's DSP application might be endogenous. While some health impairments feature a sudden onset (for example those caused by work accidents), others may result from a chronic, slow deterioration of an individual's health. In the latter case, or where a long-running health impairment has not previously led to a DSP claim, there is potential to influence the exact date of application. If so, those who apply just before 30 June 2006 might differ from those who apply just after this cut-off date in the type or longevity of their health impairments as well as in their unobserved characteristics. In particular, it seems reasonable to assume those who apply just after the cut-off date are a positive selection of all applicants in

⁸ This approach was originally proposed by Heckman et al. (1997).

terms of their probability of becoming independent from welfare. If this is the case, any potential activating effect of reduced payments and increased activity requirements would be exaggerated, meaning that our estimated reform effect is biased upwards. We therefore test whether our results are robust to the exclusion of applicants who applied one or three months before or after the cut-off-date. We cannot entirely rule out a possible counteracting (i.e. downwards) bias if there are individuals who do not apply for DSP post-reform – and therefore do not show up in the data – because of the tighter eligibility criteria, but who would have done so had they experienced disability onset pre-reform. The magnitude of any such bias is likely to be small, however, because such behaviour would require potential applicants to know about the reform in advance of application and to have an expectation that they would be assessed as having both 20+ impairment points and a work-capacity of 15-29 hours per week.

Third, for individuals classified as having a work capacity of 15-29 hours per week based on their permanent health conditions, non-permanent health conditions could then be taken into account for classifying work capacity post-reform (for NSA activity-exemption purposes) but not pre-reform (for DSP purposes). In particular, it is possible that an individual would be assessed as having a work capacity of 15 to 29 hours per week for DSP purposes pre-reform, but of 8 to 14 hours per week for NSA purposes post-reform. Pre-reform this individual would be in our sample but post-reform they would not. Similarly, it is possible that an individual is assessed as having a work capacity of 30+ hours for DSP purposes but 15-29 hours for NSA purposes. As a result, the composition of applicants included in the analysis in terms of the relative types and longevity of health impairments may differ before and after the reform: applicants before the reform might have on average more long-lasting and severe impairments. In that case, any activating effect of the reform effect would be overstated by our estimates. We investigate sensitivity by re-estimating with a sub-group of individuals who do not have a

non-permanent recorded health condition, and who would therefore be expected to be classified into the same work capacity bandwidth (WCB) for both DSP and NSA purposes.

5. RESULTS AND DISCUSSION

We first estimate the hazard rate for leaving welfare (IS), whether directly from one's current payment or indirectly via another type of payment. The left hand columns of Table 3 show the coefficients and associated standard errors, and the right column the exponentiated coefficients of the model, i.e. the multiplicative factor by which an increase in a given characteristic increases the hazard rate for leaving welfare at any given point in time (the hazard ratio). Applicants who lodged their claims after 1 July 2006 have a higher chance of leaving welfare afterwards; the effect is positive and significant at the 0.1 per cent level. Specifically, the reform increases an IS recipients' hazard rate for exiting welfare by 90 per cent (i.e. the hazard almost doubles) on the first day of the spell. The longer the benefit spell lasts, however, the smaller the positive effect of the reform on the hazard becomes.

Table 3 around here

A better sense of the economic significance of this effect is given by Table 4, which shows how the difference in the hazard rate for leaving welfare across groups accumulates over time (the cumulative hazard), i.e. it shows the estimated impact of the reform on the proportion of applicants who have exited IS at various duration points. The increase in the hazard rate for leaving welfare described above translates into the reform increasing a recipient's chance of leaving welfare within six, 12, or 24 months after commencing a spell of disability-related payments by 4.6, 8.7 and 11.5 percentage points, respectively. That is, the probability of leaving welfare within one year (two years) is increased by about 60 per cent (more than 40 per cent) because of the reform for those with a weekly working capacity between 15 and 29 hours. This is a big effect.

Table 4 around here

5.1. Staying off Welfare

Table 5 shows the estimated probability that DSP applicants are still in receipt of welfare six months, 12 months and 24 months after their claim. A claimant can still be on welfare either because they have not yet exited or because they have exited and subsequently returned to welfare. Applicants after 1 July 2006 were slightly less likely to receive welfare six months after the first day of their original spell than pre-reform applicants. In other words the reform slightly reduced the probability of relying on welfare six months after the original spell began, albeit only by two percentage points. After that the effect disappears, falling in magnitude and losing statistical significance at 12 months and reversing in sign, although still statistically insignificant, at 24 months. So, although the 2006 reform appears to encourage *exits* from welfare initially (Tables 3 and 4), it seems to have no effect on the overall probability of receiving welfare one to two years later once re-entry to welfare is taken into account. This is a stark result. It is consistent with Karlström et al.'s finding of no welfare dependency impact of Swedish disability reforms, but arguably starker given the reform we examine here is not limited to older workers.

