# Essays on the economics of household finance and social insurance

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# Chapter 5

# Does Opting Out of Public Disability Insurance lead to more Outflow to Work? Evidence from the Netherlands


## Abstract

In the Netherlands, firms can opt out of partial and temporary Disability Insurance (DI), becoming responsible for reintegrating disabled workers themselves. Opting out creates incentives for firms to reintegrate disabled workers. In this chapter, I provide the first evidence of whether opting out of disability insurance increases work-related exits from partial and temporary disability. I estimate duration models using administrative records from 2006 to 2022 containing all disability spells that started between 2006 and 2021, DI status, and the labor market status of temporarily disabled workers. I find that outflow to work is higher among non-publicly insured firms. These differences are a result of composition effects, related to both employee and employer characteristics. After controlling for differences in composition, no significant differences remain between firms that opt out versus those that are publicly insured. However, I find higher outflow to recov-



This chapter is single-authored. This chapter is based on anonymized data from DI recipients in the Netherlands, provided by the Dutch Employee Insurance Agency. The opinions expressed in this chapter are solely those of the author and do not necessarily reflect the official policy or position of the Dutch Employee Insurance Agency or any of its partners. Responsibility for the data analyses and content in this chapter lies entirely with the author. The data used in this study are confidential and cannot be shared publicly. I am grateful to the Dutch Employee Insurance Agency, Marloes Lammers, Carla van Deursen, and Frank Schreuder. This project would not be possible without them. I thank Marloes Lammers, Margaretha Buurman, and Miranda de Vries for useful comments and suggestions.

ery without work, and to full disability for those who opt out, and lower outflow for other reasons such as retirement, even after accounting for composition effects. Additionally, I find more re-assessments to a lower degree of disability among non-publicly insured workers. These findings are driven by the fact that firms who opt out are more active in requesting re-assessments for non-structurally disabled workers. My findings indicate that more active requesting of re-assessments leads to more efficient sorting of workers who regain (part of) their earning potential. However, re-assessments also increase outflow to structural disability. The results may have policy implications with respect to re-assessments: facilitating re-assessment requests can enhance DI outflow by swiftly capturing updates in disabled workers' health status.[1]

## 5.1 Introduction

Disability poses one of the greatest risks one can suffer in their career. In addition to losing one's current earnings capacity, disability disrupts human capital accumulation throughout the life cycle, thereby hindering one's earnings potential for the remainder of one's working life.

Most developed countries have Disability Insurance (DI) schemes to insure workers against this risk of disability. These disability schemes are usually government-provided, paying out a percentage of one's previous earnings capacity or a percentage of the minimum wage.

Some countries have non-public alternatives to DI, such as Canada, Switzerland, and the Netherlands (McVicar et al. (2022)). In these settings, private insurers are responsible for reintegrating and covering DI benefits of disabled workers themselves. This creates an incentive to re-integrate workers more actively than in the public system, as successful reintegration lowers DI premiums.

The Dutch DI system distinguishes between partial disability, temporary (full) disability, and structural full disability. Structural

[1]This chapter is based on anonymized data from DI recipients in the Netherlands, provided by the Dutch Employee Insurance Agency. The opinions expressed in this chapter are solely those of the author and do not necessarily reflect the official policy or position of the Dutch Employee Insurance Agency or any of its partners. Responsibility for the data analyses and content in this chapter lies entirely with the author. The data used in this study are confidential and cannot be shared publicly.

full disability benefits are always provided through public insurance, but employers can opt out of public insurance for partial and temporary (full) DI. In this case, employers can either reintegrate and pay DI benefits to disabled workers themselves, or re-insure through a private DI insurer.[2]

Partial and temporary (full) DI entitlement may cease due to various factors. First, workers can be re-assessed to having recovered from disability. Second, workers can be re-assessed to full DI, after which employers no longer have obligations to reintegrate. Finally, DI benefits can end for other reasons such as retirement.

In the Netherlands, opting out of public insurance results in several distinctions compared to employers who remain publicly insured. First, opting out augments the incentive and opportunity for employers to influence the reintegration of employees. Second, non-publicly insured employers (and/or the firm they insure at) can request re-assessments themselves instead of relying on the Dutch Employee Insurance Agency to do so. While re-assessments requested by non-publicly insured employers are still handled by the Dutch Employee Agency, non-publicly insured employers have an incentive to request re-assessments[3] of workers that the Dutch Employee Agency does not have: outflow from partial DI leads to lower DI premiums. Lowering premiums through re-assessments is achievable in two forms: First, re-assessments to a lower degree of disability results in lower benefit payments. Second, re-assessments that result in an employee moving out of DI or transitioning to structural DI relieve non-publicly insured employers from the obligation to pay the employee's insurance premiums.

Earlier literature has highlighted differences between non-publicly insured firms based on selection into non-public insurance (Hassink et al. (2018)), insurer effort (Koning and van Lent (2024)), and outflow out of DI (Lammers et al. (2018)). Notably, the latter study shows that both the total DI outflow and outflow to structural DI are significantly higher among non-publicly insured firms. However, no evidence yet exists on whether Dutch non-publicly insured firms reintegrate disabled workers more effectively.

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[2] The data do not differentiate between these two options.

[3] Firms are not charged any fees for re-assessments, but are required to fulfill administrative prerequisites.

In this chapter, I provide evidence on how non-public DI affects return-to-work in a unique institutional setting with a comprehensive set of Dutch administrative records. I observe a rich set of characteristics of both disabled workers and employers. Including but not limited to labor market earnings, degree of disability, employer traits, and re-assessments. This detailed data allows me to explore the mechanisms underlying non-publicly insured reintegration. I estimate duration models using Dutch administrative records. My contribution to the literature is twofold. First, I estimate how non-public disability insurance affects outflow to work. No existing Dutch literature has investigated this yet. The Dutch system is of particular interest as strong return-to-work incentives are present for employees in both public and non-public DI provision. Second, I enhance existing Dutch DI outflow estimates with more detailed data over a longer period of time than in existing literature, allowing for a higher degree of external validity than existing papers. The data contain a rich set of employee- and employer characteristics[4], allowing me to explore the mechanisms behind differences in publicly versus non-publicly insured DI.

My findings are as follows. First, I discover higher DI outflow to work among non-publicly insured employers. However, these differences are entirely driven by selection effects on the basis of employer- and employee characteristics: differences in outflow to work disappear when I study DI spells of employees that switch from public to non-public insurance as a result of the firm switching insurance status. However, I also observe a notable increase in the outflow to full Disability Insurance (DI) and recovery without resuming work. The impact on the former is particularly substantial, even among DI spells that switch insurance status. Finally, my findings reveal marginally higher yet statistically significant rates of re-assessments resulting in a reduction of the degree of disability for non-publicly insured workers. Differences in outflow to full DI are primarily driven by non-publicly insured firms having workers re-assessed more often. These findings suggest that while outflow to work may not increase as a result of non-public DI provision, non-public DI does lead to more outflow to full disability and thereby to less outflow for other reasons such as

[4]I.e., on the basis gender, age, sector, labor market area, degree of disability, and diagnosis category.

retirement.

This chapter is organized as follows. Section 2 lays out the Dutch DI setting. Section 3 discusses the existing literature on DI. Section 4 explains the data. Section 5 lays out the methodology. Section 6 presents results. Finally, section 7 concludes.

## 5.2 Institutional setting

This section describes the key elements of the Dutch DI system and the implementation of non-public disability insurance provision.

In the Netherlands, workers receive paid sick leave from their employer if they are unable to work as a result of sickness. Dutch workers can apply for DI after two years of sick leave. After filing for DI, Dutch workers are subject to an assessment of their degree of disability. Disability claims are assessed by a physician independent of the employer. Based on the input from the physician, a vocational expert determines the lost earnings capacity. The vocational expert uses a computer system (CBBS) as an input for his or her analysis. The computer system helps in determining which jobs workers could still perform, given their (remaining) abilities and capabilities. DI claims are admitted for workers who are assessed to have lost 35% or more of their earnings capacity. After a DI claim is admitted, workers receive a benefit on the basis of their lost earnings capacity relative to their wage before sick leave. Depending on the lost earnings capacity (or: degree of disability), workers are then categorized depending on their degree of disability and potential to recover.

Workers who have lost 35% of their earning capacity or less do not receive disability benefits. Workers with disability degrees between 35% and 80% receive partial DI (WGA 35-80) benefits, with strong return-to-work incentives in place since the benefits are only partial. For disability degrees of 80% or higher, the benefit awarded depends on whether the disability is assessed to be structural. When the disability cannot (yet) be considered structural, so-called temporary WGA DI benefits are awarded (WGA 80-100), equal to 70% of one's previous wage. These benefits are subject to reintegration efforts. I henceforth refer to both WGA benefits as temporary and partial DI. When the disability is considered structural, full DI benefits (IVA) equal to 75%

of one's previous wage are awarded, with no mandatory reintegration in place. Additionally, while both WGA schemes can be insured either publicly or non-publicly, IVA is always insured publicly.

As IVA benefits are never insured non-publicly, I estimate the models for DI recipients who start out in partial and temporary disability. In both WGA schemes, workers are first awarded a wage-related benefit (LGU) for between 3 and 24 months depending on how many years they worked prior to disability. The LGU amounts to a maximum of 75% of one's prior wage for the first two months, and 70% thereafter. It is worth noting that the LGU benefit level is the same as the Dutch unemployment benefit level. After the LGU ends, the benefit awarded increases with the degree to which workers use their remaining earning capacity.

Compared to DI systems in other countries, the Netherlands has highly stringent admission requirements, as well as strong incentives for employers to facilitate return-to-work for non-structurally disabled workers. However, the benefits paid out are high relative to other OECD countries, especially for workers who comply with the return-to-work incentives (McVicar et al. (2022)).

Disability in the Netherlands is publicly insured, for which employers pay an experience-rated premium based on how many sick and partially[5] disabled employees they have. This premium is bounded by between 0.16% and 2.64%[6] of wages for sick workers, and 0.21% and 3.48% of wages for disabled workers UWV (2023). Hassink et al. (2018) provide more details on public insurance premiums. Employers, however, can opt out of this public insurance for the partial and nonstructural DI scheme[7]. When firms opt out, assessments and DI premiums remain publicly set, but insurance payments and reintegration efforts are carried out by the employer instead of the public system. Additionally, DI benefits are no longer experience rated. Employers that opt out of public insurance have two options at hand:

1. Employers self-insure disabled workers. This means that they pay the individual DI benefits of employees themselves and become

[5]Structural full DI is not subject to experience-rating.

[6]The temporary work sector is an exception to this upper bound, instead having a maximum of 8,27%.

[7]Opting out requires a warranty declaration from the Dutch Tax Agency. This measure serves to prevent firms that cease to exist from shifting non-public DI spells to the public system.

responsible for reintegration.

2. Employers re-insure their DI risk at a private insurance company, in which case the insurance company (or another private party) handles reintegration and the employer pays the private company's DI premium, which large firms can negotiate.

Opting out creates a means to facilitate outflow not present for publicly insured firms: non-publicly insured employers, as opposed to the Employee Insurance Agency, have an incentive to monitor disabled workers and request re-assessments[8]. The Employee Insurance Agency (UWV) handles these re-assessments, and workers do not have the option to refuse this re-assessment. Second, non-publicly insured employers can design their own reintegration methods, including potential work accommodations and bonuses. Third, private insurers and firms may have their own reintegration and/or health professionals, allowing for potentially faster reintegration.

Due to the different incentives and reintegration mechanisms, employers tend to self-insure when they have relatively good prospects of reintegrating disabled workers. Non-publicly insured employers are over-represented in agriculture, construction, industry, retail, and transport. Furthermore, most non-publicly insured employers have a moderate (25-100) or very large (250+) number of employees (Cuelenaere et al. (2013)). This is driven by several factors. First, irrespective of insurance status, large firms face individual DI premiums as opposed to sector-based ones, creating a larger incentive to reduce their number of disabled workers. Second, large firms tend to have more developed human resource and personnel management options. Finally, large firms relatively often have the required solvency to self-insure and can diversify their DI inflow risk.

When employers self-insure, they no longer pay the aforementioned premiums, instead covering most individual DI benefits[9] and become responsible for reintegrating workers. The Dutch experience rating in the public system leads to DI premiums and the sum of individual benefits being roughly equal. As such, there are incentives

[8]Firms do not pay the costs of the aforementioned re-assessments, though some administrative red tape and requirements on behalf of the employer are present.

[9]Increased DI benefits as a result of using one's earning capacity are not borne by non-public insurers as to not disincentivize work resumption. Instead, employers pay the benefit the worker would have received in the absence of employment (UWV (2023)).

for the firm to decrease their total number of disabled workers, but not to switch to and from public insurance for a given number of disabled workers. Employers and other external parties (like insurers, or workers) can themselves request re-assessments by an insurance doctor, which are given a higher priority than re-assessments requested by insurance doctors themselves. However, even among non-publicly insured firms, re-assessments are always performed by the Dutch Employee Insurance agency with the assessor selected at random.

Incentives for non-publicly insured employers to have their employees re-assessed are twofold: First, having disabled workers re-assessed to a lower degree of disability results in non-publicly insured employers having to pay fewer DI benefits. Second, both employees and non-publicly insured employers have an incentive to transition to full disability: the employee then receives a 75% replacement rate without any reintegration obligations, while the non-publicly insured employer no longer has to pay the worker's disability benefits.

## 5.3 Literature review

Although the incentives underlying Dutch DI inflow and outflow have been extensively studied, there is very little direct evidence on the relative outflow rates between public and non-public DI insurance. Hassink et al. (2018) find no structural selection into non-public DI when DI outflow is low, but do discover selection in the two years prior to DI inflow when risks are temporarily low. Koning and van Lent (2024) show that non-public DI creates incentives to reduce moral hazard and increase outflow. Lammers et al. (2018) show that, as compared to public DI, non-public DI increases total outflow, outflow to recovery, and outflow to structural disability. However, to the best of my knowledge, no evidence on how non-public DI compared to public DI affects outflow to work exists yet. This chapter contributes to the scarce literature on non-public DI and is related to several strands of existing literature.

DI insures workers against disability risk over the life cycle. However, DI also creates several disincentives for disabled workers. First, DI decreases the relative return of working, and therefore discourages return-to-work efforts (French and Song (2014); Gelber et al.

