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The effect of trial periods in employment on firm hiring behavior

Abstract

Recent changes in New Zealand law decreased the cost of dismissing employees within their first 3 months with an employer, with the aim of encouraging firms to increase hiring by reducing the associated risk. We use monthly linked employer–employee data and exploit the staggered introduction of the policy to estimate its effect on hiring. We find that the policy had little effect on the number of hires, the hiring of jobseekers of unknown quality, or the stability of employment. Our results suggest that policies that temporarily lower dismissal costs do not necessarily increase firm hiring.

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1 Introduction

A reduction in the cost to a firm of dismissing an employee may affect hiring behavior via two main channels. First, it gives firms more flexibility to respond to external shocks; firms may hire until they have the optimal amount of labor, knowing their employees can be cheaply dismissed if demand for their output decreases. Second, it reduces the cost to the firm when a new employee turns out to be a poor match for the job. This cost reduction could make firms more willing to hire new workers, especially those about whom they have less information. Most previous literature looking at the effects of dismissal costs combines the two channels. We isolate the importance of the matching channel by using a natural experiment in New Zealand which reduced dismissal costs for up to 90 days after hiring only, and estimate the effect of the law change on firm hiring behavior.

In 2009, the New Zealand Government passed an amendment that introduced 90-day trial periods for new employees, with the goal of stimulating employment and increasing opportunities for disadvantaged jobseekers. From March 1, 2009, firms with fewer than 20 employees could hire consenting new employees on a trial basis. Within a trial period, which can last up to 90 days, fewer legal requirements must be met to dismiss an employee. Although we cannot definitively prove that trial periods reduce dismissal costs, we argue that the wording of the law and survey evidence strongly suggest this to be the case. The policy was deemed a success, and trial-period eligibility was extended to all firms on April 1, 2011.

The policy changes present a natural experiment, where only firms below the 20-employee threshold had access to trial periods between the two policy changes. We use this discontinuity in eligibility to form a treatment group of firms just below the threshold, and a control group of firms just above the threshold, and run a difference-in-differences (DID) estimate of the impact of the legislation on firm hiring behavior. Both sizes of firm around the threshold were affected similarly by changes in economic conditions such as the Global Financial Crisis (GFC), and so any difference in their change in hiring behavior can be attributed to the policy. We use the second policy change as a placebo test; this eliminated any difference in eligibility between treatment and control firms, meaning that any difference in behavior that was caused by trial period policy should disappear.

To motivate our empirical analysis, we develop a simple theoretical model of how the availability of trial periods affects firm hiring behavior. In the model, a firm decides whether to hire an applicant whose impact on firm profit is not perfectly observed by the firm until after the hire is made. We compare a world without trial period policy, in which hiring decisions cannot be reversed, and a world with trial period policy. In the latter, a firm that hires the worker can hire her with or without a trial period, and if she is hired with a trial period then the firm can costlessly dismiss her after one period, when her impact on profit is known. Since trial period legislation reduces the potential downside of hiring an employee, the model predicts that firms will make more hires in the world with trial periods. It also predicts a shift toward applicants whose effect on profit is known with less certainty and who could include those with little recent job market experience such as recent beneficiaries and graduates. Trivially, the model predicts that the stability of employment relationships will be lower with trial periods.

To test the predictions of this model, we use the staggered introduction of trial periods in New Zealand and monthly administrative data containing the population of employing firms

and employees from 2005 to 2014. We use a DID strategy to estimate the impact on the number of new hires by firms, the types of people hired, and the duration of new employment relationships. We focus on the effect of firms that are permitted to use trial periods, as opposed to the effect of firms actually using them, due to data limitations and because the former is more relevant from a policy perspective.

We find no evidence that access to trial periods causes firms on average to change the number of people they hire, nor to be more likely to hire those struggling in the labor market, such as recent beneficiaries, recent migrants, or young people of Māori or Pasifika ethnicity. These estimated policy effects are close to zero and precisely estimated. Furthermore, we find no evidence that trial-period eligibility increases short-term hiring or makes workers reluctant to change jobs. We also find little evidence of heterogeneous effects on the number or types of hires when considering the prevalence of trial period use in a firm's industry; the prevalence of permanent contracts in a firm's industry; the prevalence of employer-funded training in a firm's industry; the seasonality of a firm's industry; and recent firm growth.

We test whether trial periods replaced temporary contracts as a means of trialing new employees; if this were the case, the policy change would not have substantially lowered dismissal costs. We find that the employee and job characteristics associated with temporary contracts are negatively associated with trial period contracts, inconsistent with this hypothesis.

Overall our results suggest that New Zealand trial periods allow firms to benefit from reduced costs associated with hiring and dismissals without changing their behavior, while jobseekers may bear increased perceived uncertainty about their job security while on a trial period. Although caution must be taken in extrapolating our findings to other settings, our findings indicate that a temporary decrease in dismissal costs does not necessarily encourage firms to hire more, as we might expect based on firms' improved ability to screen applicants. One potential explanation, consistent with prior literature, is that job insecurity causes employees to exert more effort during the trial period, rendering trials ineffectual at informing employers about the long-term productivity of their new hires.[1]

One potential concern is endogenous selection into the treatment group, wherein some firms that would have 20 or more employees absent the policy limit their size to below the 20-employee cutoff to be eligible to make their next hire with a trial period. We believe endogenous firm size is unlikely to be an issue for two reasons. First, it seems implausible that the benefit of remaining eligible to hire a future employee on a trial period would be great enough to cause a firm to shed staff whom it would otherwise retain or to refrain from making a desired hire in the present. Second, the data suggest no decrease in hiring just below the firm size cutoff, and our results are robust to excluding firms near the cutoff and to defining the treatment group based on firms' pre-policy size.

Most prior literature on employment protection tests whether a permanent change in the ease of dismissal affects the labor market through either of the two channels described earlier. In the United States, several studies analyze the differential introduction of worker-protection laws in different states and show that such laws may increase outsourcing and temporary work (Autor, 2003); decrease the employment-to-population ratio (Autor et al., 2006); and decrease

1 We used Survey of Working Life data to test whether employees in their first 90 days in a job at a firm that was eligible to use trial periods reported greater difficulties than other employees due to long work hours, as a measure of effort, but statistical power was too low to draw any conclusion.

employment flows (Autor et al., 2007). Other studies suggest that dismissal costs decrease employment flows in Colombia (Kugler, 1999) and Italy (Kugler and Pica, 2008). More broadly, Lazear (1990) and Botero et al. (2004) argued that countries with higher employment protection have reduced employment and, as a result, labor force participation.

Kugler and Saint-Paul (2004) focused on the effect of dismissal costs on the type of workers hired, and argued that the unemployed in the United States are less likely to be re-employed due to employment protection, relative to the already employed. Relatedly, Acemoglu and Angrist (2001) found that employment protection targeted at disabled people in the United States may decrease their employment.

In contrast, von Below and Thoursie (2010) found that Swedish relaxation of employment protection for small firms had no effect on hiring or separations, suggesting that firms use other methods to get around such restrictive laws. Similarly, Bauer et al. (2007) used variation in worker-protection laws by time and firm size in Germany and found no effect on employment flows. They suggested that the costs to firms of the worker-protection laws may have been small, or that perhaps firms adjust the hours worked by their current employees, rather than changing the number of employees.