Table 5 around here

To explore this further we analyse the return behaviour of the sub-sample of 1,817 individuals who exit welfare for at least one day within two years of the commencement of their DSP or NSA spell. For this sub-sample, we model the duration until return to welfare, regardless of payment type, within one year of originally leaving welfare. Results are presented in Table 6.

Table 6 around here

The left hand columns of Table 6 show the resulting estimated hazard rate coefficients and associated standard errors for re-entry to welfare and how it is affected by the reform. The right hand column gives the estimated hazard ratios as before. The estimates suggest that the reform increases the risk of re-entry to nearly three times the original return rate on the first day after having left welfare, as indicated by the hazard ratio of 2.76 for applicants after the reform. The longer the individual stays off welfare, however, the less the reform increases their risk of returning.

As before, a better sense of the economic significance of this effect is given by the impact on the cumulative hazard, as shown in Table 7. Again it is large. While prior to the reform only 38 per cent of those with the relevant WCB who left welfare returned within one year, this is true for 69 per cent of the same group after the reform. Before the 2006 reform 26 per cent of welfare recipients in our sample left welfare within two years (see Table 4), and 63 per cent of those welfare-exiters did *not* return within another year (see Table 7). This leaves about 16 per cent of the original group of DSP applicants who leave welfare and remain independent from welfare payments a year later. After the reform, 37 per cent left welfare, but only 31 per cent of these welfare-exiters do not return within a year. Post-reform, 12 per cent of the original group of DSP applicants have left welfare and remain independent from welfare one year later. In sum, the proportion of DSP applicants awarded DSP or NSA who exit and then remain off welfare for any prolonged period of time is at best unchanged by the reform.

Table 7 around here

As well as increasing churn off and on welfare did the reform induce switching directly from one payment type to another? Differences in conditionality and payment levels between DSP and NSA provide an incentive to do so. An individual might never leave welfare but might end their current (NSA) benefit spell and begin a new spell on an alternative (DSP or other passive)

payment the very next day, as long as they meet the eligibility requirements. In other words, in addition to the initial displacement from disability to unemployment benefits (the direct result of the reform) there may be a subsequent displacement from unemployment benefits back onto disability or other passive benefits.

To explore this we estimate the duration until an individual experiences a change in benefit type without having left welfare previously as a competing risks model, where if an individual leaves welfare they are no longer at risk of switching benefit types and are thus treated as censored in the estimation. Table 8 shows the impact of the 2006 reform on the hazard rate of ending receipt of one's current type and switching to another. The results clearly show a significant reform impact: the corresponding hazard rate increases by a factor of more than 1.4 on the first day of receiving a disability-related welfare payment, and increases further with the duration of the benefit spell. Many claimants who at first are deemed ineligible as a result of the reform appear to find their way onto their originally intended benefit, or other benefits, in time. This is not unique to Australia (see, for example, Autor and Duggan, 2006).

Table 8 around here

To illustrate how this generates large cumulative effects Table 9 shows the probability of having switched benefit types by elapsed duration since commencement of the original disability-related welfare spell. After two years, one in three recipients in the treatment group have switched benefit types, compared to only four per cent of those in the control group.

Table 9 around here

Table 10 cross-tabulates the benefit type of the original spell with that of the subsequent welfare spell for the 1,008 individuals in our matched sample who switched benefit types within two years of commencing their original welfare spell. First, more than four in five of all benefit

switchers received NSA in their original spell, i.e. were applicants after the reform. Second, while nearly all DSP recipients who switch benefit types go on to subsequently receive an age pension or widow's pension, the vast majority of NSA recipients goes on to receive DSP, i.e. they do switch back to their originally intended benefit. Among other things this requires a re-assessment of their WCB with the result that their WCB is assessed to be below 15 hours/week. Rather than increasing independence from welfare as originally intended, a major impact of the reform was to trigger re-assessments that resulted in lower assessed WCB than previously.