(2017); Gruber (2000); Maestas et al. (2013); Ruh and Staubli (2019); Soika (2018); Spierdijk et al. (2009)). This phenomenon is known as moral hazard. Second, the presence of DI systems may lead to self-selection out of DI by individuals with low disability risk (Landais et al. (2021)), a phenomenon known as adverse selection. Adverse selection leads to conventional private DI markets failing, which makes public DI systems welfare enhancing (Chetty (2008)). These welfare gains then exhibit a trade-off between moral hazard and the insurance effect (Chetty (2008)).

Some countries offer private DI schemes in addition to public ones (McVicar et al. (2022)). International literature provides some evidence on the incentives created by non-public DI. Rehwald et al. (2017) discover, in an experiment, that non-public employment services offer more intensive job search assistance. This heightened intensity, however, does not result in higher job finding rates. Autor et al. (2014) find – using firm records in the United States, that non-public long-term disability exhibits higher return-to-work rates, particularly among low-liquidity and relatively healthy disability claimants, as these individuals have the most remaining work capacity and greater incentives to resume work. Seitz (2021) shows, using German private DI insurer data, that private DI increases moral hazard in the public DI system, as this allows workers to acquire additional benefits in addition to the public ones. However, the overall existence of public and private DI is not necessarily detrimental to welfare. Seibold et al. (2022) show German private DI take-up is primarily concentrated among low-risk and high-income individuals. This type of advantageous selection is primarily driven by these individuals having a lower risk preference than high-risk and low-income individuals.

DI design parameters may also affect outflow rates. Some Dutch evidence with respect to how non-public DI insurance affects inflow and outflow exists in the literature. Hassink et al. (2018) show that there is a selection mechanism in Dutch public disability insurance: Employers who anticipate relatively low DI inflow risk opt out of public insurance, arguing that differences in DI inflow rates are driven by selection mechanisms rather than caused by the services of private DI insurance. These selection mechanisms make it difficult to provide causal evidence of the effects of non-public DI. Garcia-Mandicó et al.

(2020) identify a 20% reduction in DI income and a €636 increase in labor market earnings for every €1000 reduction in DI benefits. These findings are primarily driven by workers with shorter claim durations and disabilities which are difficult to measure. De Groot and Koning (2016) estimate the effects of experience rating DI premium payments for small firms, finding a 7% decrease in DI inflow and a 12% increase in DI outflow. However, it is worth noting that the estimates in De Groot and Koning (2016) are primarily concentrated among the first year of sick leave and estimated prior to a DI reform that increased sick leave durations from 1 to 2 years. This reform means that the estimates likely do not hold for the current Dutch DI system. Koning and van Lent (2024) estimate the role of insurer effort in reintegrating disabled workers in the Netherlands. They estimate that insurer-based incentives to offer work accommodations and rewards for work resumption can offset moral hazard from DI recipiency. DI stringency also directly affects inflow and outflow rates. Garcia-Mandicó et al. (2020) estimate how the re-assessment of Dutch workers under a more stringent DI system affected disability receipt and earnings, and find increases in earnings as well as decreases in DI recipiency. Engström et al. (2017) estimate how early caseworker meeting with sick workers affects inflow to DI in Sweden, showing a 20% increase in DI inflow, with this inflow increase being potentially driven by sick workers signaling bad health in said intervention.

Related literature has investigated re-integration and preventative programs. Baert et al. (2018) use Dutch administrative data to estimate the effect of interventions by medical and occupational specialists on the sick leave of self-employed workers, finding that both intervention types adversely affect recovery rates. While no clear underlying mechanism is demonstrated, potential explanations include asymmetric information with respect to sickness and caseworkers being more focused on long-term recovery than on sick leave durations compared to self-employed workers. Viering et al. (2015) estimate the effect of individual placement and support on job finding rates for disability pensioners, showing that disability pensioners with individual placement and support were 20 percentage points more likely to find work than those who did not. Brongers et al. (2023) discover, using a randomized controlled trial in the Netherlands, that specialized care by labor market experts does not increase recovery for workers with multiple disabilities as compared to Dutch baseline reintegration systems. Peijen and Wilthagen (2022) show that company-based vocational education increases rehabilitation in the Netherlands, especially among workers with cognitive disabilities. Re-integration may therefore be most effective for workers with a single non-physical disability.

## 5.4 Data

I use administrative data from the Dutch Employee Insurance Agency from 2006 to 2022. The core sample is based on temporary and partial DI admissions between 2006 and 2021. I observe start- and end dates of DI spells and other spell-related characteristics such as sector, diagnosis type, degree of disability, employer size, and the age and gender of the employee.

I also observe monthly labor market records and monthly DI payments. These allow me to identify whether individuals work, at which employer DI recipients work, their earnings, the exact DI benefits handed out, and the insurance status of the employer. I merge labor market and DI payment records to the core sample such that I observe in each month whether DI recipients are working, and at which employer they work if so. From these data, I then construct indicators on whether DI recipients work during and/or after their DI spells.

Prior to the sample selection, I observe the start and end dates of all 195.000 DI spells that started in the sample period. In the data, I distinguish between three insurance types: publicly insured non-government employers, non-publicly insured non-government employers, and government employers. For the non-publicly insured, the data does not distinguish between either self-insuring or obtaining re-insurance at a private insurer. I distinguish between government and non-government for several reasons. First, government employers always self-insure. As such, no selection effects are present. Second, government employers are limited to only two specific sectors, whereas non-publicly insured non-government employers are active in a broader support of sectors. Third, government employers themselves have strongly different characteristics from non-publicly insured employers. Due to these differences, I do not include government employ-

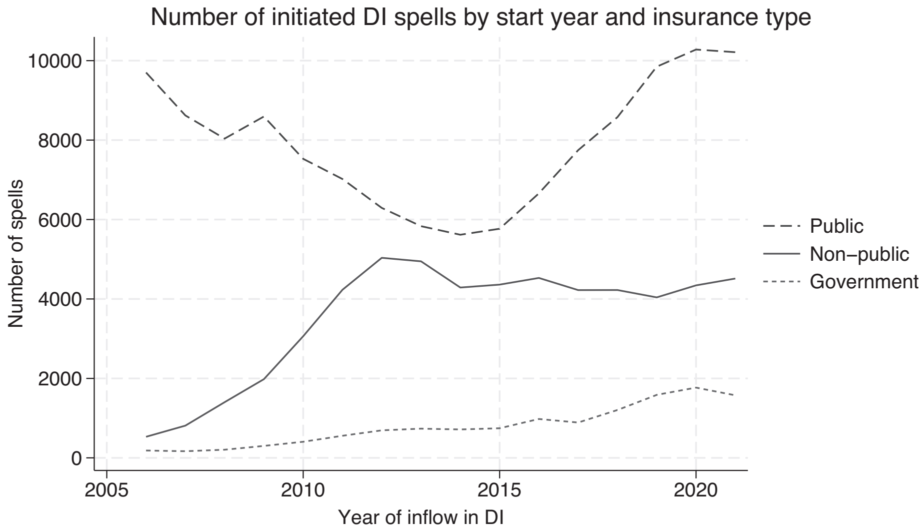


Figure 5.1: Total number of DI spells that started between 2006 and 2021, separated by start year and insurance status of the employee on the first day of DI entitlement. Government is defined as working directly for the government. Services provided to the government by private parties do not fall under the government category.

ers in the analysis in chapter 6. My selection criteria leave roughly 182 000 spells, of which roughly 13 000 result in outflow to work.

Figure 5.1 presents the number of starting DI spells by insurance type for every year in the sample. In the initial years of the observational period, a notable surge in the opt-out rate is discernible until 2013, followed by a decline in subsequent years. This trend is attributed to several contributing factors. Initially, an erroneous setting of public Disability Insurance (DI) premiums in 2009 resulted in them being set at levels that were too low (Cuelenaere et al. (2013)). Consequently, post-2009, there was an escalation in public DI premiums, prompting numerous employers to opt out of the public insurance scheme. Additionally, until 2013, non-publicly insured employers imposed lower DI premiums compared to those of the public insurance sector (UWV (2014)). After 2014, these public premiums were increased. The trend is supported by changes in the relative proportion of employers opting for non-public insurance coverage are in line with the changes in the premiums.

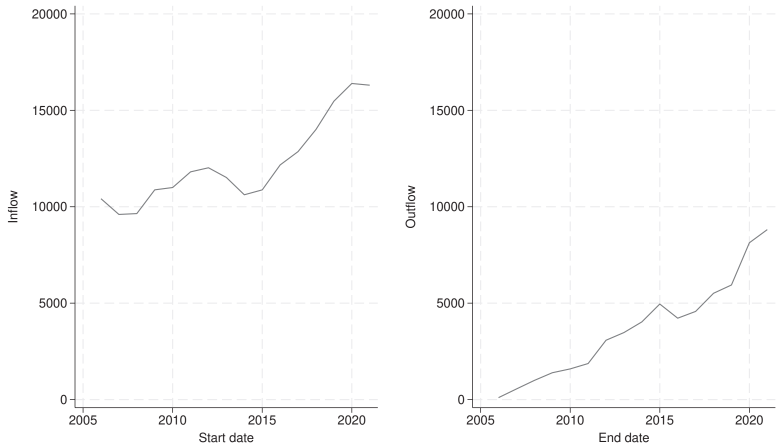


Figure 5.2: Number of governmental and non-governmental DI spells that started and ended in every year between 2006 and 2022.

Figure 5.2 illustrates DI inflow and outflow in the sample. Inflow starts at approximately 10,000 cases per year, and climbs upwards after 2015. Outflow starts at 0 cases per year, as I only investigate spells that started in 2006 or later, then steadily trends upwards to just under 10,000 cases a year by the end of the sample.

| Characteristic | Public Insurance | Non-public Insurance | Government | Total |
|---|---|---|---|---|
| Male | 44.6% | 40.8% | 40.4% | 43.2% |
| Age | 48.0 | 48.5 | 49.5 | 48.3 |
| Diagnosis: | | | | |
| Cancer | 6.8% | 6.7% | 6.4% | 6.7% |
| Cardiovascular disease | 8.4% | 9.5% | 10.0% | 9.1% |
| Psychological ailment | 34.8% | 34.2% | 44.4% | 35.3% |
| Musculoskeletal issues | 26.2% | 28.5% | 14.7% | 26.1% |
| Other/Unknown | 23.8% | 21.2% | 24.5% | 23.1% |
| Benefit (degree of disability) at inflow | | | | |
| 28 | 10.0% | 10.6% | 12.9% | 10.3% |
| 35 | 8.9% | 9.1% | 12.9% | 9.2% |
| 42 | 6.1% | 6.6% | 9.0% | 6.4% |
| 50,75 | 6.4% | 6.9% | 11.2% | 6.9% |
| 70 | 18.0% | 6.2% | 5.5% | 13.8% |
| 75 | 50.6% | 60.7% | 48.5% | 53.4% |
| Employed at inflow | 56.5% | 67.3% | 85.8% | 61.5% |
| Employed at same employer at inflow | 48.9% | 58.9% | 77.6% | 53.6% |
| Employer sector: | | | | |
| Agriculture and nutrition | 4.4% | 4.6% | 0% | 4.2% |
| Construction and wood | 4.2% | 4.0% | 0% | 3.8% |
| Industry | 11.6% | 13.0% | 0% | 11.2% |
| Retail | 13.4% | 14.6% | 0% | 12.9% |
| Transport | 5.8% | 7.7% | 0% | 6.0% |
| Financial services | 11.3% | 10.5% | 0% | 11.0% |
| Healthcare | 19.6% | 32.2% | 0% | 22.0% |
| Government (incl. education) | 16.5% | 12.9% | 100% | 17.2% |
| Other | 13.2% | 11.1% | 0% | 11.7% |
| Employer size: | | | | |
| Small (0-10) | 10.3% | 8.0% | 0.2% | 9.0% |
| Medium (10-100) | 18.9% | 14.9% | 0.7% | 16.6% |
| Large (>100) | 43.6% | 62.4% | 81.2% | 51.5% |
| Unknown | 27.2% | 14.7% | 17.9% | 23.0% |

Table 5.1: Individual characteristics of DI recipients separated by insurance status on the first day of DI entitlement. DI benefits in the Netherlands are assigned to categories, rather than an exact percentage. 'Government' is defined as working directly for the government. Services provided to the government by private parties do not fall under the government category.

Table 5.1 presents characteristics of DI recipients the moment they enter DI. Gender, age, diagnosis, and sector differences between publicly and non-publicly insured spells are small. Non-public DI spells have lower degrees of disability upon entry and are relatively likely to be employed upon entry.

On the employer-side, DI claimants at firms that opt out work in tertiary sector jobs relatively often, and are more often employed at large employers. Both of these phenomena are likely driven by firm size: Larger firms, on average, have a relatively stable number of DI recipients due to having individual public premiums instead of sector-based ones, and as such a greater incentive to self-insure. The healthcare sector, in addition to generally having large employers, have the expertise to reintegrate workers themselves, underlying the relatively large opt-out rate in this sector. No government employees are present in the healthcare sector, as healthcare provided by the government is classified as belonging to the government sector. Finally, non-government employees are present even in the government sector due to private contracts in this sector. These employees are classified as non-government. The data allow me to control for a wide set of employer characteristics, as well as the type of health issues DI claimants suffer from. This is particularly important because the differences in characteristics likely result in non-public DI spells ending more quickly.

Approximately a third of the DI spells in the data do not end within the sample, usually as a result of starting near the end of the sample. Additionally, for approximately one percent of DI spells I know that they ended within the sample, but not when. I know that these spells lasted for at least one month, but I cannot accurately determine their duration. As such, I set these spells as censored after the first month. Not accounting for spells ending past the point where I can observe them or have unknown durations leads to biased results. To prevent said bias, spells that do not end within the data or become censored are included in my estimates and implicitly subject to counterfactual prediction: On the basis of other spell end dates, my estimates predict when spells that did not end in the data would have ended later. As such, the number of estimated DI exits is larger than the number of DI exits I observe in the data.