The only study we are aware of that isolates the match quality channel was by Martins (2009). He used a natural experiment in Portugal which gave small firms more freedom than larger firms to dismiss workers for a cause. Hence, any change in hiring behavior would emerge by lowering firms' expected costs when hiring an employee of unknown quality or fit. Martins found little robust evidence of an impact on worker flows, which he noted may be masked by the annual data, but he found that firm performance increases possibly because employees increase their effort. Similarly, we found little impact on hiring, and in our setting such an impact could not be obscured by data aggregation because we observe employment spells regardless of duration.

Related studies focused on the effects of employment protection on firm productivity, with mixed findings. Autor et al. (2007) found tentative evidence that employment protection increases capital deepening and decreases productivity among US firms, while Boeri and Garibaldi (2007) found a similar negative effect on productivity in Italy. On the other hand, Jahn et al. (2012) highlighted that employment protection may increase productivity by encouraging firms to invest in the human capital of their employees. Such mechanisms, whether positive or negative, are likely to be less important in our setting, where the decreased worker protection is only for 90 days.

A similar strand of research looks at the effects of employment protection on worker effort. Riphahn (2004), Olsson (2009) and Ichino and Riphahn (2005) found evidence that employment protection increases worker absenteeism which is a driver of lower firm productivity. The impacts of employment protection legislation differ by contract type: as Engellandt and Riphahn (2005) showed, worker effort increases the most for those with contracts that allow upward mobility. Our results are consistent with these findings in that if employees work harder during their trial periods then these trials might not reveal true employee productivity to their employers. However, we are unable to test directly for increased worker effort during trial periods.

This literature suggests that the effects of permanent changes in the costs of dismissal depend on the context and the avoidance tactics available to firms; in some circumstances, a legislated decrease in dismissal costs may not ease constraints for firms as much as expected.

Previous research into trial periods in New Zealand consists of three government reports based on surveys.[2] This research shows that many firms use trial periods and appreciate the increased flexibility; that over half of employing firms had hired someone on a trial period in the previous year or two; that employers use trial periods to gain information about the quality of a candidate; and that one-third of employers say they hired someone on a trial period whom they otherwise would not have hired (DOL, 2010; DOL, 2012; MBIE, 2014).[3]

The rest of the article is organized as follows: Section 2 gives background on trial periods in New Zealand; Section 3 motivates the analysis with a simple theoretical model of firm hiring behavior; Section 4 describes the data used; Section 5 presents the empirical strategy and results from the econometric analysis; and Section 6 concludes.

2 Background

2.1 Introduction of trial periods

Trial periods were introduced as part of the government's response to the Global Financial Crisis and consequent weak economic conditions in New Zealand. In the context of the slowing growth and rising unemployment seen in late 2008, the Minister of Labour described trial periods for small and medium firms as a way of lowering the risks employers face, creating jobs and getting struggling jobseekers into the labor market.[4] The Employment Relations Amendment Act 2008 was passed under urgency in December 2008.[5] It introduced trial periods for firms with fewer than 20 employees and came into effect on March 1, 2009. The extension to firms of all sizes of eligibility to use trial periods was announced in July 2010 and came into effect on April 1, 2011.[6,7]

Section 67A of the Employment Relations Act 2000, which was added in 2009, describes a trial provision in an employment agreement as follows:

> (2) **Trial provision** means a written provision in an employment agreement that states, or is to the effect, that—
>
> (a) for a specified period (not exceeding 90 days), starting at the beginning of the employee's employment, the employee is to serve a trial period;

2 In addition, NZIER (2011) uses a DID strategy with aggregate data and finds trial periods increased total jobs and hiring. In the working paper version of this article (http://www.treasury.govt.nz/publications/research-policy/wp/2016/16-03/twp16-03.pdf), we show that their finding is driven by the inclusion of very large firms (far from the 20-employee threshold) in their control group. Such firms were especially negatively affected by the Global Financial Crisis and therefore do not provide a good counterfactual for what would have happened to small firms without trial period policy.

3 Our finding that trial period availability had no significant effect on firm hiring may appear at odds with the survey finding that one-third of employers report hiring someone on a trial period whom they otherwise would not have hired. However, we believe that data on employers' actual behaviour are likely to more accurately capture firm behavior than do employer survey responses to this question for two reasons. First, employers who like having the option of using trial periods may shade their survey responses to make trial periods seem more beneficial. This is especially easy to do when the survey question involves comparison with an unobservable counterfactual (their hiring decision had trial periods not been available). Second, employers are likely to genuinely not know what hires they would have made in the counterfactual world.

4 See the December 11, 2008 media release: https://www.national.org.nz/news/news/media-releases/detail/2008/12/11/90-day-trial-period-to-provide-job-opportunities.

5 http://www.lawlink.co.nz/articles.php?articleid=131

6 https://www.beehive.govt.nz/release/90-day-trial-period-extended-all-employers

7 The 2010 Department of Labour evaluation was used as evidence that the policy was a success.

(b) during that period the employer may dismiss the employee; and
(c) if the employer does so, the employee is not entitled to bring a personal grievance or other legal proceedings in respect of the dismissal.

A trial period must be specified in writing in the contract, which can be permanent or fixed term, and must be agreed by both parties and signed before the employee begins work. Importantly, trial periods may be used for employees who have not previously been employed by the firm only.

2.2 **Dismissing without versus with a trial period**

Trial period policy is expected to affect firm hiring only if firms believe that trial periods genuinely make dismissing an employee easier. Although data do not exist which could definitely show that trial periods decrease dismissal costs, this subsection describes the legal requirements for dismissal with and without a trial period, and argues that trial periods lower the bar for dismissal. The following subsection discusses firms' *beliefs* about how trial periods affect dismissal costs.

In New Zealand, dismissing an employee who is not currently on a trial period can be slow, costly, and risky for the employer. This applies equally if the employee was hired before her firm was eligible to use trial periods, her employment contract does not include a trial period provision, or she is beyond the first 90 days of her employment.[8]

A dismissal without a trial period must meet two standards of fairness. First, it must be substantively fair, meaning that there was a valid reason for dismissal. Reasons for dismissal can generally be grouped into serious misconduct that justifies summary dismissal and less serious misconduct. Serious misconduct might include behavior such as fighting, direct disobedience, or dishonesty, and less serious misconduct might include behavior such as absenteeism, unsatisfactory work performance, or using abusive language.[9] Second, a dismissal must be procedurally fair. The test, as laid out in section 103A of the Employment Relations Act, is whether the employer acted as a "fair and reasonable employer could have done." Before taking action against or dismissing an employee, an employer is expected to sufficiently investigate the allegations; communicate the concerns to the employee; give the employee clear standards to meet and a genuine opportunity to improve and meet the required standards (except in case of serious misconduct); and consider any relevant explanations of the employee.[10] The employer must also follow any procedures laid out in the employment contract.