Table 10 around here

5.2. Robustness Checks

As discussed in section 4 there are some potential threats to our estimator's validity that we cannot rule out *ex ante*, including possible endogeneity in the timing of the original DSP claim, the possibility of unobserved differences in health conditions between the treatment and control groups, and the possibility that the onset of the GFC differentially altered the labour market conditions facing post-reform claimants. We test the impact of the first problem by excluding welfare recipients who lodged their application one or three months before and after 1 July 2006, and the second by limiting the estimation sample to individuals who have *only* conditions that are considered permanent, i.e. whose assessment for DSP and NSA purposes would rely on the same set of conditions. We use two robustness checks to assess the third issue. First, we censor every welfare recipient in the estimation sample after 365 days, which means that the latest applicant in the sample on 30 June 2007 is censored on 30 June 2008 and thus before the GFC began to impact on the Australian labour market. Second, we extend the analysis to applicants who were assessed as having a working capacity of 8 to 14 hours per week and then include a dummy-variable that indicates the assessed WCB, a dummy variable that indicates whether the application was lodged before or after the reform, and an interaction between the

two. The last now estimates the reform-effect in a PSM-DID framework, which allows us to wash out time effects such as differences in labour market conditions pre- and post-reform.⁹

Table 11 shows, for different sample restrictions and specifications, the cumulated probability of having left welfare after six months, 12 months or 24 months. The corresponding baseline estimates were shown in Table 4. The table shows that the total reform effect is both qualitatively and quantitatively robust to these sensitivity analyses, suggesting (net) biases due to unobserved time effects, unobserved differences in health relating to differential assessment criteria for DSP and NSA, and endogenous timing of initial claims, are small.

Table 11 around here

We also present estimates for an alternative competing risks model in Table 11. We have already shown reform impacts on exiting welfare and on switching welfare payments. These are not technically competing risks in the sense that the event ‘switching to a different payment type’ does not prevent the event ‘leaving welfare receipt’ from happening. However, once an individual draws another benefit type, any subsequent full exit from welfare receipt will no longer be directly influenced by the reform, and is thus of less interest for our purposes. If, instead of estimating the hazard for leaving welfare regardless of whether it is a direct exit or an indirect exit via another payment as in Table 3, we estimate the hazard for exiting welfare from your current payment in the framework of a competing risks hazard model, the reform’s estimated effect on the probability of leaving welfare remains essentially unchanged.

⁹ The estimated hazard rate for the difference-in-difference estimation is $\lambda_i(t) = \lambda_0(t) \cdot \exp(T_i\gamma_1 + A_i\gamma_2 + T_iA_i\delta_c + T_iA_it\delta_t)$, where T_i indicates the treatment group and takes the value one for individuals with an assessed working capacity of 15 to 29 hours per week, and zero for all individuals with working capacity 8 to 14 hours per week. The dummy variable A_i indicates whether an observation belongs to the period after the new regulations were introduced and takes the value one for claimants who applied between 1 July 2006 and 30 June 2007, and the value zero for claimants who applied between 1 July 2005 and 30 June 2006.

We perform similar robustness checks for the estimates presented in Table 5, shown in Table 12. Our baseline estimate is that an individual is two percentage points less likely to be in receipt of welfare six months after their original spell began if they are subject to the reform. This result remains virtually unchanged if we exclude individuals who applied close to the cut-off date; it increases somewhat if we exclude those with non-permanent conditions, and decreases in the PSM-DID estimation, but loses statistical significance either way. Again any of the biases targeted by these sensitivity analyses appear small. Reform impacts essentially disappear within a year whether we include or exclude applications close to the cut-off date and whether we include or exclude individuals with non-permanent conditions (there is a marginally significant effect at 12 months in this one case), or use individuals with a lower assessed working-capacity as a DID control group. After two years all point estimates are positive, and some are statistically significant. In other words the suggestion is that the reform either has no impact on the probability of being on welfare two years after the start of the initial spell or may even *increase* it. There is certainly nothing here that leads us to question the conclusion that the 2006 reform did not lead to reduced welfare dependency among people with disability.

One remaining potential bias which we do not have a robustness test for might arise if there are individuals who do not apply for DSP post-reform – and therefore do not show up in the data – but would have done so had they experienced disability onset pre-reform. The most likely sign of any such bias on the probability of exiting welfare is negative, assuming it would be the most marginal applicants who are dissuaded from applying for DSP. Yet we still see a clear positive impact of the reform on welfare exit, whether in the baseline case or any of the sensitivity analyses. The implication is that the zero effect on welfare dependency after one or two years shown in Table 5 and Table 12 cannot be driven solely by a selection effect of this kind.