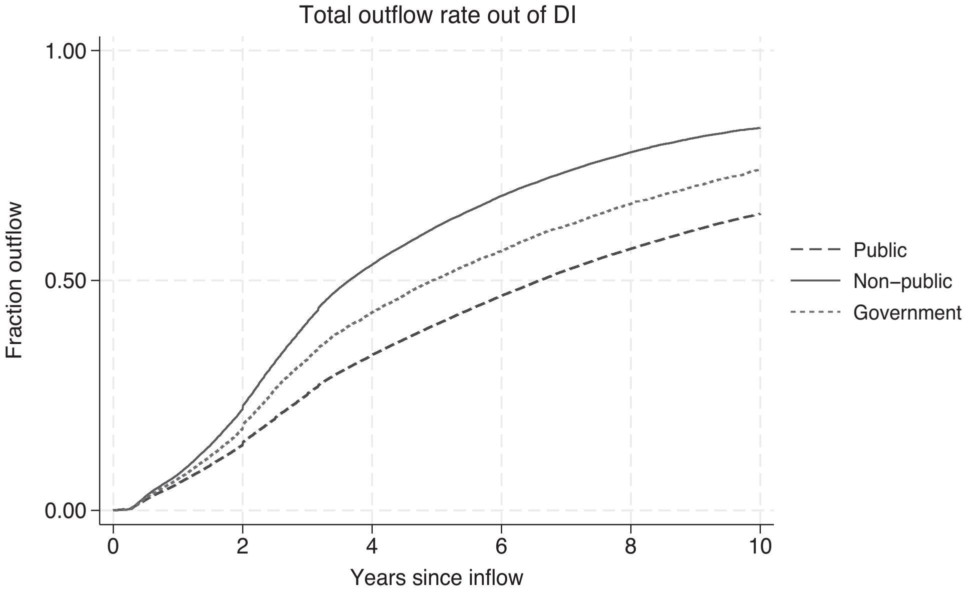


Figure 5.3: Fraction of spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement.

Figure 5.3 shows outflow from DI for any reason per insurance type, measured as the fraction of DI recipients in the sample that has left DI. In the first two years of DI benefits, outflow rates are relatively high, with small spike effects two years and thirty-eight months after inflow, and an acceleration of outflow afterwards. This phenomenon may be a result of disabled workers no longer being entitled to their initial benefit (LGU)[10] and transitioning to a (lower) benefit after this period. The two distinct peaks are driven by the maximum duration of the LGU decreasing within the sample: Before 2015, the maximum duration of the LGU was 38 months. From 2015 on, the LGU was phased out to last at most 2 years for new DI recipients. This phenomenon matches the spike effects found in Mesman et al. (2023).

Afterward, DI outflow accumulates at a decelerated rate. After 10 years, roughly two thirds of DI recipients stopped receiving benefits, with higher outflow rates among non-publicly insured workers, especially among those who do not work for the government. In the longer run, outflow continues to accumulate. The data continues af-

[10]Details on the LGU can be found in section 2.

ter the 10 years shown, but the additional outflow afterward is almost entirely for reasons such as retirement.

In addition to examining the characteristics of employers and employees, my main analysis focuses on employers that transition between public and non-public DI provision within the sample. Prior to 2014, when an employer opted out, either the employer or the insurer assumed responsibility for the rehabilitation of all disabled workers within the firm. Conversely, when a non-publicly insured firm elected to transition to public insurance, all existing DI cases remained with the firm or its insurer. Starting in 2014, employees at small firms continued to be covered by public insurance even if the firm opted out, and this policy was extended to large firms in 2017. This has the following implications for the analysis:

1. When firms opted out prior to 2014, all of their current DI spells became non-publicly insured.
2. From 2014 to 2016, the current spells of small firms stayed publicly insured when they switched insurance status.
3. From 2017, all current DI spell remained publicly insured when the employer opted out.

Firms having both publicly and non-publicly insured employees allows me to control for firm-specific and employee-specific characteristics that influence the speed of outflow from (partial and temporary) DI. Moreover, this also allows me to test for dynamic selection on the basis of outflow. To do so, and to prevent switching from being an outcome of long DI spells, I investigate spells of firms that switched from public to non-public insurance prior to 2017. I refer to these spells as switchers henceforth. If dynamic selection is present, I expect outflow rate effects to be present in the year prior to switching to non-public insurance. This chapter tests for this type of selection in the results.

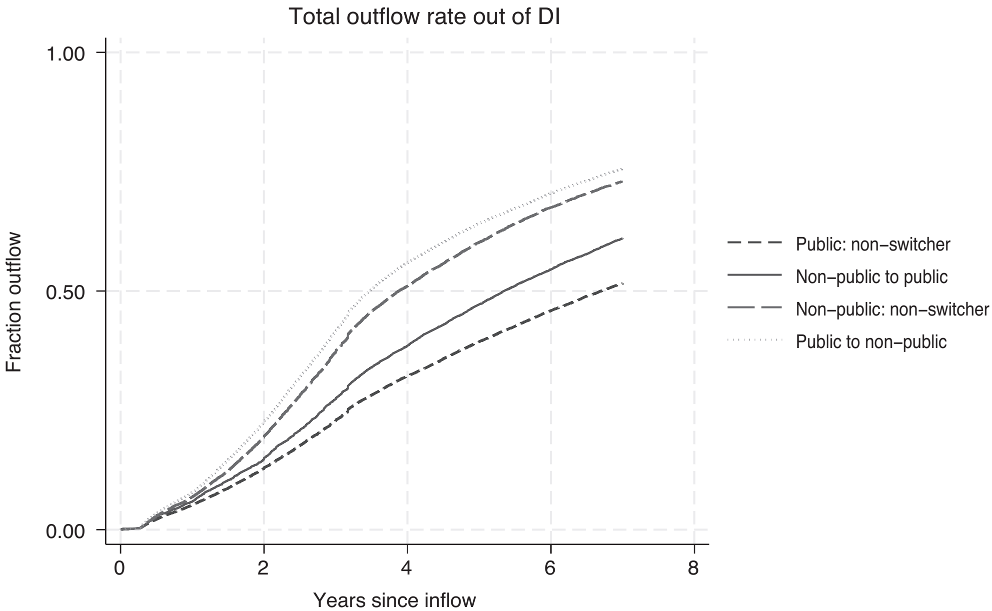


Figure 5.4: Fraction of DI spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement and whether the employee switched to or from public insurance during their disability spell.

Figure 5.4 shows DI outflow rates separated on the basis of both non-public and publicly insured firms in addition to a binary indicator of whether firms switch from public to non-public insurance during the employee's disability spell. As in figure 1, DI outflow is higher among non-publicly insured spells both among switchers and among non-switchers. Spells that switch, however, exhibit a slight increase in DI outflow as compared to spells that do not switch. This may be the result of dynamic selection, but may also indicate a causal mechanism being present. These differences may be the result of selection effects in non-public insurance. Note that figures 1 and 2 measure total outflow without accounting for competing risks. I also compute DI outflow separated by outflow type. Figure 4 shows Cumulative Incidence functions resulting from that exercise. Cumulative Incidence functions differ from Kaplan-Meier estimates in that they scale the incidence function by all other failure types, thus accounting for competing risks.

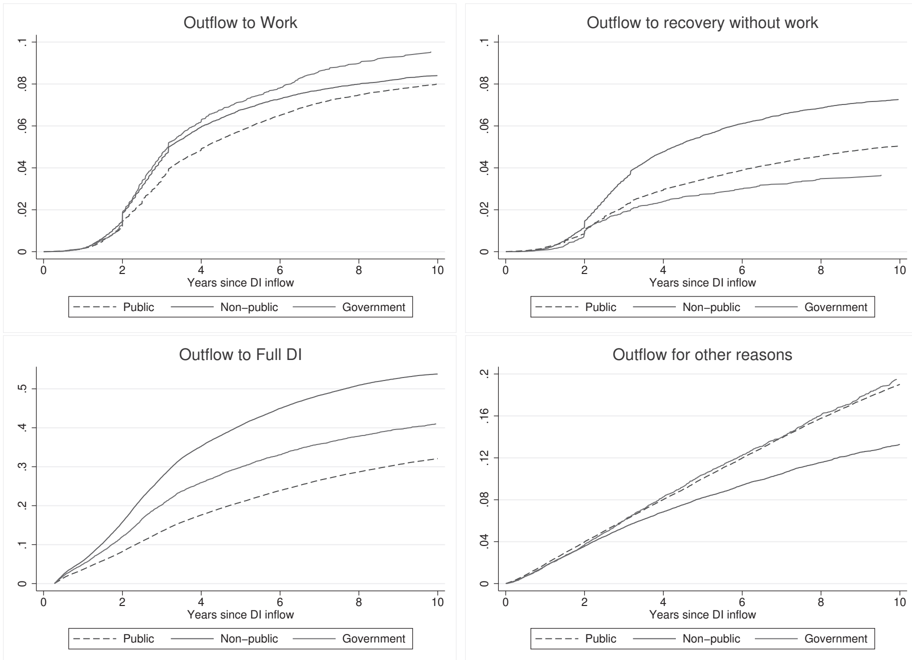


Figure 5.5: Fraction of DI spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement and accounting for cumulative incidence of outflow reasons.

Figure 5.5 presents total DI outflow separated by outflow type. After 10 years, roughly 8% of DI spells end in outflow to work, with slightly higher figures among non-publicly insured DI recipients. Recovery without work is roughly 7% among non-publicly insured workers and roughly 4.5% among publicly insured workers. Figure 3 shows large outflow differences to full DI. Whereas roughly 30% of publicly insured workers flow out to full DI after 10 years, this figure is nearly twice as large for non-publicly insured workers. Most of the remaining DI outflow occurs as a result of retirement. Outflow for other reasons is notably smaller for non-publicly insured DI spells than for public and government DI spells. Additionally, outflow for other reasons becomes the main outflow reason in the long run, with the effect dominating earlier for public and government employers than for non-publicly insured employers.

Table 5.2: Percentage of spells in the sample that resulted in an approved re-assessment, separated by whether the spell is publicly or non-publicly insured.

| | **Public** | | **Non-public** | |
|---|---|---|---|---|
| | **Mean (%)** | **Standard error (%)** | **Mean (%)** | **Standard error (%)** |
| **Partial DI to temporary full DI** | 4.05 | 0.06 | 5.66 | 0.09 |
| **Temporary full DI to partial DI** | 3.89 | 0.06 | 6.64 | 0.12 |
| **Temporary/partial DI to recovery** | 5.09 | 0.07 | 10.62 | 0.13 |
| **Partial DI to structural full DI** | 2.99 | 0.05 | 4.49 | 0.08 |
| **Temporary full DI to structural full DI** | 11.26 | 0.09 | 21.75 | 0.17 |
| **Total** | 27.28 | 0.15 | 49.16 | 0.25 |

I additionally investigate re-assessments in the data to identify a causal mechanism underlying outflow, as all spells that end in the data either end as a result of a re-assessment taking place or due to other reasons such as retirement. As I do not observe re-assessments prior to 2013, estimates of successful re-assessments are necessarily a lower bound. Table 5.2 shows the percentage of spells that were successfully re-assessed on the basis of whether the requester is public or non-public. Re-assessments from partial DI to temporary full DI are much less common among non-public requesters, likely due to this type of re-assessments increasing the DI payments they have to make. I also see slightly fewer approved re-assessments from partial DI to structural full DI. All other types of successful re-assessments, however, are more common among non-public requesters. Particularly re-assessments from non-structural full DI are relatively common. These differences are again incentive-driven: Re-assessments to full DI entail non-publicly insured firms no longer having to pay DI benefits or otherwise reintegrate the worker in question. As such, the descriptives indicate that non-public insurers specifically aim at the type of re-assessments that lead to lower DI premiums.

## 5.5 Methodology

This chapter aims to investigate how non-public DI (compared to public DI) affects DI outflow rates, focusing primarily on outflow to work. As the data exhibits censoring, I estimate exponential duration models. [11] I estimate (semi-)parametric hazard rates instead of propor-

[11] OLS and fixed effects models of outflow within a given timeframe yield similar conclusions. Appendix A5.1 shows these estimates.

tional ones as the data exhibit strategic outflow timing, in particular with respect to LGU benefits. Semi-(semi-)parametric and non-(semi-)parametric methods cannot capture this dynamic, as nonparametric methods omit the time horizon. The models estimated impose several assumptions. First, the hazard rate is assumed to be exponential conditional on the included covariates. Second, all dynamics underlying the relative risk of publicly and non-publicly insured employers are included. The second assumption is similar to the assumptions underlying least-square estimation. My baseline model is a hazard rate model in which the transition rate out of DI at duration $t$ conditional on observed characteristics $Z$ is specified as follows:

$$\theta(t|Z) = \lambda(t) \exp(Z'\beta) \tag{5.1}$$

where $\lambda(t)$ is a piecewise constant function representing the pattern of duration dependence (with coefficients for each year after inflow in disability).[12] $\beta$ is a parameter vector, and $Z$ is a vector containing: an indicator for non-public insurance at inflow, control variables, inflow-year, and calendar-year effects. In my basic analysis, I compare outflow on the basis of whether the employer is enrolled in public versus non-public DI insurance at DI inflow[13]. To eliminate potential composition effects between non-public and public insurance, I control for gender, age, sector, labor market area, degree of disability at inflow[14], diagnosis category, and a set of calendar- and inflow-year effects.

Additionally, I estimate models for the following four outflow reasons:

1. Recovery from disability with work
2. Recovery from disability without finding work
3. Outflow to full disability: Re-assessments can lead to full disability benefits. In this case, workers are no longer subject to reintegration obligations.

[12] I cannot estimate duration dependence for individual years after the 10th year of disability, as the data do not contain enough exits due to recovery after the 10th year of disability.

[13] I only show this parameter in the results section.

[14] Measured as the benefit percentage at inflow

4. Remaining outflow reasons such as retirement. I refer to this outflow type as outflow for ‘other’ reasons.

The individual outflow models do not account for the fact that the outflow reasons mentioned are mutually exclusive. Not accounting for this leads to upward-biased estimates. As such, after estimating individual exponential models, I compute total DI outflow by outflow reason while accounting for the risks competing with one another. To this end, I compute outflow rates by insurance status per outflow reason accounting for cumulative incidence. I bootstrap the sample and use Monte Carlo simulation with 100 repetitions to estimate the corresponding standard errors.