If a dismissed employee feels he was let go unfairly ("unjustifiably," in the terminology of the Employment Relations Act 2000), he can raise a personal grievance against his former employer. If the parties are unable to resolve the grievance between them, the next step is mediation, followed by the Employment Relations Authority (ERA). If either party is unsatisfied

8 For context, we note that employment protection in New Zealand is low relative to the OECD average, and that this was true even before trial period policy was introduced (OECD 2015).

9 http://communitylaw.org.nz/community-law-manual/chapter-18-resolving-employment-problems/personal-grievances-chapter-18/

10 See the Employment Relations Act 2000 for details: http://legislation.govt.nz/act/public/2000/0024/latest/DLM60322.html as at October 16, 2015, and http://employment.govt.nz/er/solvingproblems/resolving/dismissal.asp for an interpretation.

with the ERA's determination, it may make an appeal to the Employment Court.[11] This can mean months of costly court battles for the employer, and potentially payments of lost wages and damages to the employee. As a result, it can be time-consuming and risky for an employer to dismiss an unsatisfactory or underperforming worker.

The main purpose of the trial period provision is to remove a dismissed employee's right to raise a personal grievance based on unjustified dismissal. This removes a great deal of the risk to the employer associated with dismissal of a new employee, and thus reduces the risk of hiring a person whose fit for the job is imperfectly known. If a new employee is underperforming or a bad fit, or if a new position within the firm turns out to be unnecessary, the employee can be let go without the risk of court battles and legal costs. Dismissal can also be substantially faster in a trial period, because the employee need not be given behavior or performance goals and the opportunity to improve and meet them before it can occur. However, trial periods do not give employers the right to "fire at will." Good faith principles still apply; the employer must have a reason for dismissal; and processes stated in the employment contract must be adhered to.

At times, a firm may wish to dismiss an employee when external shocks lessen demand for its products. Legally, this is not a valid reason for dismissal. Trial periods increase the ease of dismissing employees in such circumstances, but only to the extent that the firm has recent hires still within their trial periods.

Even prior to the introduction of trial periods, firms could use "probationary periods" to test the match quality of new employees. Probationary periods are significantly weaker than trial periods, whereas trial periods in New Zealand are akin to what are called probationary periods in other countries. With a New Zealand probationary period, employers are not immune to personal grievances based on unjustified dismissal. The only increase in flexibility comes from the uncertain hope that employers will be held to lower standards in legal disputes if a dismissed employee was on a probationary period.[12] Firms can also hire temporary workers, who made up around 10% of the New Zealand workforce in 2008 and 2012.[13] By law, temporary contracts must not be used to test the suitability of an employee. However, some firms may have used them for this purpose and then switched to trial periods when they were made available. We test the substitution of 90-day trials for temporary contracts in Section 5.4.

2.3 **Employers' and employees' views on trial periods**

We would expect trial period policy to have a measurable effect only if firms know about trial periods and use them. Survey evidence shows that firms generally know about trial periods and understand their basic nature. A year after trial periods were first introduced, 74% of surveyed employers knew that employees must consent to trial periods and 70% knew that employees retain protection against discrimination and harassment (DOL, 2010); knowledge about trial periods is likely to be even higher among the 59% of firms that report using them (MBIE, 2014). Despite trial periods not being a "get out of jail free" card for employers, survey and interview

11 http://www.findlaw.co.nz/articles/4296/unjustified-dismissal.aspx

12 See https://employment.govt.nz/starting-employment/trial-and-probationary-periods/probation-periods/ for details. Note that there is no legal limit for the length of probationary periods, and they can be used for employees switching roles with the same employer.

13 See the Statistics NZ Survey of Working Life 2008 and Survey of Working Life 2012.

evidence shows that employers view trial period policy as substantially reducing the cost and risk of dismissal, and therefore of taking on a new employee (DOL, 2010; DOL, 2012; MBIE, 2014). For example, in one survey 79% of employers reported using trial periods to check an employee's suitability for the job before making a commitment (DOL, 2010), which suggests that they believe that trial periods lower the costs of dismissal.

Further supporting the argument that firms' perceived dismissal costs are lower when using trial periods, Table 1 presents the distribution of contract type for new hires in 2012, using nationally representative survey data. It shows that 37.7% of new hires are permanent roles with a trial period, 41.7% are permanent roles without a trial period, and 20.7% are temporary roles. This highlights the prevalence of trial periods, and hence their value to firms.

Thus there is a reason to believe that trial period policy could have changed hiring behavior on a large scale.

Surveys report less about employees' views on trial periods. Qualitative interviews show employees lacked in-depth knowledge of trial periods a year after they were brought in, though employees did understand the basic idea: trial periods are for employers to judge their suitability for the role and make dismissal much easier (DOL, 2010). An important lesson is that employees generally do not view trial periods as negotiable, but rather consider job offers to be conditional on accepting them, meaning that their only alternative is to walk away.

2.4 **Other policy changes targeted at small firms**

A concern for any DID analysis is whether other policy changes differentially affected the control and treatment groups. If this were the case, it would be difficult to isolate the causal effect of the one policy change we are interested in.

In the wake of the GFC, other policies were introduced especially to help small firms. The most important of these is the Taxation (Business Tax Measures) Act 2009, introduced to help smaller firms with the pressures of the recession by helping cash flows and reducing the time spent working through tax forms.[14] The new laws came into effect at several different dates, the earliest of which was April 1, 2009, right after our first policy change of March 1, 2009. However, even if these tax laws differentially changed the hiring behavior of very small firms, for most of our analysis we compare firms in a narrow band around the threshold of 20 employees. It is unlikely that these tax changes differentially affected firms close to this threshold. As an additional check, we look for any policy effect to disappear after the 2011 policy change, and

Table 1 Distribution of contract type for new hires in 2012

Type of contract	Permanent contract with a trial period (%)	Permanent contract without a trial period (%)	Temporary contract (%)
% of new hires	37.6	41.7	20.7

Notes: Limited to individuals hired by the firm in the previous year, giving an observation count of 2,397. The sample is limited to 2012, and it comes from the Survey of Working Life (see Section 4.1 for details on this nationally representative survey).

14 For more details, see http://www.ird.govt.nz/technical-tax/legislation/2009/2009-5/leg-2009-5-changes-to-help.html

changes driven by trial period policy will be eliminated after 2011 whereas any differential changes driven by this tax policy will not be eliminated.

Throughout 2009, the government fast-tracked US$500 million of publicly funded building projects with the aim of creating jobs and stimulating the economy.[15] Although not specifically designed to aid small firms, small firms may have reaped disproportionate benefit from these projects. However, firms just above and below the 20-employee threshold were affected similarly by the government stimulus.

More broadly, economic difficulties in the wake of the GFC should have continuous effects across the threshold and thus should not bias our estimates.

3 Theoretical model

3.1 Quantity of hires

This section presents a simple model of employer hiring behavior that motivates our empirical tests of the effect of the policy on quantity of hires.