6. CONCLUDING REMARKS

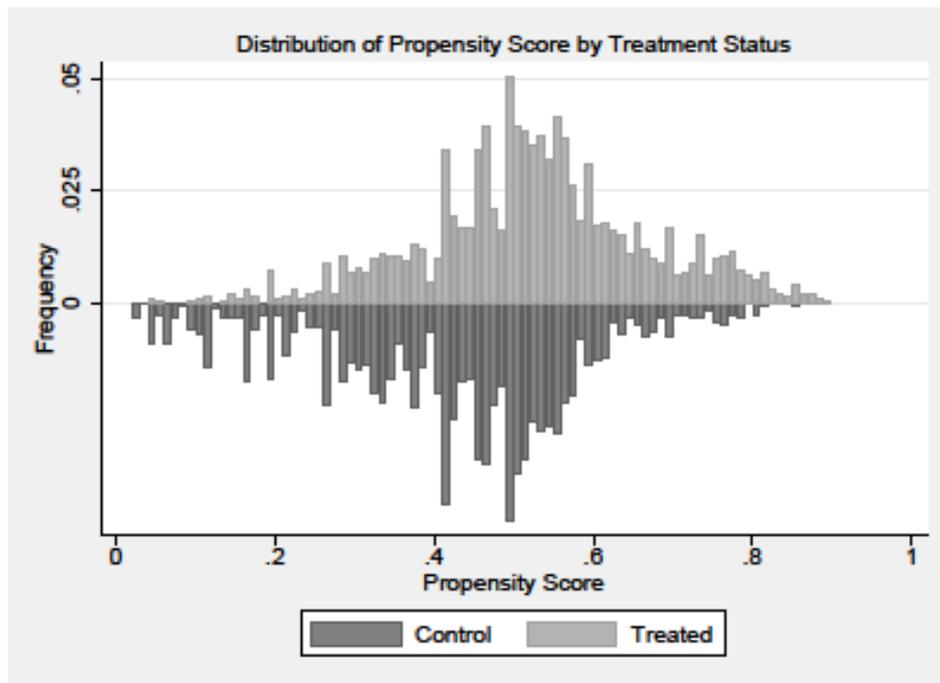
This paper examines a major 2006 reform to Australian welfare benefits for people with disability. The reform both reduced the generosity and increased the conditionality of welfare payments by shifting partially disabled disability benefit claimants from disability benefits to unemployment benefits. This led to increases among partially disabled welfare recipients in the hazards for exiting welfare and for switching (back) from unemployment to disability or other passive benefits. It also led to an increase in the hazard for returning to welfare for those having previously exited welfare. Overall the reform had no impact on the probability of being on welfare 12 months or 24 months later.

So there is little evidence of ‘turning the supertanker’ here, but there is evidence of ‘squeezing the balloon’ consistent with that found by Karlström et al. (2008), Staubli (2011) and Borghans et al. (2014) for related reforms in Sweden, Austria and the Netherlands respectively. Of these studies, our conclusion is closest to that of Karlström et al. (2008), which also found no impact on welfare dependency after two years, albeit for a reform specifically targeted at older workers. . Like this earlier paper we cannot be sure what happens beyond the study period, and it is possible that the reforms do subsequently lead to a reduction in welfare dependency. Given the estimated impact on escaping welfare dependency is monotonically decreasing over the first two years, however, this seems unlikely.

Although our main result – zero impact on welfare dependency – is particularly stark, the studies cited above show that the conclusion that disability reforms of this type may lead primarily to benefit displacement rather than re-entry to employment is not unique to these particular Australian reforms. The suggestion is that disability reforms may need to do more than simply reduce the generosity and tighten the conditionality of payments – with the associated risk of exacerbating the already high levels of poverty experienced by people with disability in some countries (e.g. OECD, 2010) – if they are to substantially impact on welfare

dependence among people with disability. People with disability face barriers to employment that these measures, in isolation, are unlikely to overcome.