I also estimate one more set of equations in which I compare reassessment to a lower degree of disability without outflow. With this set of equations I test the hypothesis that firms and insurers act to reduce the degree of disability of disabled workers even if it does not lead to complete outflow, as this results in lower individual benefit payments.

At first glance adverse selection seems like a concern: firms may opt out as a result of having low DI risk instead of opting out itself lowering DI risk. However, this type of adverse selection is in practice mitigated by moderately-sized and large firms being subject to experience rating. This experience rating leads to long-run public DI premiums roughly equaling the sum of the employer’s DI benefit payments. While smaller employers are subject to a non-experience-rated sector premium, non-public DI insurance providers generally charge this same sector premium for small firms without differentiating with respect to individual DI risk (Cuelenaere et al. (2013)). As such, for a given number of DI recipients, opting out of public insurance will not reduce DI benefit payments in the long run, leaving little scope for adverse selection. Additionally, empirical evidence (i.e. Hassink et al. (2018)) does not find adverse selection on the basis of inflow as a result of this experience rating.

DI premiums can fluctuate and differ between public and non-public DI, creating a remaining threat to inference in the form of dynamic selection on outflow risk. Employers may opt out of public insurance in the short run when expected outflow is temporarily low. Existing literature (i.e. Hassink et al. (2018)) shows dynamic selection on the basis of transitory inflow risk. However, this is less likely to be an issue for outflow, as inflow can be predicted on the basis of sick workers, whereas no such method exists for outflow.

I estimate a second set of hazard rate models for spells during which large firms transitioned from public to non-public insurance before 2017: When large firms made this switch, all the firm's current DI spells also shifted to non-public insurance. Estimating whether outflow rates of spells accelerate after they switch to non-public insurance helps to eliminate adverse selection. In contrast, measuring outflow rates in the year prior to switching provides insight into dynamic selection, as this may capture firms acting based on anticipated short-run outflow rates.

The data allow me to include a wide range of employee- and employer characteristics. However, I cannot include fixed effects in my duration models as the number of firms in my data exceeds the number of exits to work. As such, some endogeneity likely remains. I additionally estimate duration models for employees who switch insurance status due to the reintegrating employer switching from public insurance to non-public insurance during the DI spells. This variation in insurance status eliminates most of the endogeneity in the opt-out decision, as this entails variation for workers and firms that were both publicly and non-publicly insured. I define recovery to work as being re-assessed to having recovered from disability and working at least two consecutive months in the timeframe from two months[15] before recovery to two months after recovery. As an extension, I estimate the same outcome for outflow to recovery with work at the employer at which the worker became disabled. This extension allows me to estimate whether outflow to work is the result of reintegration or of within-firm substitution.

Finally, firms and insurers have an incentive to reduce the degree of disability of disabled workers even if it does not entail outflow, as this results in lower individual benefit payments. To this end, I estimate one more set of cumulative incidence models in which I compare re-assessment to a lower degree of disability without outflow.

[15]Measuring work over a longer timespan roughly yields the same results

## 5.6 Results

This section presents results on outflow to work and computes the effects of non-public DI compared to public DI.

### 5.6.1 Outflow by outflow reason

I start by estimating the hazard rate for total outflow. Afterward, I estimate hazard rates for specific outflow reasons, treating outflow not for that particular reason as censored. These models thus estimate individual risks without accounting for competing risks.

| Dependent variable: DI outflow to... | Total | Work | Recovery without work | Structural DI | Other reasons |
|---|---|---|---|---|---|
| Non-publicly insured at DI inflow | 0.515*** | 0.271*** | 0.577*** | 0.734*** | -0.0260* |
| | (0.0070) | (0.0201) | (0.0220) | (0.00932) | (0.0157) |
| Controls | yes | yes | yes | yes | yes |
| Number of spells | 182,469 | 182,469 | 182,469 | 182,469 | 182,469 |
| Number of exits | 102,594 | 13,154 | 10,129 | 55,920 | 25,646 |

Table 5.3: Piecewise-constant estimates of outflow by outflow reason, not accounting for cumulative incidence. Publicly insured spells form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.3 shows piecewise constant exponential estimates of outflow[16] on the basis of DI insurance types without modeling competing risks[17]. I find significantly positive effects for all outflow reasons except for outflow for other reasons, which has precisely estimated null effects. Estimates of outflow to work are positive, but estimates for recovery without work and especially estimates to structural DI are even higher than outflow to work. These results may be indicative of higher outflow to work among non-publicly insured employers compared to non-publicly insured employers, but may also be an overestimation of the true effects as exponential models do not consider cumulative incidence.

To take into account composition effects in non-public insurance as well as to test for dynamic selection on outflow risk[18], I re-estimate my model for employees that switch insurance status, which, prior to

[16]

[17] I only present the effect of non-public insurance at DI inflow for conciseness. A5.2 contains a full coefficient list.

[18] In appendix A5.3, I also do not find evidence of selection on the basis of inflow risk.

2017, can occur if firms switch insurance status during the employee's disability spell. To this end, I estimate four parameters: First, the baseline effect of non-public DI. Second, I estimate a time-varying dummy that equals 0 before the firm opts out, and 1 after the firm opts out. Third and fourth, I estimate the effect of the year before switching to/from non-public DI. In case dynamic selection is present, the year-prior effects are significantly nonzero.

| Dependent variable: DI outflow to... | Total | Work | Recovery without work | Structural DI | Other reasons |
|---|---|---|---|---|---|
| Non-publicly insured at inflow | 0.461*** | 0.139*** | 0.595*** | 0.648*** | 0.0351 |
| | (0.0160) | (0.0428) | (0.0528) | (0.0215) | (0.0352) |
| Switch to non-public insurance | 0.354*** | 0.104* | 0.356*** | 0.527*** | 0.0846* |
| | (0.0221) | (0.0554) | (0.0775) | (0.0303) | (0.0449) |
| Year before switch to non-public insurance | 0.157** | 0.0238 | 0.145 | 0.338*** | -0.259* |
| | (0.0632) | (0.137) | (0.247) | (0.0834) | (0.148) |
| Year before switch to public insurance | 0.040 | 0.003 | 0.0377 | 0.041 | 0.110 |
| | (0.0330) | (0.0935) | (0.0966) | (0.0408) | (0.0918) |
| Controls | yes | yes | yes | yes | yes |
| Number of spells | 37,415 | 37,415 | 37,415 | 37,415 | 37,415 |
| Number of exits | 27,976 | 3,725 | 2,667 | 16,780 | 5,205 |

Table 5.4: Piecewise-constant estimates of dynamic selection, not accounting for cumulative incidence. Publicly insured spells that did not switch insurance status form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.4 shows that DI outflow rates for non-publicly insured spells by enrollment are higher than for publicly insured DI spells. Outflow seems to increase at the moment of switching in model 1. Short-run outflow effects after switching are present for all outflow reasons. This may be driven by firms immediately becoming more active in requesting re-assessments after switching. I find evidence of dynamic selection for total outflow and outflow to full DI. This dynamic selection for total outflow is likely driven by dynamic selection in full DI, as full DI is the main outflow reason.

However, these estimates do not yet have a direct interpretation since they are measured as hazard rates and do not account for cumulative incidence. To provide a clean interpretation, I predict outflow rates by outflow reason on the basis of Table 5.4 while accounting for cumulative incidence. I utilize the 'switch to non-public insurance' parameter to identify causal effects, as employees who switch insurance status preclude adverse selection by the employee and employer. I compute outflow rates of non-public vs public insurance as the estimated outflow to work if all disabled workers switched from public to non-public insurance during their DI spell, as opposed to outflow rates if all spells are publicly insured, no dynamic selection is present, and none of the disabled workers switched from public to non-public insurance.[19]. I perform the same strategy for outflow to recovery without work, outflow to full disability, and outflow for other reasons. To compute single effects for every year since inflow, I compute the average predicted outflow rates for spells that started between 2006 and 2016[20], accounting for cumulative incidence.

[19]As a sensitivity analysis, I employ an auxiliary identification strategy based on the findings in 5.3. In this approach, I focus on firms that switch insurance status to or from public insurance during the employee's Disability Insurance (DI) spell. This selection aims to mitigate potential long-term adverse selection effects. I then compare two scenarios: one where all DI recipients are publicly insured upon entry versus one where all DI recipients are non-publicly insured upon entry. The predicted outflow rates resulting from this sensitivity analysis are provided in Appendix A5.4. This analysis also allows for the examination of non-switchers, allowing me to disentangle composition effects and causal mechanisms. To match the data used for the main outflow rates, I select on start years prior to 2017 and large firms.

[20]Start years after 2016 are not included as firms switching insurance status after 2016 no longer entails the employee switching insurance status as well.

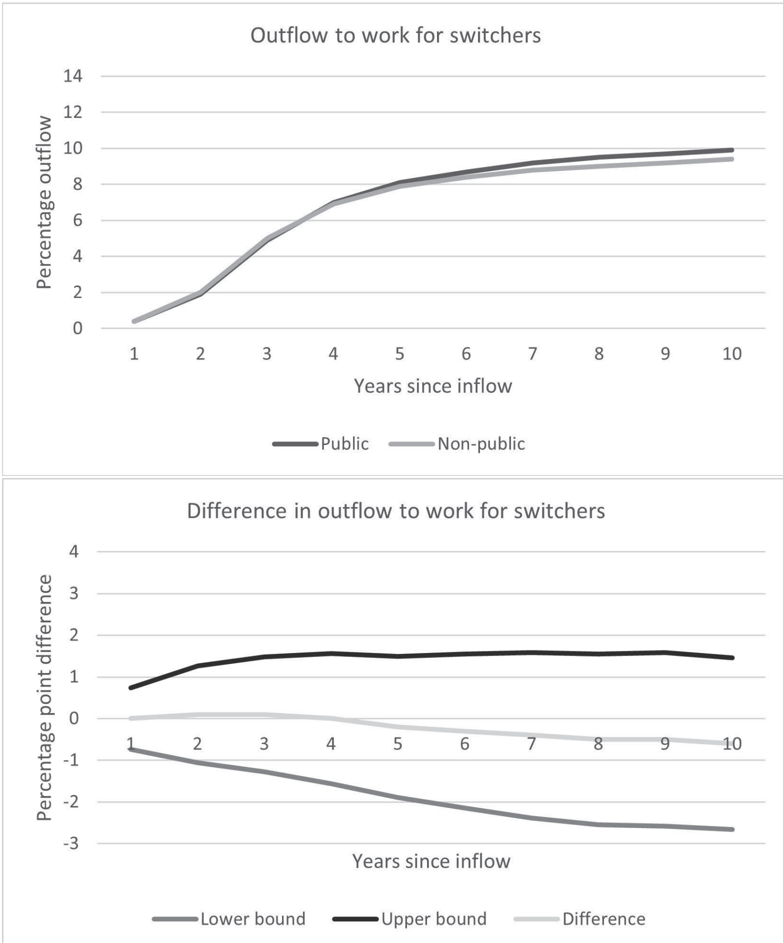


Figure 5.6: Estimated DI outflow to work within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

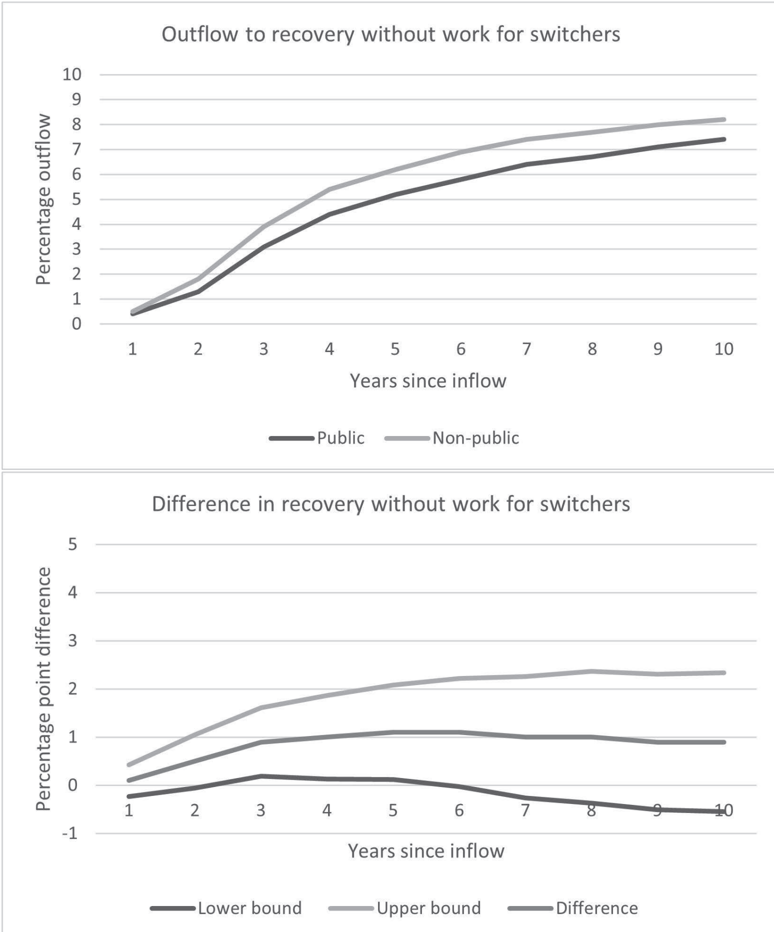


Figure 5.7: Estimated DI outflow to recovery without work within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