3.1.1 No trial periods

First consider a situation without trial periods. A firm is faced with a one-time choice of whether to offer a job to a single applicant, a.[16] If offered the job, the applicant will accept with probability 1, start work in period $t = 0$, and stay with the firm permanently. Assume without loss of generality that the firm's profit in each period will be zero if it does not hire the applicant. Hiring the applicant will result in profit p each period. At the time of the hiring decision, the firm has imperfect knowledge of p and views it as a normally distributed random variable, with $p \sim N(\mu, \sigma^2)$ and $\sigma^2 > 0$. The firm can be assumed to observe p in the first period the applicant works if it makes the hire, but it is unable to act on this knowledge. Assume the firm is risk neutral and it maximizes the net present value (NPV) of future profit flows using a discount rate of $\delta > 0$. Then the expected NPV of profits if the firm hires is given by:

$$E[NPV^{hire\_notrial}] = \sum_{t=0}^{\infty} \frac{1}{(1+\delta)^t} \mu = \frac{1+\delta}{\delta} \mu \tag{1}$$

Clearly, the firm will then hire the applicant if $\mu > 0$, will not hire him if $\mu < 0$, and will be indifferent to hiring him if $\mu = 0$.

3.1.2 With trial periods

Now consider the case after the government introduces trial period legislation. The firm has three options: it can offer the applicant a job without a trial period; offer the applicant a job with a trial period; or not offer a job. The applicant will always accept an offered job. An applicant hired without a trial period will work for the firm permanently. If the applicant is hired on a trial period, the firm observes p during $t = 0$, after which it faces the one-time choice of

15 For more details, see http://www.beehive.govt.nz/release/fast-tracked-public-projects-give-500m-boost

16 Large firms may be thought of as facing many such decisions. The underlying assumption is that the value to the firm of any particular employee under such consideration is not dependent on which other individuals are hired.

whether to retain the worker for period 1 onward or costlessly dismiss him. However, hiring the worker with a trial period imposes a cost $c > 0$ on the firm at $t = 0$.[17]

After learning the value of P, at the start of $t = 1$, the NPV of profits of retaining the worker is:

$$NPV^{retain} = \sum_{t=1}^{\infty} \frac{1}{(1+\delta)^{t-1}} p = \frac{1+\delta}{\delta} p \tag{2}$$

Clearly, the firm will then retain the applicant if $p > 0$, will dismiss the worker if $p < 0$, and will be indifferent to retaining him if $p = 0$. For simplicity, we assume the distribution of productivity does not change following the introduction of trial period policy. Relaxing this assumption (e.g., if employees put in more effort when hired under a trial period) may alter the model's predictions. We also abstract away from any substitution between trial periods and other types of contracts such as temporary contracts, and we conduct a detailed empirical analysis of temporary contracts described in Section 5.4.

Hence, we can use a normal distribution truncated below at zero to model the firm's profits each period after the initial period if it hires the applicant on a trial period. Let $\psi(p;\mu,\sigma)$ denotes the cumulative distribution function (cdf) of p so that $\psi(0;\mu,\sigma)$ is the probability, from the firm's perspective, that p is negative. Then the expected NPV of profits when hiring on a trial period is:

$$\begin{aligned}
E[NPV^{hire_trial}] &= E[p] - c + \sum_{t=1}^{\infty} \frac{1}{(1+\delta)^t} ([\Psi(0)]E[p \mid p] \le 0] + (1-\Psi(0))E[p \mid p > 0]) \\
&= E[p] - c + \sum_{t=1}^{\infty} \frac{1}{(1+\delta)^t} (0 + (1-\Psi(0))E[p \mid p > 0]) \\
&= \mu - c + (1-\Psi(0)) \sum_{t=1}^{\infty} \frac{1}{(1+\delta)^t} \left(\mu + \sigma \frac{\phi(\frac{-\mu}{\sigma})}{1-\Phi(\frac{-\mu}{\sigma})} \right) \\
&= \mu - c + (1-\psi(0)) \frac{1+\delta}{\delta} \left(\mu + \sigma \frac{\phi(\frac{-\mu}{\sigma})}{1-\Phi(\frac{-\mu}{\sigma})} \right)
\end{aligned} \tag{3}$$

where $\phi(\cdot)$ is the probability density function (pdf) and $\Phi(\cdot)$ is the cdf of the standard normal distribution.[18]

Figure 1 shows, for values $\delta = 0.1$ and $c = 0.5$, how firm hiring behavior depends on the values of μ and σ, and how this changes when trial periods are introduced, based on the profit functions given in equations (1) and (3).

As stated earlier, without trial periods the firm will hire the worker if $\mu > 0$ or to the right of the vertical line in the figure, regardless of the value of σ.

The value of hiring a worker without a trial period is not affected by the availability of trial periods, so when trial periods are available a firm will never hire a worker with $\mu < 0$ without a trial period.

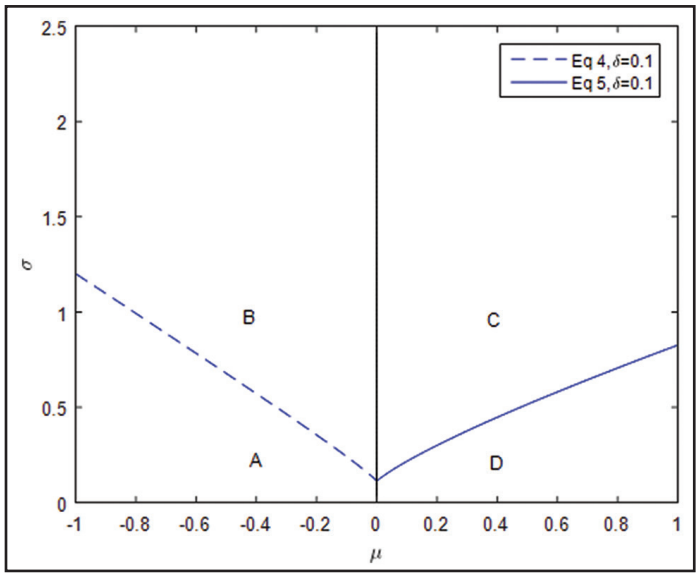
Consider a worker with $\mu > 0$ in a world with trial periods. The firm will hire her *with* a trial period if the value of equation (3) is positive, and the line

$$\mu - c + (1-\Psi(0)) \frac{1+\delta}{\delta} \left(\mu + \sigma \frac{\phi(\frac{-\mu}{\sigma})}{1-\Phi(\frac{-\mu}{\sigma})} \right) = 0 \tag{4}$$

17 A cost to using trial periods is necessary to rationalize some employees not being hired on trial periods even when they are available. Alternatively, this could be modeled as the applicant turning down the job offer with some positive probability if it is offered with a trial period. The cost could take many forms, such as increased administrative burden or decreased worker productivity through diminished morale or firm loyalty.

18 See Greene (2002) for this formula of the expected value of a truncated normal distribution.

Figure 1 Theoretical prediction of trial period policy impact on whether to hire.



Notes: Area A represents $\mu-\sigma$ combinations such that the applicant will not be hired whether or not trial periods are available; applicants in area B will be hired only when trial periods are available and then will be hired with a trial period; applicants in area C will be hired regardless of trial period legislation and hired on a trial period when available; and applicants in area D will always be hired and never with a trial period. A value of 0.5 is used for the cost of using trial periods, and the discount rate is $\delta = 0.1$.

shown as a dashed line in Figure 1 marks the boundary between not hiring (below) and hiring with a trial period (above).