Figure 1



**Table 1 – Comparison of Policy Parameters for New Start Allowance (NSA)
and Disability Support Pensions (DSP)**

	DSP	NSA
Eligibility for Payment Type - Pre-Reform		
Functional Impairment	≥ 20 Impairment Points	If any condition for DSP is not met
Maximum Weekly Work Capacity	< than 30 hours/week	
Eligibility for Payment Type - Post-Reform		
Functional Impairment	≥ 20 Impairment Points	If any condition for DSP is not met
Maximum Weekly Work Capacity	< than 15 hours/week	
Activity Requirements		
Maximum hours/week	None	Weekly Work Capacity
Payments and Income Tests for Singles (2006)		
Maximum Weekly Payment (in \$)	A\$265.34	A\$205.30
Maximum Weekly Payment (in % Minimum Wage)	48%	40%
Maximum Weekly Income Before Payment is Reduced	A\$64	A\$62
Taper Rate	>\$A64: 50%	A\$62-A\$250: 50% >A\$250: 60%

Table 2: Characteristics of Applicants Before and After Reform

	01 July 2005 -30 June 2006	01 July 2005 -30 June 2006	01 July 2006 -30 June 2007	t-Test H0: Diff. =0
	Before Matching	After Matching		After Matching
	Mean	Mean	Mean	p-value
Highest Education				
Degree/post-graduate	0.05	0.04	0.04	0.97
Diploma/ Y12/ CertIII-IV	0.37	0.35	0.35	0.92
Y10-Y11	0.35	0.33	0.32	0.60
Less than Y10	0.23	0.28	0.29	0.54
Recent Work Experience				
Full-time	0.22	0.21	0.21	0.98
Part-time, >=8 hours per week	0.16	0.18	0.17	0.70
Unemployed	0.18	0.22	0.23	0.43
Unpaid/Irregular/Part-time 0-7 hrs/week; Out of the labour force	0.43	0.38	0.38	0.69
English Language Proficiency				
Good	0.80	0.75	0.73	0.34
Mixed	0.14	0.12	0.12	0.96
Poor	0.05	0.14	0.15	0.30
Geographic Region of Residence: Remoteness				
Metropolitan, inner regional	0.93	0.96	0.95	0.29
Outer regional, remote, very remote, migratory	0.07	0.04	0.05	0.29
Aboriginal or Torres Strait Islander				
No	0.89	0.89	0.89	0.50
Yes	0.11	0.11	0.11	0.50

Table 2 - continued

Marital status				
Single	0.39	0.30	0.29	0.67
Married	0.26	0.31	0.31	0.87
De-facto partner	0.07	0.06	0.06	0.85
Divorced	0.07	0.08	0.08	0.74
Separated	0.20	0.23	0.23	0.83
Widowed	0.02	0.02	0.02	0.35
Age groups				
<=29	0.24	0.10	0.10	0.96
30-39	0.17	0.17	0.17	0.94
40-49	0.24	0.30	0.30	0.95
50-59	0.22	0.34	0.34	0.98
>=60	0.13	0.09	0.09	1.00
Gender				
Female	0.39	0.43	0.43	0.94
Male	0.61	0.57	0.57	0.94
# individuals	3104	3104	2817	
Total weight (after matching)		2806	2817	

Table 3: Estimated Reform Impacts on the Hazard Rate for Exiting Welfare (Income Support)

	Leaving Income Support		
	Coeff.	Std. Err.	Haz. Ratio
Reform-Effect	0.640***	0.105	1.896
Reform-Effect Over Time (Interaction: Reform with time since beginning of spell, in 100 days)	-0.054*	0.026	0.948
Number of Observations		5910	
Number left Income Support		1876	
Number censored		4034	
Log-Likelihood		-15929.435	
Chi2(dF)		78.33(2)	

Note: ***, ** and * indicate significance at the 0.1%, 1% and 5% -level.

Table 4 Estimated Proportion Having Exited Income Support by Duration (Cumulative Hazard)

# of days after IS enrolment	Applicants		Difference
	1 July 2005-30 June 2006	1 July 2006-30 June 2007	
30	0.41%	0.78%	0.36pp
60	1.23%	2.29%	1.05pp
90	2.40%	4.36%	1.97pp
180	6.15%	10.75%	4.60pp
365	14.12%	22.84%	8.73pp
730	25.97%	37.46%	11.50pp

Table 5: Estimated Probability of Being in Receipt of any Income Support Payment by Duration (Cumulative Hazard)

	6 months after beginning of the spell		12 months after beginning of the spell		24 months after beginning of the spell	
	Mean	Std.Err.	Mean	Std.Err.	Mean	Std.Err.
Probability of receiving any income support payment						
applied 2005/06	0.942***	0.005	0.888***	0.007	0.810***	0.008
applied 2006/07	0.923***	0.005	0.871***	0.006	0.830***	0.007
Difference	-0.019**	0.007	-0.016	0.009	0.020	0.011

Notes: ***, ** and * indicate significance at the 0.1%, 1% and 5% -level.