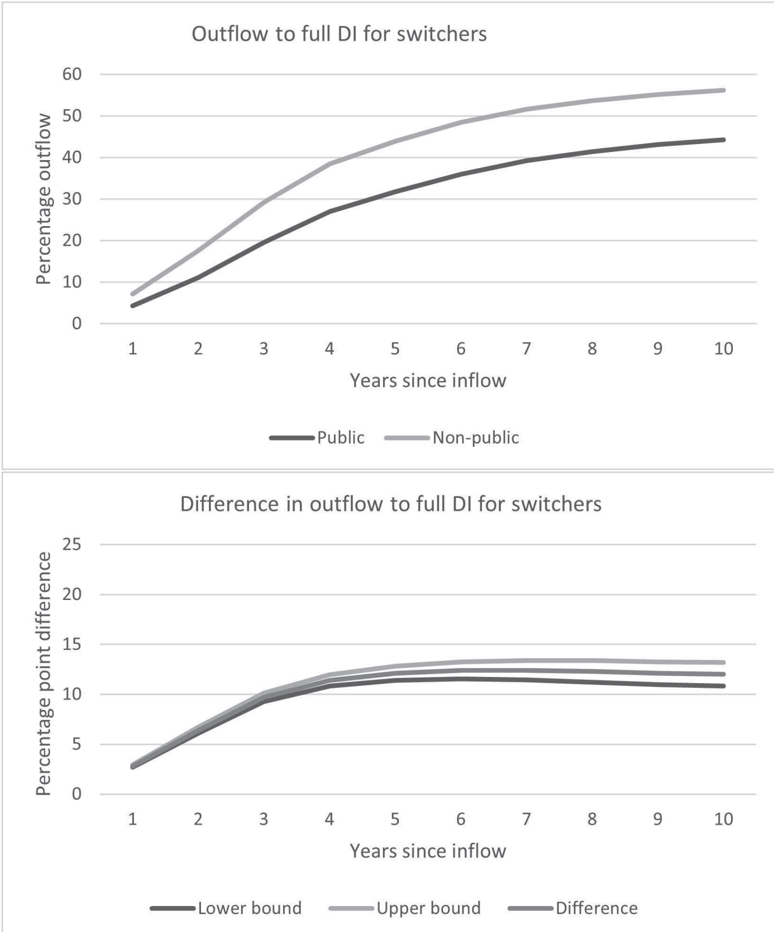


Figure 5.8: Estimated DI outflow to full DI within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

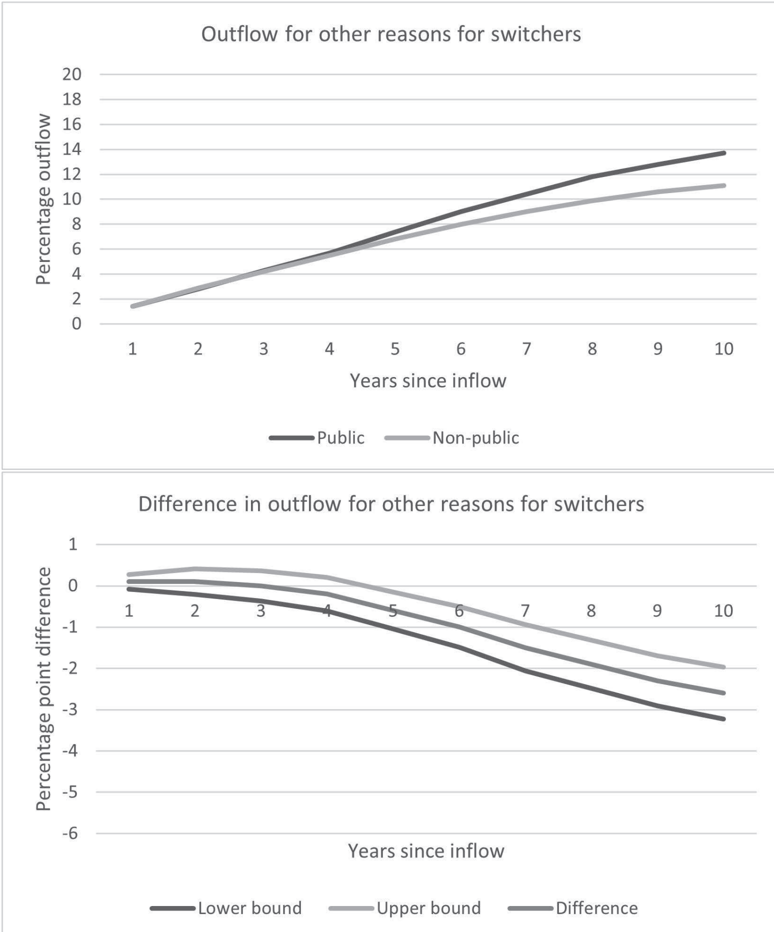


Figure 5.9: Estimated DI outflow for reasons such as retirement within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figures 5.6, 5.7, 5.8, and 5.9 depict estimates of outflow for each of the competing risks in the data. For each of these results, the estimates for switchers capture causal mechanisms, whereas the estimates for non-switchers capture both causal mechanisms and composition effects.

Regarding outflow to work, approximately 10% of spells for end in this manner. Differences on the basis of insurance status, insofar as they exist, are small and statistically insignificant. Consequently, there is no discernible causal evidence suggesting that non-public disability insurance leads to increased outflow to work. Rather, variations appear to be driven by composition effects at the employee and employer levels.

Examining outflow to recovery without work, 8% of non-publicly insured switchers and 7% of all publicly insured spells among switchers end in this manner. In the first three to five years of the disability spell, outflow to recovery without work for non-publicly insured spells is significantly higher than for publicly insured spells. However, these differences are no longer statistically significant after 5 years.

For outflow to full DI, 45% of publicly insured spells and roughly 55% of publicly insured switchers transition to full DI. These differences are large and statistically significant, suggesting that outflow to full DI is driven by a blend of causal mechanisms and composition effects.

Lastly, outflow for other reasons, such as retirement, accounts for 14% of publicly insured spells and 11% for non-publicly insured spells. Once more, the differences are significantly nonzero, indicating the presence of both causal mechanisms and composition effects. The difference in outflow for other reasons is likely driven by spells ending in full DI before individuals retire.

### 5.6.2 Outflow to Work by Current or Different Cmployer

Outflow to work can manifest in two forms. First, disabled workers can work elsewhere. Second, workers can find work at their existing employer. Non-publicly insured employers likely have more resources available to facilitate the latter outflow. To estimate this, I estimate a second set of duration models. I separate outflow to work based on whether the former DI recipient finds work with the employer they became disabled at and at a different employer from the one they became disabled at, respectively.

| Dependent variable: DI outflow to... | Work at same employer | Work at different employer |
|---|---|---|
| Non-publicly insured at DI inflow | 0.343*** | 0.227*** |
| | (0.0313) | (0.0263) |
| Controls | yes | yes |
| Number of spells | 182,469 | 182,469 |
| Number of exits | 5,497 | 7,657 |

Table 5.5: Piecewise-constant estimates of outflow by outflow reason, not accounting for cumulative incidence. Publicly insured spells form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5.5 displays estimates of outflow to work on the basis of DI insurance types at the same employer. The number of exits in Table 5 is higher than in Table 4 as the outcome measure estimated now also includes work at a different employer. The outflow rate for employees returning to work at the same employer where they fell ill is higher among non-publicly insured employers than among publicly insured employers. These effects may be indicative of substitution: Firms that opt-out of public DI may create jobs for disabled workers within their own firms, as opposed to finding work elsewhere. Note that estimates for full DI and other reasons are identical to those reported in Table 5.3, as the spells in the first two columns of Table 5.3 sum up to the same spells as the spells in the first two columns of Table 5.5.

I re-estimate the dynamic selection model using outflow to work at the same employer as an outcome measure instead. Selection effects may differ here as employers may be able to create jobs within their own firms, creating a stronger basis for dynamic selection.

| Dependent variable: DI outflow to... | Work at same employer | Work at different employer |
|---|---|---|
| Non-publicly insured at inflow | 0.116* | 0.160*** |
| | (0.0600) | (0.0612) |
| Switch to non-public insurance | 0.206*** | -0.0488 |
| | (0.0727) | (0.0859) |
| Year before switch to non-public insurance | 0.216 | -0.756** |
| | (0.151) | (0.342) |
| Year before switch to public insurance | 0.0701 | -0.0624 |
| | (0.119) | (0.153) |
| Controls | yes | yes |
| Number of spells | 37,415 | 37,415 |
| Number of exits | 2,006 | 1,719 |

Table 5.6: Piecewise-constant estimates of dynamic selection, not accounting for cumulative incidence. Publicly insured spells that did not switch insurance status form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.6 demonstrates higher outflow rates to work at the same employer among non-publicly insured DI spells than publicly insured DI spells. Furthermore, in the year of switching to non-public insurance, outflow to work at the same employer increases. In contrast, no effects of switching are present for work at different employers, and there is even evidence of negative dynamic selection on this type of outflow. These findings complement the estimates in Table 5.5 and Table 5.4. Not only is the effect of outflow to work at the same employer larger than the effect of outflow to work in general, there are also short-run effects after switching to non-public insurance in finding work at the same employer not present for work resumption in general. This may be incentive-driven: Non-publicly insured employers have incentives to themselves create employment opportunities for the workers they re-integrate. I do not observe any outflow-based anticipation effects when factoring in controls. The results suggest that while outflow among non-publicly insured employers is higher than among publicly insured employers. These differences are likely driven by non-publicly insured employers both creating jobs within their own firms and re-assessing disabled workers more actively immediately after (or even before) switching.

### 5.6.3 Re-assessments to a lower degree of disability

I also compute the percentage of spells re-assessed to a lower degree of disability, while accounting for cumulative incidence resulting from outflow from DI.

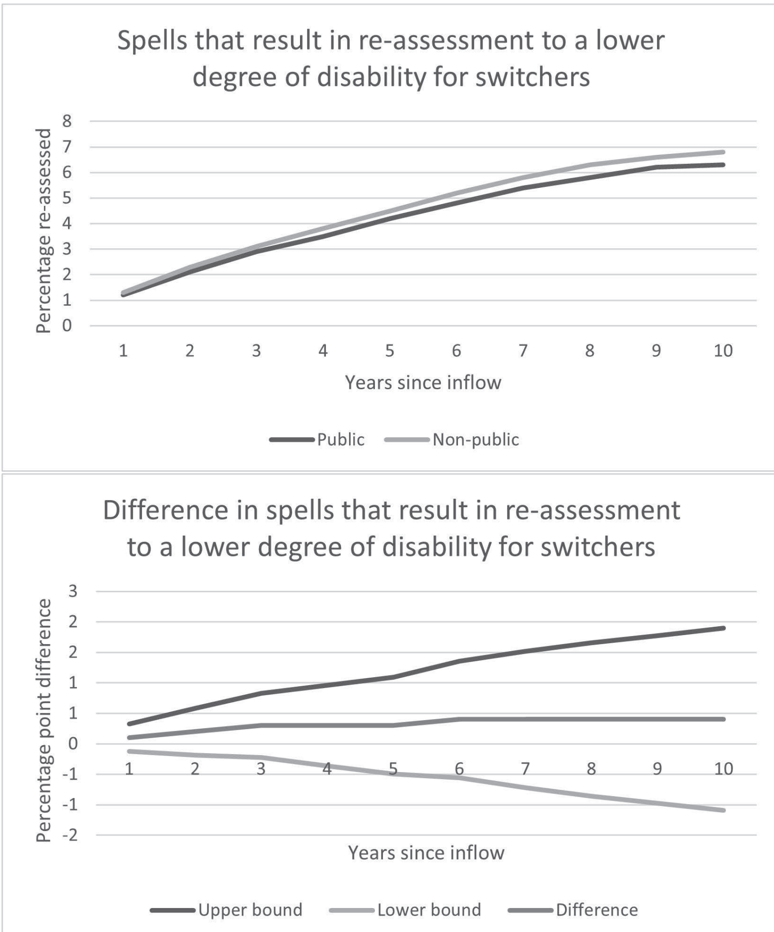


Figure 5.10: Estimated re-assessments to a lower degree of disability during the DI spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

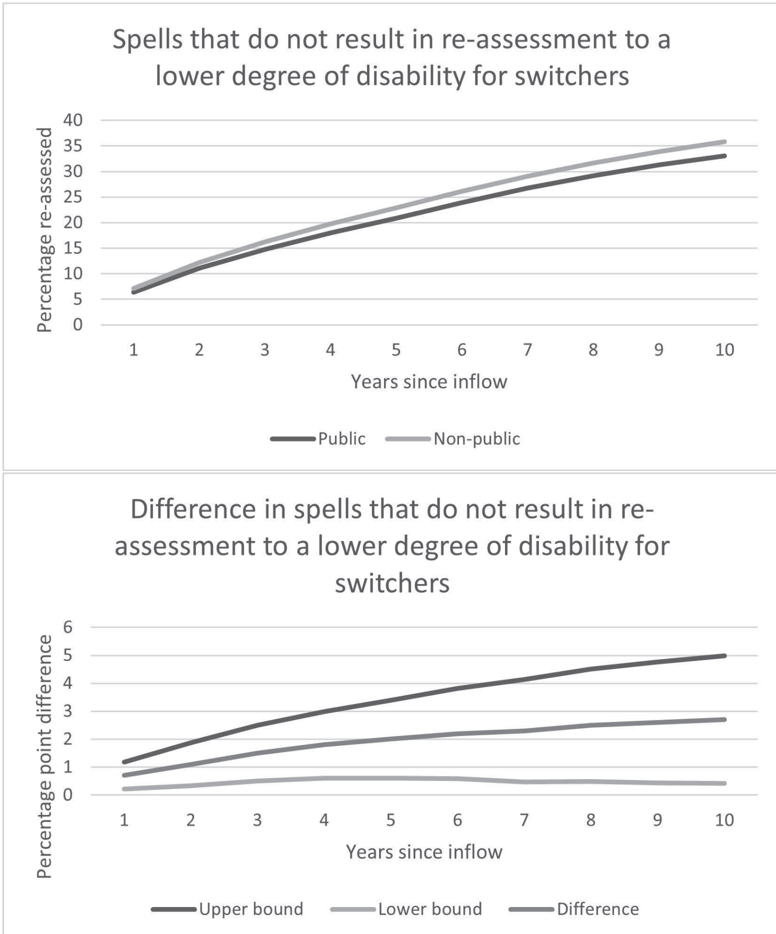


Figure 5.11: Estimated spells that do not result in re-assessments to a lower degree of disability during the DI spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figure 5.10 illustrates the predicted percentage of spells that result in re-assessments to a lower degree of disability, with spell that do not result in a re-assessment to a lower degree of disability as a competing risk. Figure 5.11 illustrates the opposite. I do not discover evidence that non-publicly insured spells are re-assessed to a lower degree of disability more often, but do find evidence of more non-publicly insured spells ending without a re-assessment to a lower degree of disability.