Now consider a worker with $\mu > 0$ who will always be hired (without a trial period) in a no-trial-period world. If trial periods are available, she will be hired *with* a trial period if the value of equation (3) is greater than the value of equation (1), i.e., if

$$\mu - c + (1-\Psi(0))\frac{1-\delta}{\delta}\left(\mu + \sigma\frac{\phi(\frac{-\mu}{\sigma})}{1-\Phi(\frac{-\mu}{\sigma})}\right) > \frac{1+\delta}{\delta}\mu \tag{5}$$

and will be hired without a trial period otherwise. This boundary is shown by the solid diagonal line in Figure 1.

Overall, Figure 1 shows that the $\mu - \sigma$ plane is divided into four sections: workers with $\mu - \sigma$ combinations in section A will not be hired in either world; those in section B will be hired in the trial period world only and will be hired with a trial periods; those in section C will be hired in both worlds, with a trial period when it is available; and those in section D will be hired with no trial period in both worlds. Note that there are no $\mu - \sigma$ combinations that would cause the worker to be hired when trial periods are not available but not when they are available. Thus if any preferred candidates fall into section B of Figure 1, the model predicts that hiring will be higher when trial periods are available.

3.1.3 **Heterogeneous effects**

An important dimension on which firms differ is the prevalence of long-lasting new hires, and firms in certain industries tend to have more employee churn than others. To explore how expected employee tenure affects the impact of trial period policy, we augment the above model

by assuming any worker in a job quits each period with some exogenous probability τ, after which the firm's profits are zero. An increase in τ can be modeled as an increase in the firm's discount rate, δ, and profits farther in the future are valued less because they are less likely to be realized.

Figure 2 replicates Figure 1, but it shows how the equations dividing firm behavior change when the discount rate increases to 0.4. Area B that represents hires caused by the policy shrinks with a higher discount rate. We thus predict that the effect of trial period policy will be larger in industries where employment relationships tend to last longer.

The model also trivially predicts that trial period legislation decreases the stability of employment, because an employee will be dismissed if his effect on firm profit is revealed to be negative.

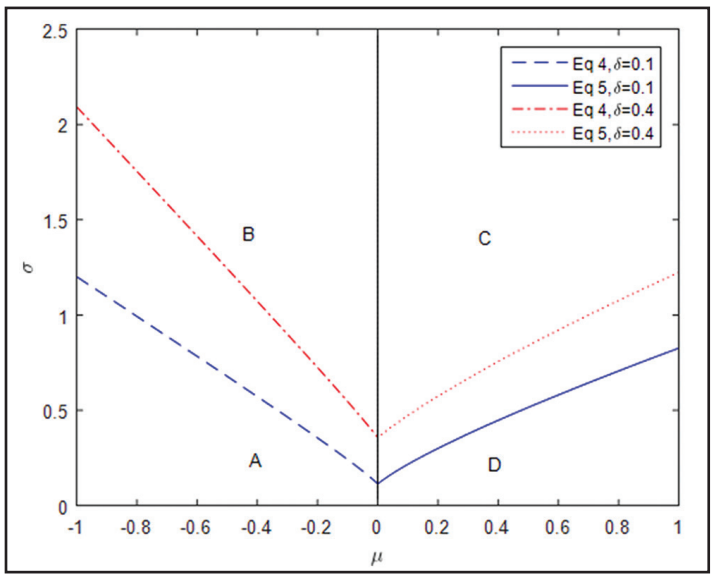
3.2 Type of hires

This section extends the model from the previous section to show that, conditional on a hire being made, trial periods increase the probability a riskier applicant is hired.

Then the same risk-neutral firm considers whether to hire applicant a, applicant b, or neither with $\sigma_a > \sigma_b$ Assume the distributions of μ_a, μ_b, σ_a, and σ_b are all independent and the support of the distribution of σ is unbounded above.[19] Then the following three propositions together show that trial periods increase the probability that applicant b is hired, both in total and conditional on a hire being made. Proofs are presented in Appendix.

Proposition 3.2.1. *If the riskier applicant, b, is hired when trial periods are unavailable, she is also hired when trial periods are available.*

Figure 2 Theoretical prediction of trial period policy, heterogeneous effects.



Notes: This figure shows how the effects of trial period policy differ for firms with different discount rates. The blue lines (dashed and solid) denote a discount rate of 0.1, and the red lines (dash-dotted and dotted) denote a discount rate of 0.4. See further notes in Figure 1.

19 The independence of σ from μ is a strong assumption. It is sufficient but not necessary for the proof of proposition 3.2.3, and not required for the other propositions.

Proposition 3.2.2. *For each combination of* μ_a, μ_b, *and* σ_a, *where* $\mu_a > \mu_b$ *and* $\mu_a > 0$, *meaning that candidate a is hired if trial periods are not available, there exists* σ_b *such that candidate b is hired when trial periods are available.*

To illustrate, consider an applicant *a* with $\mu_a = 0.5$ and $\sigma_a = 0.5$. Consider the values of μ_b and σ_b such that the firm hires *a* when trial periods are unavailable, and hires *b* when trial periods are available:

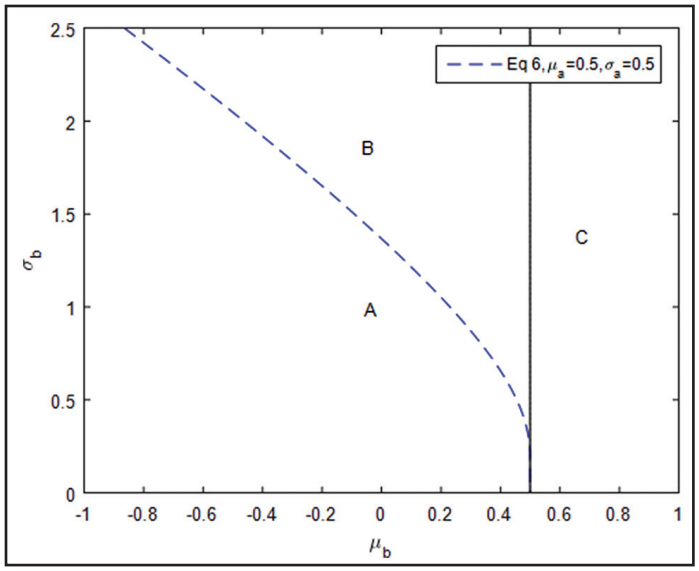
$$\mu_b < \mu_a \text{ and } E[NPV^{hire\_trial_b}] > E[NPV^{hire\_trial_a}] \text{ and } E[NPV^{hire\_trial_b}] > E[NPV^{hire\_notrial_a}] \quad (6)$$

Figure 3 plots μ_b against σ_b. The area labeled B is the combinations satisfying equation (6).

Proposition 3.2.3. *If neither applicant is hired when trial periods are unavailable, and one is hired when trial periods are available, then the higher variance applicant, b, is more likely to be the one hired.*

These three propositions yield the prediction that, conditional on a hire being made, trial period legislation increases the probability that the riskier of two candidates under consideration will be hired.[20]

Figure 3 Substitution toward riskier applicants with trial periods.