Table 6: Estimated Reform Impact on the Hazard Rate for Returning to Income Support

Characteristics	Returning to Income Support		
	Coeff.	Std. Err.	Subhaz. ratio
Reform-Effect	1.014***	0.102	2.757
Reform-Effect Over Time (Interaction: Reform with time since beginning of spell, in 100 days)	-0.077*	0.036	0.926
Number of Observations		1817	
Number returned to income support		1197	
Number censored		620	
Log-Likelihood		-8382.766	
Chi2(dF)		146.90(2)	

Note: ***, ** and * indicate significance at the 0.1%, 1% and 5%-level.

Table 7: Estimated Proportion Having Returned to Income Support by Duration (Cumulative Hazard)

# of days after leaving IS	Applicants		Difference
	1 July 2005-30 June 2006	1 July 2006-30 June 2007	
30	8.27%	20.96%	12.69pp
60	14.41%	34.26%	19.85pp
90	18.72%	42.54%	23.82pp
180	27.54%	56.82%	29.28pp
365	37.45%	68.94%	31.49pp

Table 8: Estimated Reform Impact on the Hazard Rate for Leaving Current Benefit Type

Characteristics	Switching benefit type		
	Coeff.	Std. Err.	Haz. Ratio
Reform-Effect	0.349**	0.110	1.418
Reform-Effect Over Time (Interaction: Reform with time since beginning of spell, in 100 days)	0.561***	0.034	1.752
Number of Observations		5910	
Number left IS		1806	
Number censored		3096	
Number switched benefit type		1008	
Log-Likelihood		-7972.9078	
Chi2(dF)		479.07(2)	

Note: ***, ** and * indicate significance at the 0.1%, 1% and 5%-level.

Table 9: Cumulative Hazard of Having Switched Benefit Type

# of days after leaving IS	Applicants		Difference
	1 July 2005-30 June 2006	1 July 2006-30 June 2007	
30	0.11%	0.27%	0.16pp
60	0.24%	0.60%	0.36pp
90	0.43%	1.16%	0.74pp
180	1.19%	3.99%	2.79pp
365	2.76%	14.04%	11.28pp
730	3.89%	34.98%	31.09pp

Table 10: Benefit Types Received by Those who Switch

Benefit Types: Subsequent Spell	Benefit Types: Original Spell		
	DSP	NSA	Total
Age pension/Widow's pension	156	46	202
DSP	0	650	650
NSA	6	0	6
Other	14	136	150
Total	176	832	1008

Table 11: Reform Impact on Cumulative Hazard of Having Exited Income Support (Single Risk) – Different Specifications

<i>Change in Cumulated Hazard Rate After:</i>	<i>180 days</i>	<i>365 days</i>	<i>730 days</i>
Baseline Estimation	4.60pp	8.73pp	11.50pp
Exclude one month before and after 30 June 2006	4.67pp	8.56pp	10.50pp
Exclude three month before and after 30/06/2006	5.45pp	9.64pp	11.25pp
Exclude individuals with non-permanent conditions	6.87pp	11.86pp	12.69pp
Censor after one year	4.82pp	8.45pp	
Difference-in-differences	2.90pp	5.88pp	9.03pp
Competing-Risks estimation	4.97pp	9.95pp	13.58pp

Table 12: Reform Impact on Overall Probability of Being in Receipt of Any Income Support Payment – Different Specifications

	6 months after beginning of the spell		12 months after beginning of the spell		24 months after beginning of the spell	
	Mean	Std.Err.	Mean	Std.Err.	Mean	Std.Err.
Reform's impact on probability of receiving an IS payment						
Baseline Estimation	-0.019**	0.007	-0.016	0.009	0.020	0.011
Exclude one month before and after 30 June 2006	-0.020**	0.008	-0.014	0.010	0.029*	0.011
Exclude three month before and after 30 June 2006	-0.024**	0.009	-0.021	0.011	0.027*	0.013
Exclude individuals with non-permanent conditions	-0.032	0.017	-0.056*	0.023	0.018	0.025
Difference-in-differences	-0.010	0.008	-0.002	0.010	0.035**	0.011

Note: ***, ** and * indicate significance at the 0.1%, 1% and 5%-level.