### 5.6.4 Re-assessments

The differences in outflow seem mainly driven by active re-assessment on behalf of non-public insurers. I test for this by estimating the number of re-assessments on the basis of insurance status. To this end, I estimate the number of reassessments by reassessment type that took place in the sample using both Ordinary Least Squares (OLS) and (firm) Fixed Effects (FE) models, using the same controls as in the previous estimates. These models use the number of re-assessments in the sample as the dependent variable.

| | Dependent variable | | | | | |
|---|---|---|---|---|---|---|
| | Partial to Temp DI | Temp to Partial DI | Temp/Partial to Recovery | Partial to Struct DI | Temp to Struct DI | Total |
| **OLS estimates** | | | | | | |
| Non-public DI | 1,051*** | 3,766*** | 7,296*** | 1,605*** | 14,102*** | 27,820*** |
| | (228) | (278) | (334) | (190) | (486) | (786) |
| Observations | 180,253 | 180,253 | 180,253 | 180,253 | 180,253 | 180,253 |
| **Fixed Effects estimates** | | | | | | |
| Non-public DI | 132 | 2,345*** | 5,098*** | 766** | 8,949*** | 17,290*** |
| | (402.2) | (499.6) | (550.7) | (342.7) | (828.6) | (1,246) |
| Number of firms | 59,304 | 59,304 | 59,304 | 59,304 | 59,304 | 59,304 |
| Observations | 180,253 | 180,253 | 180,253 | 180,253 | 180,253 | 180,253 |

Table 5.7: Estimates of number of DI spells successfully Re-assessed in the sample, separated by re-assessment type. Estimated with OLS and FE. FE estimates include firm-fixed effects. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Partial to temporary measures re-assessments from partial DI to full DI. Temporary to partial measures re-assessments from temporary full DI to partial DI. Temporary/partial to recovery measures re-assessments from non-structural DI to recovery. Partial to structural DI measures re-assessments from partial DI to structural full DI. Temporary to structural measures re-assessments from temporary full DI to structural full DI. Firm-level clustered standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.7 shows OLS and FE estimates of re-assessed individuals in the sample. I find no differences in re-assessments from partial DI to temporary full DI after controlling for fixed effects. However, I discover a higher number of re-assessments among the remaining four types, which are all forms of re-assessments that lower DI payments for non-publicly insured firms. Two mechanisms drive these results. First, there exists a higher degree of activity in re-assessments among non-publicly insured firms, and may indicate non-publicly insured firms more effectively monitoring and requesting re-assessments than publicly insured firms, leading to updates in disabled workers' health status being captured more swiftly. Second, re-assessments requested by private parties are prioritized by the Dutch Employee Insurance agency, and thus processed more swiftly.

All in all, I conclude few to no differences in outflow to work, but find large differences in outflow to full DI and a small decrease of one's degree of disability as a consequence of the opting-out decision of the employer. This is indicative that the higher outflow to work among non-publicly insured firms is a composition effect, but there may be causal effects present in the other outflow types. The outflow to full DI may be a result of spells with no prospect of recovery ending, but may also indicate incentives to shift benefit recipients to the public system. The causal differences in outflow between public and non-public differences are likely driven by more active re-assessments for non-publicly insured spells.

## 5.7 Conclusion

In this chapter, I estimate how non-public DI insurance affects outflow to work using a rich administrative dataset from the Dutch Employee Insurance Agency. In doing so, I estimate piecewise constant duration models, and predict outflow from DI by outflow reason while accounting for cumulative incidence. The competing risks here include recovery without work, outflow to full disability, and outflow for other reasons. The data allow me to control for a rich set of employee- and employer-characteristics, limiting the scope for endogeneity.

I find differences in outflow rates to work among non-publicly insured workers, but these differences are entirely driven by composition effects: after eliminating these composition effects, no statistically significant differences in outflow to work are present. However, I find non-selection-based differences in recovery without work. These results suggest that while non-public disability insurance does lead to re-assessments to lower degrees of disability, this does not entail more outflow to work.

There are also large differences in outflow to full disability and outflow for reasons such as retirement: Outflow to full disability is higher among non-publicly insured employers, whereas outflow for reasons such as retirement is lower among non-publicly insured employers. This effect is likely driven by spells ending in full DI before individuals reach the point of retirement.

Finally, I estimate re-assessments to a lower degree of disability in addition to the aforementioned outflow effects. I do not discover causal evidence of differences in the number of re-assessments to a lower degree of disability for non-public insurance. These findings may indicate stronger rehabilitation efforts on both the extensive margin as opposed to the intensive margin.

My findings indicate that outflow is primarily driven by direct exits from partial and partial and temporary disability rather than mechanical channels such as retirement. Non-public insurers may be more efficient at rehabilitating disabled workers, and may create jobs for disabled workers within their firms. However, most of the outflow in my findings is to structural disability.

The outflow results are primarily driven by differences in re-assessments: Re-assessments are more common in non-publicly insured firms. Particularly outflow to full disability exhibits differences between publicly and non-publicly insured firms in the form of re-assessments from temporary full disability to full disability. Smaller but nonetheless remarkable differences are also present in re-assessments from temporary full disability to partial disability. The types of re-assessments non-public parties realize are those that reduce DI premiums for themselves, indicating that incentives to request re-assessments may facilitate more effective reintegration. However, this may also lead to fewer re-assessments for workers who experience an increase in their non-structural degree of disability.

The results have several welfare implications. First, reintegration efforts among firms that opt out of public insurance are partially driven by composition effects, as the decision to opt out is an outcome of the firm's risk profile and options to reintegrate disabled workers. Second, the full DI outflow differences may be beneficial for disabled workers, as they no longer have to reintegrate and receive higher benefits. However, this also entails these workers exiting the labor market altogether. The additional outflow to full DI also entails higher overall public DI benefit payments. Differences in re-assessments to full disability create the follow-up question of whether workers re-assessed to full disability had any remaining recovery potential. However, I cannot compute welfare effects as I do not observe the counterfactual outcome in case of workers not being re-assessed, and I do not observe individuals after they exit DI.

For future research, investigating the mechanisms behind re-assessments may be of interest: While I observe more re-assessments and can to some degree correct for selection in this phenomenon, I cannot investigate the process that leads to these re-assessments being more common. Further investigating workers after they exit DI on the basis of insurance type may be of interest for future research.

All in all, I do not find causal evidence of outflow to work on the basis of non-public DI provision. However, I discover differences in other outflow types, with differences in outflow to full DI and outflow for reasons such as retirement being especially large, the latter due to spells ending as a result of re-assessments before the aforementioned individuals reach the retirement age. These findings are driven by non-public insurers requesting re-assessments more actively, leading to DI exits through both lower degrees of disability and higher degrees of disability, with the latter being most prevalent. This phenomenon primarily stems from non-public firms being more active in shifting workers with no recovery potential out of partial DI.

# A5 Appendices

## A5.1 OLS and FE estimates of outflow

| | Timeframe (years) | | | | | |
|---|---|---|---|---|---|---|
| **Variables** | **1** | **2** | **3** | **4** | **5** | **6** |
| **Outflow to Work** | | | | | | |
| OLS: Non-public insurance | 23.44 | 44.71 | 349.5** | 268.4 | 187.2 | 135.8 |
| | (34.18) | (75.53) | (145.1) | (170.0) | (180.2) | (187.6) |
| OLS: Constant | -495.3 | -2,866*** | -4,947*** | -5,262*** | -5,367*** | -5,329*** |
| | (439.1) | (662.0) | (896.7) | (1,190) | (1,414) | (1,443) |
| FE: Non-public insurance | 21.67 | 59.00 | 171.6 | 177.4 | 208.4 | 142.6 |
| | (57.22) | (213.6) | (253.1) | (264.6) | (288.4) | (299.7) |
| FE: Constant | -180.7 | -2,028*** | -3,179** | -2,580* | -3,192** | -2,817* |
| | (635.6) | (784.4) | (1,242) | (1,538) | (1,579) | (1,626) |
| **Outflow to Recovery without Work** | | | | | | |
| OLS: Non-public insurance | -22.02 | 311.1** | 1,220*** | 1,400*** | 1,466*** | 1,495*** |
| | (53.29) | (126.1) | (235.2) | (277.9) | (296.8) | (309.1) |
| OLS: Constant | -205.0 | -3,758*** | -6,108*** | -6,968*** | -7,746*** | -7,639*** |
| | (651.4) | (929.2) | (1,299) | (1,544) | (1,741) | (1,861) |
| FE: Non-public insurance | 95.39 | 369.5* | 1,053*** | 1,410*** | 1,615*** | 1,717*** |
| | (84.11) | (224.0) | (288.5) | (319.8) | (348.9) | (361.6) |
| FE: Constant | 471.1 | -2,113* | -2,978* | -2,388 | -3,242 | -2,445 |
| | (865.5) | (1,085) | (1,767) | (2,070) | (2,138) | (2,310) |
| **Outflow to Full DI** | | | | | | |
| OLS: Non-public insurance | 1,489*** | 4,912*** | 8,501*** | 10,087*** | 10,887*** | 11,391*** |
| | (233.3) | (485.8) | (717.3) | (782.6) | (828.0) | (859.8) |
| OLS: Constant | 6,768*** | 16,601*** | 22,149*** | 26,463*** | 28,327*** | 28,845*** |
| | (1,256) | (2,010) | (2,444) | (2,746) | (2,886) | (2,973) |
| FE: Non-public insurance | 1,262*** | 4,467*** | 7,003*** | 8,034*** | 8,606*** | 8,631*** |
| | (218.3) | (387.3) | (477.0) | (521.3) | (564.6) | (599.9) |
| FE: Constant | 8,918*** | 21,317*** | 27,861*** | 33,057*** | 35,513*** | 36,259*** |
| | (1,506) | (2,288) | (2,537) | (2,596) | (2,745) | (2,856) |
| **Outflow to Other** | | | | | | |
| OLS: Non-public insurance | -328.7*** | -729.4*** | -1,148*** | -1,683*** | -2,136*** | -2,582*** |
| | (81.26) | (120.4) | (150.8) | (178.5) | (203.1) | (221.6) |
| OLS: Constant | 7,432*** | 16,060*** | 23,308*** | 31,215*** | 38,516*** | 42,651*** |
| | (1,243) | (1,471) | (1,518) | (1,665) | (1,723) | (1,744) |
| FE: Non-public insurance | -82.35 | -159.3 | -248.5 | -433.3* | -518.1** | -647.3** |
| | (123.4) | (182.2) | (208.1) | (227.9) | (243.0) | (258.1) |
| FE: Constant | 6,553*** | 13,947*** | 20,559*** | 27,857*** | 34,511*** | 38,909*** |
| | (1,412) | (1,602) | (1,715) | (1,897) | (1,947) | (1,991) |

Table A5.1: OLS and Fixed effects estimates of outflow within a given timeframe. Clustered standard errors in parentheses. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

Table A5.1 shows OLS and Fixed Effects estimates of the number of individuals flowing out of DI within the sample by outflow reason within 1, 2, 3, 4, 5, and 6 years after inflow, using the same controls as in the rest of the paper. OLS and Fixed Effects estimates generally do not reveal additional outflow to work as a result of non-public DI. Recovery without work, meanwhile, is substantially higher among non-publicly insured spells than among publicly insured spells, with approximately 10000 more non-publicly insured spells ending in full DI for non-publicly insured as compared to publicly insured ones. Outflow for other reasons, meanwhile, is lower, though FE models find smaller differences. The estimates in Table A5.1 roughly match the effects I find in my cumulative incidence estimates.

## A5.2 Full coefficient list of main estimates

| Dependent variable: Outflow to: | Total | Work | Recovery without work | Full DI | Other |
|---|---|---|---|---|---|
| Non-public insurance | 0.515*** | 0.271*** | 0.577*** | 0.734*** | -0.0260* |
| | (0.00702) | (0.0201) | (0.0220) | (0.00932) | (0.0157) |
| Benefit %: 28 | -0.359*** | 0.977*** | 0.0504 | -0.976*** | 0.00681 |
| | (0.0111) | (0.0251) | (0.0343) | (0.0180) | (0.0208) |
| Benefit %: 35 | -0.397*** | 0.659*** | -0.269*** | -0.841*** | 0.0253 |
| | (0.0118) | (0.0291) | (0.0415) | (0.0178) | (0.0210) |
| Benefit %: 42 | -0.369*** | 0.567*** | -0.286*** | -0.715*** | -0.0142 |
| | (0.0138) | (0.0337) | (0.0480) | (0.0203) | (0.0253) |
| Benefit %: 50.75 | -0.325*** | 0.483*** | -0.371*** | -0.587*** | -0.0212 |
| | (0.0134) | (0.0349) | (0.0510) | (0.0189) | (0.0248) |
| Benefit %: 70 | -0.135*** | 0.117*** | 0.0631 | -0.123*** | -0.0895*** |
| | (0.0134) | (0.0367) | (0.0392) | (0.0186) | (0.0257) |
| Labor market area: 00 | 0.0614** | -0.0546 | 0.232*** | 0.0748** | 0.0554 |
| | (0.0260) | (0.0689) | (0.0873) | (0.0351) | (0.0524) |
| Labor market area: 01 | 0.135*** | 0.0980 | 0.0802 | 0.211*** | -0.0231 |
| | (0.0324) | (0.0867) | (0.111) | (0.0429) | (0.0693) |
| Labor market area: 02 | 0.189*** | 0.317*** | 0.420*** | 0.184*** | 0.0711 |
| | (0.0344) | (0.0888) | (0.114) | (0.0466) | (0.0711) |
| Labor market area: 03 | 0.0727** | -0.0649 | -0.00200 | 0.178*** | -0.0449 |
| | (0.0341) | (0.0946) | (0.119) | (0.0447) | (0.0715) |
| Labor market area: 05 | 0.0982*** | 0.0341 | 0.162* | 0.122*** | 0.00594 |
| | (0.0291) | (0.0769) | (0.0968) | (0.0390) | (0.0604) |
| Labor market area: 06 | 0.0461 | -0.0324 | 0.0718 | 0.0744* | 0.00427 |
| | (0.0289) | (0.0773) | (0.0973) | (0.0386) | (0.0594) |
| Labor market area: 07 | 0.0436 | 0.0547 | 0.0919 | 0.0444 | 0.0269 |