Notes: This figure compares applicant *a,* with expected profitability of 0.5 and a standard deviation of profitability of 0.5, with applicant *b*, whose μ and σ are plotted on the two axes. Area A gives the $\mu-\sigma$ combinations for applicant *b* such that applicant *a* is hired regardless of whether trial periods are available; area B gives the combination such that applicant *a* is hired when trial periods are not available and applicant *b* is hired when trial periods *are* available; and area C gives the combinations such that applicant *b* is hired regardless of whether trial periods are available. The equation comes from the theoretical model in Section 3.2. A value of 0.5 is used for the cost of using trial periods, and the discount rate is $\delta = 0.1$.

20 This proposition does not require that the expected profitabilities of the two candidates are equal. Furthermore, the variance of profitability is conditional on the information the employer has about an applicant. An applicant may be riskier from the employer's perspective because she has a shorter work history, or less work history relevant to a particular job. This motivates our examination of "risky" groups for whom employers have less information, such as young people, recent beneficiaries, and recent migrants.

4 Data

4.1 Description of data and key variables

We use data from Statistics New Zealand's Integrated Data Infrastructure (IDI), the core of which is the Employer Monthly Schedule (EMS), a linked employer–employee data set derived from tax records that cover at a monthly level essentially every employment relationship in New Zealand. These data are linked to a variety of other administrative data at the individual and firm levels.

For most of our analysis, we restrict our sample to hires occurring in the period January 2005 to March 2014 which has comprehensive data and covers a substantial period before the first and after the second policy change. However, some of our specifications use a shorter sample period; in particular, those involving education leavers end in December 2012 because education data are required for the following year and, at the time of writing, were available only until December 2013.

Our key variables are obtained from the EMS table. We define a hire as a new employer–employee pair in the EMS that did not exist in the previous month. For most of our analysis, we restrict this and consider only new hires, defined as new employer–employee pairs that did not exist in the previous 5 years. This is to exclude those who appear to be hired by the same firm many times due to seasonal work, temporary work, or other such phenomena. New hires are also the more relevant measure because trial periods may only be used for employees who have never worked for the firm earlier, so any change in hiring behavior should be seen in this group.[21]

We define firm size as the start-of-month head count of a firm, calculated by subtracting the number of hires (of any kind) from the total number of employees paid at any time in the month. The relevant firm size measure for trial-period eligibility is a head count of employees, whether permanent or temporary. Between the policy changes, an employee hired with a trial period by a firm with fewer than 20 employees could be dismissed within his first 90 days even if at that time the firm had grown to 20 or more employees. When other employees leave the firm or the firm hires multiple employees during the month, start-of-month size may not perfectly capture size at time of hiring. However, the number of firm months with eligibility affected by the difference between the two is likely to be low. To check that this minor mismeasurement does not affect our results, we also run specifications in which we exclude firms of size 19 or 20.

Note that our measure includes anyone paid by the firm as an employee, and so could in principle include working proprietors if they receive wages. We derive firm size this way to match the legal definition used for application of trial period law.

In calculating a firm's number of hires, and in regressions at the hire level, we exclude people hired more than 100 times in the period January 2005–March 2014, assuming that these reflect data issues. The impact is small; for hires involving firms with 15–24 employees, 3,072 individuals and 9,360 new hires are dropped from a total of over 800,000 hires. These individuals are still subtracted off in deriving a firm's start-of-month size.

21 The legal requirement is that the employee has never worked for the firm previously; since we have data on employment relationships from 2000 only, we consider an employee who has not worked for a firm in 5 years to have never worked for that firm.

We use additional information on people who are hired to investigate whether trial periods encourage the hiring of disadvantaged types or affect the duration of employment. Some detail can be gleaned from the EMS. We categorize a person as not having worked in the previous year if he received no wages in the data, and class him as having worked elsewhere the previous month if he was paid by a different employer.[22] Similarly, we classify a person as a beneficiary in the last year if he received benefit income from any of the main working age benefits in the EMS.

We also use the EMS to construct indicators related to duration of employment. We do not know specifically when within a month employees started or finished working for a firm, but we do know the number of consecutive months in which they were paid by the employer and use this as our measure of duration.[23] For example, parts of our analysis look at whether a new employee lasted five or more months with the firm, meaning that she was paid by the firm in at least five consecutive months. We consider this a reasonable indicator that the employee was not dismissed while on a trial.

The EMS has certain limitations. In particular, we cannot tell the nature of the employment agreement (e.g., whether the contract is permanent, fixed term, or casual), whether the employee was hired with a trial period, whether a separation was voluntary or the employee was dismissed, the number of FTEs worked, or the occupation or role in which the employee worked. We thus supplement EMS data with data from the Survey of Working Life (SOWL), which was conducted in 2008 and 2012 and covered a representative[24] sample of approximately 10,000 wage and salary earners in 8,000 households each survey period. We use the 2012 SOWL to estimate industry-level use of trial periods and the 2008 SOWL to estimate industry-level prevalence of permanent contracts and employer-funded training. In additional analysis, we use individual-level SOWL data on whether workers were hired on trial periods or temporary contracts to investigate whether trial periods act as substitutes for temporary contracts.[25]

We generate additional information about hires using the links between the EMS and other data sources. The IDI contains information on gender, age, and ethnicity.[26] These information allow us to identify hires who are under 25 years of age, and those who are under 25 and Māori or Pasifika, two ethnicities whose socioeconomic outcomes tend to lag those of Europeans. To identify jobseeker beneficiaries, being hires who have received a jobseeker benefit in the previous 12 months, we use a Ministry of Social Development administrative data set. Similarly, we use tables of visa data to identify "recent migrants" who had their visa approved

22 Note we are able to observe employment in New Zealand only, so some non-workers may have in fact been working overseas previously. Any effects of such misclassification will be reduced to the extent that employers do not consider foreign experience to be a perfect substitute for New Zealand experience.

23 The EMS table does include fields that indicate the start and end dates of employment, but the quality of these variables is very poor and so we choose not to rely on them.

24 After applying survey weights.

25 We also estimated the impact of being on a trial period on whether employees reported a high chance on no-fault job loss and whether long working hours were causing difficulties. This was estimated by comparing the change in outcomes for trial-period employees on either side of the 90-day tenure threshold, with the change in outcomes for non-trial-period employees on either side of the 90-day threshold. Results, not reported, were noisy, with standard errors too large to be informative.

26 The IDI contains ethnicity information from multiple sources, and individuals who have supplied their ethnicity multiple ways are more likely to state multiple ethnicities. To maximize consistency, we use ethnicity sourced from tertiary education where available, from school education where tertiary is unavailable, and from all other sources where neither of these are available.

in the previous 2 years.[27] Finally, the administrative data on secondary and tertiary education enrollments from the Ministry of Education allow us to identify recent education leavers, defined as those who attended school or university in New Zealand in the year before but not in the year after being hired.

The IDI also contains industry information for firms. Industry classifications come from Australian and New Zealand Standard Industrial Classification (ANZSIC) 2006 codes, and they are consistent for a firm over time. They divide firms into 19 divisions at the broadest level (level 1), and for much of our analysis we use more detailed level 3 ANZSIC 2006 codes that divide firms into 203 industries.