References

- Autor, D.H. & Duggan M.G. (2006). The growth in the Social Security disability rolls: A fiscal crisis unfolding. *Journal of Economic Perspectives*, 20, 71–96.
- Autor, D.H., Maestas, N., Mullen, K. and Strand, A. (2015). Does delay cause decay? The effect of administrative decision time on the labor force participation and earnings of disability applicants. NBER Working Paper 20840.
- Autor, D.H., Duggan, M., Greenberg, K. and Lyle, D.S. (2016). The impact of disability benefit on labor supply: evidence from the VA's Disability Compensation program. *American Economic Journal: Applied Economics* 8(3): 31-68.
- Borghans, L., Gielen, A.C. and Luttimer, E.F.P. (2014). Social support substitution and the earnings rebound: evidence from a regression discontinuity in Disability Insurance reform. *American Economic Journal: Economic Policy* 6(4): 34-70.
- Bound, J. (1989). The Health and Earnings of Rejected Disability Insurance Applicants. *American Economic Review*, 79(3), 482-503.
- Bound, J. & Burkhauser, R.V. (1999). Economic analysis of transfer programs targeted on people with disabilities. In O. Ashenfelter & D. Card (Eds.), *Handbook of Labor Economics Volume 3C.* , Elsevier North Holland.
- Burkhauser, R.V., Daly, M.C., McVicar, D. & Wilkins R. (2014). Disability benefit growth and disability reform in the US: lessons from other OECD nations. *IZA Journal of Labor Policy* 3:4.
- Campolieti, M. (2004). Disability Insurance Benefits and Labor Supply: Some Additional Evidence. *Journal of Labor Economics*, 22(4), 863-887.
- Chen, S. & van der Klaauw, W. (2008). The Work Disincentive Effects of the Disability Insurance Program in the 1990s. *Journal of Econometrics*, 142(2), 757-784.
- de Jong, P., Lindeboom, M. & van der Klaauw, B. (2011). Screening Disability Insurance Applications. *Journal of the European Economic Association*, 9(1), 106-129.
- DEEWR (2008). *Welfare to Work Evaluation Report*. Canberra: Department for Employment, Education and Workplace Relations.

Department of Social Services (2015). A New System for Better Employment and Social Outcomes: Final Report of the Reference Group on Welfare Reform to the Minister for Social Services. Canberra: Department of Social Services.

Giertz, S.H. & Kubik, J.D. (2011). The Disability Screening Process and the Labor Market Behavior of Accepted and Rejected Applicants: Evidence from the Health and Retirement Study. *Journal of Labor Research*, 32(3), 237-253.

Gruber, J. (2000). Disability Insurance Benefits and Labor Supply. *Journal of Political Economy*, 108(6), 1162-1183.

Karlström, A., Palmeb, M. & Svensson, I. (2008). The employment effect of stricter rules for eligibility for DI: Evidence from a natural experiment in Sweden. *Journal of Public Economics*, 92, 2071-2082.

Kuehnle, D. & Scutella, R. (2011). Employment retention in the economic downturn, Melbourne: Melbourne Institute of Applied Economic and Social Research.

McVicar D. & Wilkins, R. (2013). Explaining the growth in the number of recipients of the Disability Support Pension in Australia. *Australian Economic Review*, 46(3), 345–356.

Mitra, S. (2009). Disability Screening and Labor Supply: Evidence from South Africa. *American Economic Review Papers and Proceedings*, 99(2), 512-516.

Organization for Economic Co-operation and Development (OECD) (2010). Pathways onto (and off) Disability Benefits: Assessing the Role of Policy and Individual Circumstances. In: *OECD Employment Outlook 2009*. Paris: OECD.

Organization for Economic Co-operation and Development (OECD) (2010). *Sickness, Disability and Work: Breaking the Barriers: A Synthesis of Findings across OECD Countries*. Paris: OECD.

Staubli, S. (2011). The impact of stricter criteria for disability insurance on labor force participation. *Journal of Public Economics*, 95(9-10), 1223-1235.

von Wachter, T., Song, J. & Manchester, J. (2011). Trends in Employment and Earnings of Allowed and Rejected Applicants to the Social Security Disability Insurance Program. *American Economic Review*, 101(7), 3308-3329.