| | | | | | |
|---|---|---|---|---|---|
| | (0.0320) | (0.0841) | (0.107) | (0.0431) | (0.0659) |
| Labor market area: 08 | -0.0493 | -0.0859 | 0.0131 | -0.0207 | -0.113 |
| | (0.0355) | (0.0973) | (0.120) | (0.0473) | (0.0725) |
| Labor market area: 09 | 0.0126 | 0.0574 | 0.128 | -0.0227 | 0.0269 |
| | (0.0342) | (0.0917) | (0.114) | (0.0461) | (0.0690) |
| Labor market area: 10 | -0.00169 | 0.00362 | 0.0907 | -0.0297 | 0.00748 |
| | (0.0386) | (0.104) | (0.130) | (0.0523) | (0.0784) |
| Labor market area: 11 | -0.0443 | 0.0724 | 0.318*** | -0.166*** | 0.0318 |
| | (0.0310) | (0.0802) | (0.0988) | (0.0428) | (0.0617) |
| Labor market area: 12 | -0.0685* | 0.0882 | 0.244* | -0.232*** | 0.0150 |
| | (0.0397) | (0.103) | (0.126) | (0.0564) | (0.0759) |
| Labor market area: 13 | -0.134*** | -0.103 | 0.00504 | -0.201*** | -0.0140 |
| | (0.0288) | (0.0751) | (0.0949) | (0.0394) | (0.0575) |
| Labor market area: 14 | -0.0898*** | 0.0461 | 0.199* | -0.227*** | 0.0931 |
| | (0.0333) | (0.0847) | (0.105) | (0.0463) | (0.0658) |
| Labor market area: 15 | 0.0597** | -0.0130 | -0.0435 | 0.0955** | 0.0307 |
| | (0.0288) | (0.0764) | (0.0992) | (0.0387) | (0.0591) |
| Labor market area: 17 | 0.0382 | -0.0177 | -0.0699 | 0.0891** | -0.0681 |
| | (0.0312) | (0.0829) | (0.108) | (0.0417) | (0.0651) |
| Labor market area: 18 | -0.222*** | -0.288*** | 0.176** | -0.386*** | 0.0449 |
| | (0.0272) | (0.0715) | (0.0885) | (0.0375) | (0.0536) |
| Labor market area: 19 | -0.0500 | -0.0237 | 0.210** | -0.175*** | 0.109* |
| | (0.0316) | (0.0837) | (0.103) | (0.0440) | (0.0615) |
| Labor market area: 20 | -0.0821* | -0.0424 | 0.172 | -0.219*** | 0.168* |
| | (0.0475) | (0.125) | (0.146) | (0.0678) | (0.0891) |
| Labor market area: 21 | -0.120*** | -0.211*** | 0.182** | -0.222*** | 0.0520 |
| | (0.0283) | (0.0757) | (0.0924) | (0.0386) | (0.0562) |
| Labor market area: 22 | -0.0343 | -0.327*** | 0.164* | -0.0223 | 0.0113 |
| | (0.0275) | (0.0748) | (0.0912) | (0.0371) | (0.0554) |
| Labor market area: 23 | -0.0464 | -0.261*** | -0.193* | 0.0330 | -0.0496 |
| | (0.0341) | (0.0940) | (0.116) | (0.0451) | (0.0711) |
| Labor market area: 24 | 0.0616* | -0.0152 | 0.0163 | 0.0859** | 0.0525 |
| | (0.0324) | (0.0848) | (0.110) | (0.0436) | (0.0666) |
| Labor market area: 25 | 0.0633** | -0.0710 | 0.294*** | 0.0790** | -0.0203 |
| | (0.0294) | (0.0787) | (0.0958) | (0.0397) | (0.0605) |
| Labor market area: 26 | -0.133*** | -0.250*** | 0.261*** | -0.264*** | 0.0494 |
| | (0.0307) | (0.0834) | (0.0977) | (0.0425) | (0.0601) |
| Labor market area: 27 | -0.0580* | -0.265*** | 0.114 | -0.0684* | 0.00569 |
| | (0.0304) | (0.0836) | (0.0991) | (0.0411) | (0.0612) |
| Labor market area: 28 | -0.129*** | -0.216** | 0.161 | -0.202*** | -0.0131 |
| | (0.0312) | (0.0841) | (0.100) | (0.0427) | (0.0623) |
| Labor market area: 29 | -0.193*** | -0.243** | 0.257** | -0.424*** | 0.0815 |
| | (0.0367) | (0.103) | (0.114) | (0.0537) | (0.0668) |
| Labor market area: 30 | 0.0734*** | -0.302*** | -0.207** | 0.224*** | -0.0580 |
| | (0.0279) | (0.0788) | (0.0971) | (0.0370) | (0.0579) |
| Labor market area: 32 | -0.142*** | -0.169* | 0.0975 | -0.243*** | -0.0385 |
| | (0.0353) | (0.0954) | (0.114) | (0.0490) | (0.0694) |
| Labor market area: 33 | -0.0853*** | -0.376*** | -0.276** | -0.0400 | 0.0227 |
| | (0.0328) | (0.0956) | (0.118) | (0.0436) | (0.0653) |

| | | | | | |
|---|---|---|---|---|---|
| Labor market area: 34 | -0.0883*** | -0.149* | 0.0901 | -0.161*** | 0.0869 |
| | (0.0332) | (0.0892) | (0.110) | (0.0454) | (0.0643) |
| Labor market area: 35 | -0.0454 | -0.101 | -0.00358 | -0.0109 | -0.0640 |
| | (0.0674) | (0.183) | (0.209) | (0.0904) | (0.140) |
| Labor market area: 36 | 0.0639** | -0.0138 | -0.00312 | 0.0806* | 0.0174 |
| | (0.0310) | (0.0823) | (0.107) | (0.0417) | (0.0632) |
| Labor market area: 37 | -0.0918*** | -0.134 | 0.0184 | -0.152*** | 0.0212 |
| | (0.0319) | (0.0856) | (0.106) | (0.0435) | (0.0634) |
| Sector: Agriculture/nutrition | 0.112 | 0.671*** | 0.744*** | -0.0752 | -0.128 |
| | (0.0736) | (0.247) | (0.272) | (0.0976) | (0.135) |
| Sector: Construction/wood | 0.0672 | 0.501** | 0.465* | -0.0785 | -0.153 |
| | (0.0737) | (0.247) | (0.274) | (0.0977) | (0.135) |
| Sector: Industry | 0.107 | 0.782*** | 0.679** | -0.1000 | -0.111 |
| | (0.0727) | (0.244) | (0.269) | (0.0963) | (0.133) |
| Sector: Retail | 0.144** | 0.592** | 0.781*** | -0.0772 | 0.0135 |
| | (0.0726) | (0.244) | (0.269) | (0.0963) | (0.133) |
| Sector: Transport | 0.174** | 0.964*** | 0.692** | -0.0623 | -0.0315 |
| | (0.0732) | (0.245) | (0.271) | (0.0970) | (0.134) |
| Sector: Healthcare | 0.328*** | 1.023*** | 0.809*** | 0.171* | -0.0834 |
| | (0.0725) | (0.244) | (0.269) | (0.0960) | (0.133) |
| Sector: Government education | 0.146** | 0.957*** | 0.397 | -0.151 | -0.0356 |
| | (0.0736) | (0.246) | (0.274) | (0.0981) | (0.134) |
| Sector: Government, other | 0.154** | 1.010*** | 0.128 | -0.0584 | -0.0416 |
| | (0.0733) | (0.245) | (0.274) | (0.0973) | (0.134) |
| Sector: Financial services | 0.115 | 0.848*** | 0.563** | -0.120 | -0.0445 |
| | (0.0728) | (0.244) | (0.269) | (0.0965) | (0.133) |
| Sector: Temporary work | 0.141* | 0.498** | 1.091*** | -0.126 | 0.0124 |
| | (0.0757) | (0.253) | (0.273) | (0.101) | (0.140) |
| Sector: Other | 0.165** | 0.326 | 0.941*** | -0.0301 | -0.0246 |
| | (0.0728) | (0.246) | (0.269) | (0.0964) | (0.133) |
| Diagnosis type: Unknown | 0.315*** | 1.129*** | 1.324*** | -0.331*** | 0.710*** |
| | (0.0155) | (0.0419) | (0.0458) | (0.0246) | (0.0277) |
| Diagnosis type: Heart disease | 0.172*** | 0.697*** | -0.00721 | -0.0764*** | 0.507*** |
| | (0.0122) | (0.0344) | (0.0565) | (0.0164) | (0.0234) |
| Diagnosis type: Cancer | -0.0122 | -0.0550 | -0.0445 | -0.110*** | 0.160*** |
| | (0.0135) | (0.0480) | (0.0639) | (0.0177) | (0.0250) |
| Diagnosis type: Psychological | -0.297*** | 0.0455* | 0.356*** | -0.524*** | -0.0870*** |
| | (0.00930) | (0.0264) | (0.0327) | (0.0121) | (0.0202) |
| Diagnosis type: Musculoskeletal | -0.134*** | 0.211*** | 0.491*** | -0.315*** | -0.0324 |
| | (0.00942) | (0.0277) | (0.0343) | (0.0122) | (0.0201) |
| Age category: $\leq 30$ | -1.214*** | 2.348*** | 1.472*** | -1.066*** | -3.607*** |
| | (0.0171) | (0.0727) | (0.0655) | (0.0281) | (0.0429) |
| Age category: 31 - 40 | -1.217*** | 2.034*** | 1.285*** | -0.813*** | -3.649*** |

| | | | | | |
|---|---|---|---|---|---|
| | (0.0123) | (0.0698) | (0.0616) | (0.0190) | (0.0289) |
| Age category: 41 - 50 | -1.127*** | 1.625*** | 0.864*** | -0.410*** | -3.854*** |
| | (0.0109) | (0.0694) | (0.0614) | (0.0164) | (0.0262) |
| Age category: 51-60 | -0.796*** | 0.879*** | 0.310*** | -0.105*** | -2.263*** |
| | (0.00979) | (0.0700) | (0.0621) | (0.0153) | (0.0166) |
| Male | 0.114*** | 0.0320 | -0.138*** | 0.124*** | 0.227*** |
| | (0.00740) | (0.0210) | (0.0238) | (0.0101) | (0.0146) |
| Years disabled: <1 | 0.0549 | -0.550*** | 0.314 | 1.446*** | -1.781*** |
| | (0.0625) | (0.196) | (0.214) | (0.0917) | (0.106) |
| Years disabled: 1-2 | 0.650*** | 0.886*** | 1.237*** | 1.883*** | -1.467*** |
| | (0.0575) | (0.178) | (0.198) | (0.0848) | (0.0978) |
| Years disabled: 2-3 | 1.056*** | 1.696*** | 1.890*** | 2.130*** | -1.269*** |
| | (0.0526) | (0.163) | (0.182) | (0.0778) | (0.0897) |
| Years disabled: 3-4 | 0.940*** | 1.412*** | 1.704*** | 1.956*** | -1.036*** |
| | (0.0480) | (0.149) | (0.167) | (0.0711) | (0.0816) |
| Years disabled: 4-5 | 0.757*** | 1.024*** | 1.328*** | 1.687*** | -0.743*** |
| | (0.0434) | (0.135) | (0.152) | (0.0646) | (0.0735) |
| Years disabled: 5-6 | 0.712*** | 0.836*** | 1.221*** | 1.569*** | -0.458*** |
| | (0.0389) | (0.122) | (0.137) | (0.0582) | (0.0656) |
| Years disabled: 6-7 | 0.633*** | 0.731*** | 1.033*** | 1.386*** | -0.269*** |
| | (0.0348) | (0.111) | (0.125) | (0.0524) | (0.0579) |
| Years disabled: 7-8 | 0.508*** | 0.499*** | 0.908*** | 1.142*** | -0.186*** |
| | (0.0314) | (0.103) | (0.114) | (0.0477) | (0.0509) |
| Years disabled: 8-9 | 0.391*** | 0.381*** | 0.824*** | 0.943*** | -0.212*** |
| | (0.0287) | (0.0982) | (0.105) | (0.0443) | (0.0457) |
| Years disabled: 9-10 | 0.238*** | 0.337*** | 0.506*** | 0.678*** | -0.209*** |
| | (0.0277) | (0.0965) | (0.106) | (0.0434) | (0.0423) |
| Inflow year: 2006 | 0.753*** | -0.333 | 0.0995 | 1.287*** | 0.923*** |
| | (0.0813) | (0.302) | (0.268) | (0.117) | (0.141) |
| Inflow year: 2007 | 0.801*** | -0.342 | 0.140 | 1.454*** | 0.770*** |
| | (0.0773) | (0.291) | (0.254) | (0.111) | (0.134) |
| Inflow year: 2008 | 0.669*** | -0.170 | 0.203 | 1.199*** | 0.753*** |
| | (0.0724) | (0.280) | (0.237) | (0.103) | (0.126) |
| Inflow year: 2009 | 0.583*** | -0.259 | 0.106 | 1.072*** | 0.688*** |
| | (0.0678) | (0.269) | (0.222) | (0.0963) | (0.119) |
| Inflow year: 2010 | 0.484*** | -0.290 | 0.0683 | 0.895*** | 0.620*** |
| | (0.0633) | (0.258) | (0.206) | (0.0895) | (0.112) |
| Inflow year: 2011 | 0.420*** | -0.308 | 0.00256 | 0.806*** | 0.505*** |
| | (0.0586) | (0.248) | (0.191) | (0.0825) | (0.104) |
| Inflow year: 2012 | 0.331*** | -0.239 | 0.00704 | 0.661*** | 0.354*** |
| | (0.0540) | (0.238) | (0.175) | (0.0757) | (0.0962) |
| Inflow year: 2013 | 0.256*** | -0.213 | -0.0316 | 0.513*** | 0.325*** |
| | (0.0498) | (0.230) | (0.160) | (0.0693) | (0.0890) |
| Inflow year: 2014 | 0.179*** | -0.170 | -0.129 | 0.387*** | 0.239*** |
| | (0.0459) | (0.222) | (0.147) | (0.0634) | (0.0831) |
| Inflow year: 2015 | 0.169*** | -0.167 | -0.0340 | 0.335*** | 0.193** |
| | (0.0421) | (0.215) | (0.134) | (0.0574) | (0.0775) |
| Inflow year: 2016 | 0.105*** | -0.164 | -0.145 | 0.243*** | 0.142** |
| | (0.0387) | (0.208) | (0.122) | (0.0520) | (0.0718) |