Two level 3 industries experienced large anomalous spikes in hiring in our data: central government administration ("O751") in December 2009 and school education ("P802") in February 2010. Central government administration employers are largely outside the focus of firm size range, but school education employers are included in our data in large numbers. The reasons for these hiring spikes are unclear, but we are confident that they do not reflect an employer response to trial period policy. To ensure that they do not drive our findings, throughout our regression specifications we include a dummy variable for firms or hires in each of these industry months, and our main results are also robust to us dropping these industries entirely.

4.2 **Descriptive statistics**

To minimize time-varying unobservable differences between our treatment (small) firms and control (large) firms, for many specifications we limit our sample to firms with 15–24 employees.[28] Table 2 shows the importance of these firms relative to the whole economy in terms of employees and hires. There are many small firms in the New Zealand economy,

Table 2 Distribution of employment and hiring over firm sizes

Firm size category	Firms with 0–14 employees	Firms with 15–19 employees	Firms with 20–24 employees	Firms with 25+ employees
Average employment (employee–firm matches)	570,100	80,500	59,400	1,363,000
% of total employment	27.5%	3.9%	2.9%	65.8%
Average number of firms employing in a month	135,734	4,803	2,723	9,278
% of total firm months	89.0%	3.1%	1.8%	6.1%
Count of all hires	6,611,700	799,500	590,900	8,796,100
% of all hires	39.4%	4.8%	3.5%	52.4%
Count of new hires	3,948,500	512,100	381,200	5,439,200
% of new hires	38.4%	5.0%	3.7%	52.9%

Notes: Statistics are for the entire period from January 2005 to March 2014. A new employee is one who has not worked for the firm in the previous 5 years. Total counts are rounded to the nearest 100.

27 This will capture those who renew a visa from within New Zealand in addition to new migrants. Note that Australians do not require a visa to work in New Zealand, so are not classified as migrants.

28 Firm size is as at the start of the month in question. Thus a firm may be small 1 month, large another, and out of sample another.

but the majority of employment and hiring is in firms with 25 or more employees. If trial period policy did affect hiring behavior, the effect is expected to be smaller in larger firms, for which hiring costs are less important and the cost of a poor hiring decision is easier to absorb. Our main estimates of the policy effect are thus likely to be an upper bound on the policy effect for firms with 25 or more employees.

Table 3 presents summary statistics for our data, separately for small firms (15–19 employees) and large firms (20–24 employees), and by period relative to the policy changes. The average number of firms employing each month is stable over time for both groups, though there are around 4,800 small firms in each month as opposed to around 2,700 large firms.

A large proportion of firms hire each month. From 54% to 68% of firms make any hires in a month, and from 42% to 56% of firms hire new employees each month. The difference between new hires and overall hires is likely to reflect phenomena such as seasonal workers who return to the same employer each year, and casual employees. Hiring rates are

Table 3 Descriptive statistics for treatment and control firms by period

	Firms with 15–19 employees			Firms with 20–24 employees		
Period relative to policy changes	Pre	Between	Post	Pre	Between	Post
Average number of firms employing each month	4,884	4,710	4,753	2,754	2,648	2,733
Average firm size	16.8	16.8	16.8	21.8	21.8	21.8
% of firms with multiple plants	13.8%	13.7%	13.3%	17.7%	18.0%	16.8%
% of firm months hiring anyone	60.6%	54.0%	55.7%	67.6%	61.4%	62.8%
% of firm months hiring a nonseasonal employee	49.1%	42.2%	44.2%	56.4%	49.2%	51.1%
% of firm months hiring a new employee	47.3%	40.0%	42.3%	52.7%	46.8%	49.1%
Average number of new hires per firm month	1.1	0.9	0.9	1.4	1.2	1.2
Among firm months that hired a new employee						
Average number of new hires	2.2	2.2	2.1	2.5	2.5	2.4
25th percentile of number of hires	1	1	1	1	1	1
50th percentile of number of hires	1	1	1	2	1	1
75th percentile of number of hires	2	2	2	3	3	3
Among new hires,% who						
Stayed with the firm for 5+ months	45.1%	47.1%	49.1%	45.3%	46.5%	49.3%
Received benefit income in previous year	17.1%	18.1%	20.1%	17.2%	18.3%	19.6%
Received jobseeker benefit income in previous year	10.2%	12.2%	13.2%	10.2%	12.2%	12.7%
Had not worked in the past year	23.4%	25.3%	27.6%	23.1%	25.0%	26.7%
Arrived in New Zealand on a visa in the past 2 years	16.2%	19.0%	20.3%	16.4%	20.0%	20.8%
Are <25 years old	43.1%	40.3%	42.3%	41.3%	38.9%	40.1%
Are Maori or Pasifika and <25 years old	10.2%	8.7%	9.2%	10.3%	8.8%	9.1%
Left education in the previous year	12.6%	11.1%	11.2%	12.6%	11.1%	10.9%
Had a job elsewhere the previous month	51.4%	47.3%	47.6%	51.6%	47.6%	48.3%

Notes: Statistics are for the entire period from January 2005 to March 2014.

considerably lower in the between and post-periods for both small and large firms, reflecting the Global Financial Crisis.

Among firm months that hired new employees, large firms are slightly more likely to make more hires. The median number of hires is 1 for small firms each period, and falls from 2 pre-policy for large firms down to 1 in subsequent periods. The 75th percentile is 3 in all periods for large firms, compared with 2 for small firms.

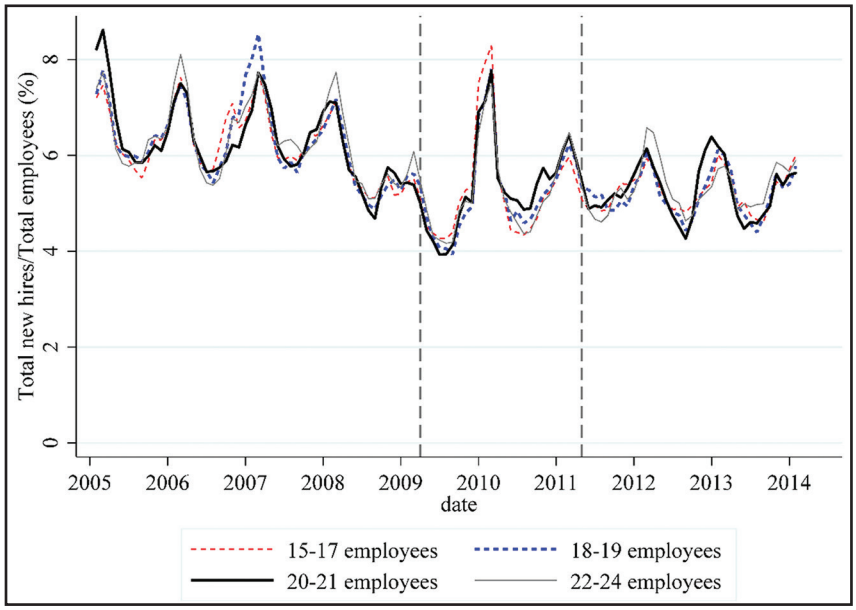
The fourth section of Table 3 shows the percentage of new hires that are various types. Across firm sizes and periods, the majority of new hires results only in short-term employment; 43–47% of new employment relationships last 5 months or longer. The percentage is very similar in small and large firms both in the pre- and post-periods, and it is slightly higher in small firms than large firms between the policy changes. The percentage of new hires who were employed elsewhere the previous month is very similar in small and large firms pre-policy, at 51.4% and 51.6%, respectively, and it declines somewhat for both firm sizes in subsequent periods.