| | | | | | |
|---|---|---|---|---|---|
| Inflow year: 2017 | 0.0788** | -0.0342 | -0.0700 | 0.175*** | 0.150** |
| | (0.0357) | (0.203) | (0.111) | (0.0473) | (0.0667) |
| Inflow year: 2018 | 0.0318 | -0.00109 | -0.0247 | 0.119*** | 0.0767 |
| | (0.0332) | (0.199) | (0.101) | (0.0432) | (0.0629) |
| Inflow year: 2019 | 0.0610* | 0.235 | 0.0210 | 0.0972** | 0.129** |
| | (0.0313) | (0.196) | (0.0928) | (0.0402) | (0.0597) |
| Inflow year: 2020 | 0.0111 | -0.0900 | 0.479*** | -0.0468 | 0.0495 |
| | (0.0313) | (0.205) | (0.0856) | (0.0399) | (0.0596) |
| Calendar year: 2007 | -0.713*** | 3.385*** | -0.105 | -1.585*** | -0.300** |
| | (0.0827) | (0.420) | (0.263) | (0.120) | (0.145) |
| Calendar year: 2008 | -0.766*** | 4.865*** | -0.990*** | -1.652*** | -0.304** |
| | (0.0756) | (0.376) | (0.247) | (0.110) | (0.134) |
| Calendar year: 2009 | -0.866*** | 4.550*** | -1.095*** | -1.706*** | -0.341*** |
| | (0.0701) | (0.367) | (0.230) | (0.102) | (0.123) |
| Calendar year: 2010 | -0.928*** | 4.435*** | -1.505*** | -1.659*** | -0.399*** |
| | (0.0650) | (0.359) | (0.215) | (0.0941) | (0.114) |
| Calendar year: 2011 | -0.960*** | 4.289*** | -1.528*** | -1.665*** | -0.321*** |
| | (0.0599) | (0.351) | (0.198) | (0.0868) | (0.104) |
| Calendar year: 2012 | -0.697*** | 4.952*** | -1.240*** | -1.477*** | -0.260*** |
| | (0.0543) | (0.342) | (0.179) | (0.0789) | (0.0949) |
| Calendar year: 2013 | -0.535*** | 4.804*** | -1.044*** | -1.125*** | -0.223*** |
| | (0.0491) | (0.335) | (0.162) | (0.0710) | (0.0858) |
| Calendar year: 2014 | -0.341*** | 4.703*** | -0.844*** | -0.789*** | -0.142* |
| | (0.0440) | (0.329) | (0.144) | (0.0634) | (0.0770) |
| Calendar year: 2015 | -0.114*** | 4.517*** | -0.534*** | -0.464*** | 0.0869 |
| | (0.0389) | (0.323) | (0.127) | (0.0561) | (0.0681) |
| Calendar year: 2016 | -0.106*** | 4.306*** | -0.733*** | -0.259*** | -0.174*** |
| | (0.0342) | (0.318) | (0.112) | (0.0491) | (0.0605) |
| Calendar year: 2017 | -0.0494* | 4.559*** | -0.680*** | -0.174*** | -0.177*** |
| | (0.0296) | (0.314) | (0.0959) | (0.0425) | (0.0524) |
| Calendar year: 2018 | 0.0667*** | 4.581*** | -0.796*** | 0.0267 | -0.0760* |
| | (0.0253) | (0.310) | (0.0822) | (0.0361) | (0.0451) |
| Calendar year: 2019 | 0.0986*** | 4.722*** | -0.751*** | 0.0836*** | -0.0895** |
| | (0.0217) | (0.307) | (0.0685) | (0.0306) | (0.0394) |
| Calendar year: 2020 | 0.121*** | 4.696*** | -0.843*** | -0.0126 | 0.252*** |
| | (0.0185) | (0.305) | (0.0568) | (0.0264) | (0.0329) |
| Calendar year: 2021 | 0.103*** | 4.593*** | -0.958*** | -0.0455** | 0.330*** |
| | (0.0163) | (0.303) | (0.0480) | (0.0231) | (0.0291) |
| Constant | -2.148*** | -12.27*** | -7.042*** | -3.913*** | -0.829*** |
| | (0.100) | (0.472) | (0.359) | (0.138) | (0.181) |

Table A5.2: Full parameter list of the models estimated in Table 5.3. Baseline categories are a benefit percentage of 75, labor market area 38, sector 'unknown', diagnosis code 'other', age 61 years or older, female, disabled for 10 or more years, inflow year 2021, and calendar year 2022. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A5.2 presents all coefficients of the models estimated in Table 5.3. Individuals with lower benefit percentages at inflow find work more often, and experience less outflow to full DI. Labor market area differences are present, but do not have a clean interpretation. Outflow to work is less prevalent in the 'other' category than in other sectors. Individuals with unknown disease types, heart disease, and psychological ailments experience different outflow patterns from other diagnosis categories. Younger individuals recover (irrespective of employment) more often, and experience less outflow to full DI and reasons such as retirement. Men experience more outflow to full DI and retirement. Strong duration dependence patterns are present: outflow to work is more common early in the DI spell, whereas outflow for full DI and reasons such as retirement becomes more common as spells last longer. Later inflow years results in less outflow to full DI and reasons such as retirement. Finally, earlier calendar years entail more outflow to work, but less outflow to full DI and reasons such as retirement.

### A5.3 Selection on inflow risk

One remaining question entails whether there is dynamic selection based on inflow risk in DI. While this is not a threat to inference for my estimates, as I investigate outflow risk, selection on the basis of inflow risk may nonetheless inform policy. To this end, I construct the total number of spells starting in every year-month combination, and estimate how switching to and from non-public insurance affects inflow within one year with OLS and fixed effects models. These estimates are as follows:

| Estimates of inflow-based dynamic selection | | | | |
|---|---|---|---|---|
| Dependent variable: | Amount of new DI spells per month | | Amount of new DI spells per month | |
| | Model 1 | Model 2 | Model 1 | Model 2 |
| | OLS | | FE | |
| Non-publicly insured at inflow | 9.879*** | 1.310*** | 38.19*** | 3.408*** |
| | (1.782) | (0.480) | (3.351) | (0.905) |
| Within 1 year of switch to non-public | -64.91*** | 1.757 | -21.99*** | -0.574 |
| | (2.334) | (1.530) | (2.223) | (1.380) |
| Within 1 year of switch to public | -8.511*** | -3.335** | -0.773 | -2.853* |
| | (2.800) | (1.614) | (2.631) | (1.647) |
| Constant | 515.7*** | 452.3*** | 505.4*** | 688.7*** |
| | (1.127) | (4.607) | (1.187) | (6.081) |
| Controls | no | yes | no | yes |

Table A5.3: Estimates of inflow-based dynamic selection. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A5.3 shows estimates of dynamic selection based on DI inflow risk, with estimates within 1 year of switching indicating this type of selection, using the number of new spells as the dependent variable and using the same controls as in the rest of the paper. I do not find selection into non-public DI when inflow is low conditional on the controls used. Some weak evidence of lower inflow when switching to public insurance is present, but the order of the magnitude of these estimates are very small compared to baseline DI inflow, ranging from 3 to 9 fewer new DI spells per month. As such, I do not find meaningful inflow-based dynamic selection.

## A5.4 Outflow rates on the basis of firm-switching

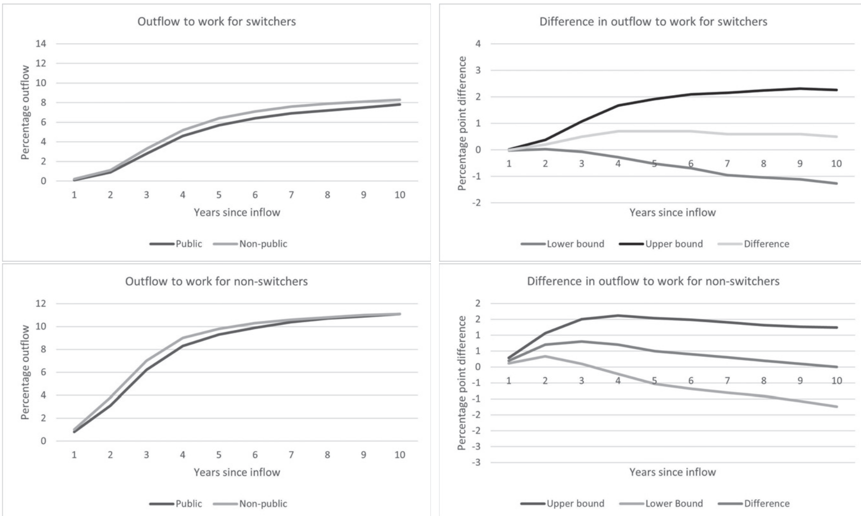


Figure A5.1: Estimated outflow to work separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

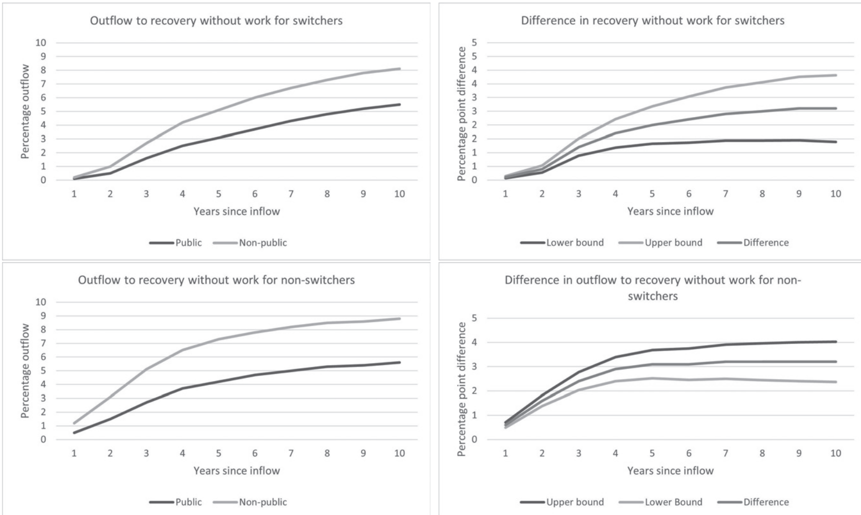


Figure A5.2: Estimated outflow to recovery without work separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

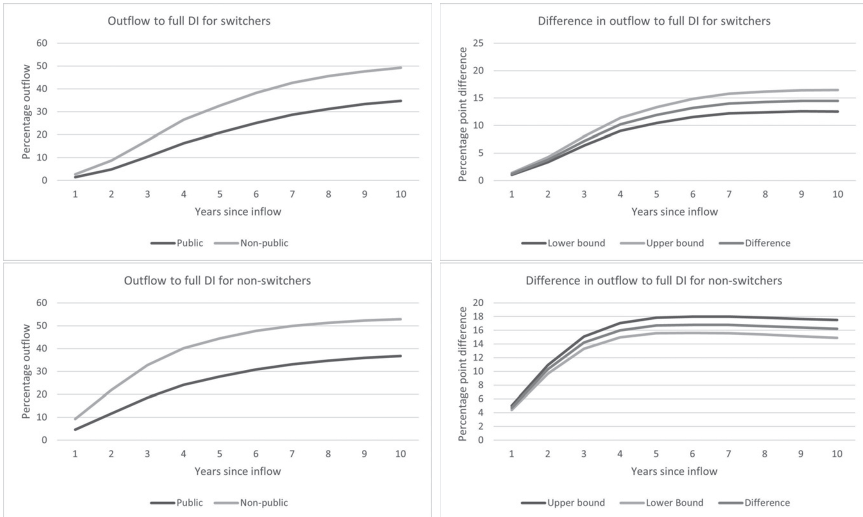


Figure A5.3: Estimated outflow to full DI separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

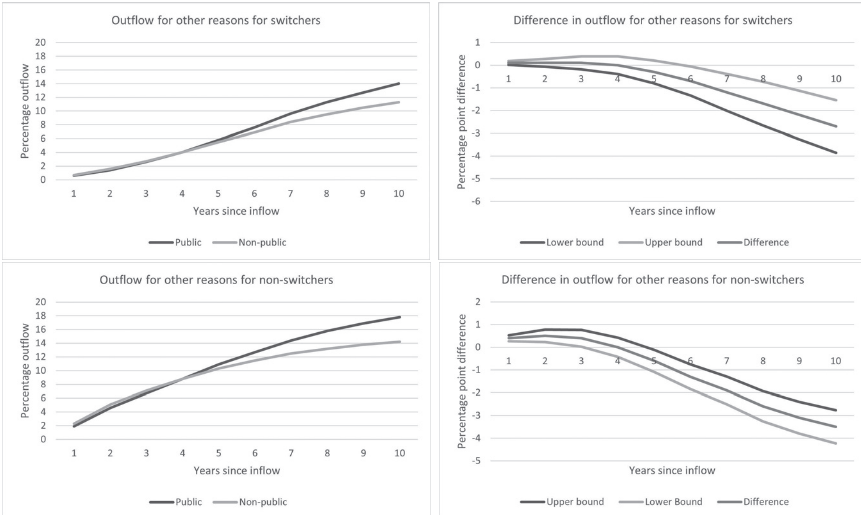


Figure A5.4: Estimated outflow for other reasons such as retirement separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figures A5.1, A5.2, A5.3, and A5.4 illustrate outflow to work, recovery without work, full DI, and other reasons, respectively. After controlling for firm characteristics and accounting for cumulative incidence, I find no differences in outflow to work based on insurance status, although the outflow to work among non-switchers is higher than for switchers. Both groups - switchers and non-switchers - demonstrate higher outflow rates to recovery without work than switchers. Differences on the basis of insurance status are similar for switchers and non-switchers. Outflow to full DI is significantly higher for non-public firms compared to public firms, with the difference being roughly 15 percentage points. This difference is more pronounced for non-switchers, suggesting residual composition effects. Finally, outflow for other reasons such as retirement is lower for non-publicly insured firms for both switchers and non-switchers. However, the differences are again larger for non-switchers. In summary, examining firms that switch insurance status at some point in the sample as opposed to examining spells that switch to non-public insurance provides roughly equal, albeit more pronounced differences on the basis of insurance status.

As a result, differences in outflow to recovery without work are larger and significantly nonzero. Otherwise, estimates match those of figures 5.6, 5.7, 5.8, and 5.9.