In the pre-period, workers of unknown quality of all types except those under 25 years old are equally common among small-firm and large-firm new hires. Around 17% of new hires are recent beneficiaries, 10% are recent jobseeker beneficiaries, 23% have not worked in the previous year, 16% are recent migrants, 10% are Māori or Pasifika under 25 years old, and 13% are education leavers. Among new hires at small firms in the pre-period, 43% are under 25 years old, whereas 41% at large firms are in this age range. The proportion of new hires of each disadvantaged type shifts with the GFC, and some types become more common and others less common.

4.3 **Patterns of hiring over time**

Figure 4 shows the monthly behavior of the total number of new hires scaled by the total number of employees for various firm size ranges within our treatment and control groups.[29]

Figure 4 New hires as a percentage of total employees by firm size.



Notes: Values are 3-month moving averages. Vertical gray lines indicate the policy changes.

29 Three-month moving averages are presented for ease of viewing.

The treatment and control groups are split into those very close to the 20-employee cutoff (18–19 employees and 20–21 employees) versus those further away (15–17 employees and 22–24 employees). The vertical lines show the introduction of the two policies. A policy impact would appear as a gap that opened up between the dotted versus solid lines after the first policy change, and closed again once trial-period eligibility was extended to all firms. The figure suggests parallel trends in hiring before the first policy change and presents no evidence of a policy effect on the number of new hires for small firms, and the lines of hiring behavior are not only parallel, but also virtually coincide in all periods. We examine the policy effect on the number of hires and test for parallel pre-trends more rigorously in Section 5.1.

5 Methodology and results

We use a DID strategy from the double natural experiment to estimate how the ability to use trial periods affects a firm's hiring behavior. We estimate the policy effect as the change in the jump in hiring behavior between firms with less than 20 employees and those with more than 20 employees that occurred when trial periods were introduced for firms with fewer than 20 employees. The second policy change when trial periods were extended to all firms provides an inbuilt placebo test in our estimates; any difference between firms above and below the 20-employee cutoff that we observe opening after the first policy change should disappear after the second policy change if it is an effect of the policy.

Not all firms use trial periods for all eligible hires even when they have the option, and some firms may have illegally used trial periods before they were legally given this option. We estimate the effect of being legally permitted to use trial periods and do not attempt to identify the effect of a firm actually using trial periods for several reasons. First, since policy allows firms to use trial periods rather than requiring them to do so, the effect of being permitted to use trial periods is more relevant from a policy perspective. Second, our main administrative data do not identify which firms or employment relationships use trial periods. Third, firms may use trial periods for some new hires but not for others, so trial period use is not a clearly defined concept at the firm level.

5.1 Number of new hires

This section investigates whether trial-period eligibility causes the average firm to increase the number of people it hires, as predicted by our model. We test the policy effect on the quantity of hiring by firms using the general formulation explained earlier, with the number of new hires by the firm in the month as the dependent variable. We estimate these specifications as negative binomial regressions to account for the count structure of the dependent variable.[30]

Our main specification is at the firm-month level and takes the form:

$$E(hires_{jt}) = exp(a_0 + a_1 small_{jt} + a_2\ between_t \times small_{jt} + a_3 post_t \times small_{jt} + \mathrm{a}_4 \ln(size_{jt}) + \delta_{mr} + \gamma_t) \quad (7)$$

30 We test and reject the null hypothesis that a Poisson regression is the appropriate specification for our data.

where *j* denotes firm and *t* denotes month.[31] The variable *small*, defined at the firm-month level, is an indicator for the firm having fewer than 20 employees at the start of the month. The variable *between* is an indicator for the period between the two policy changes, and *post* is an indicator for the period after the second policy change. The variable *size* is a continuous measure of firm size, namely the number of employees at the start of the month. It enters the equation in log form, allowing for a smooth progression in hiring behavior as firms get larger. Here δ_{mr} are industry-calendar month fixed effects, with firms grouped into 203 industries. This flexible specification allows each industry to have its own seasonal pattern of hiring. γ_t are fixed effects for each month in our sample period. We cluster standard errors at the firm level to account for within-firm correlation over time.

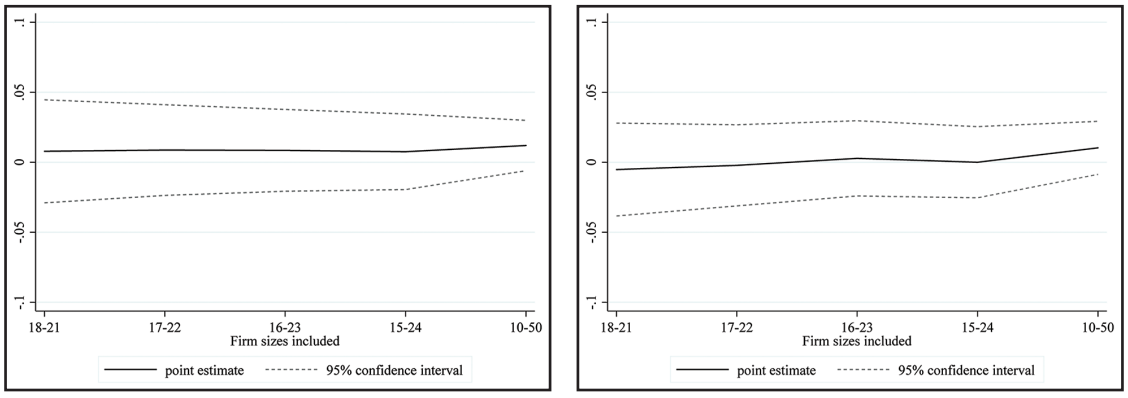
The coefficient α_2, on the interaction *between* × *small*, is our estimate of the policy effect. We expect α_1, which captures the pre-policy discrete difference between firms with 19 employees and those with 20 employees over and above the difference captured by the log firm size term, to be close to zero. Similarly, we expect α_3 to be close to zero because the sum $\alpha_1 + \alpha_3$ gives the discrete difference between firms above and below the cutoff after all firms can use trial periods.

5.1.1 **Basic specification**

The panels of Figure 5 show how the estimates of the coefficients of interest from regressions of number of hires vary as the firm sizes included change, based on the specification in equation (7). The full regression tables are shown in Table A1 in Appendix. We focus on new hires, meaning individuals who have not worked for the firm earlier, because employees who have previously worked for an employer cannot be rehired by that employer with a trial period.

Panel A1 of Figure 5 plots the estimated policy effect and 95% confidence intervals from five different regressions, varying the firm sizes included in each regression. Our preferred specification is the 15–24 version, which balances power against homogeneity of treatment and control firms. The point estimate of the policy effect here is 0.008, implying that the policy

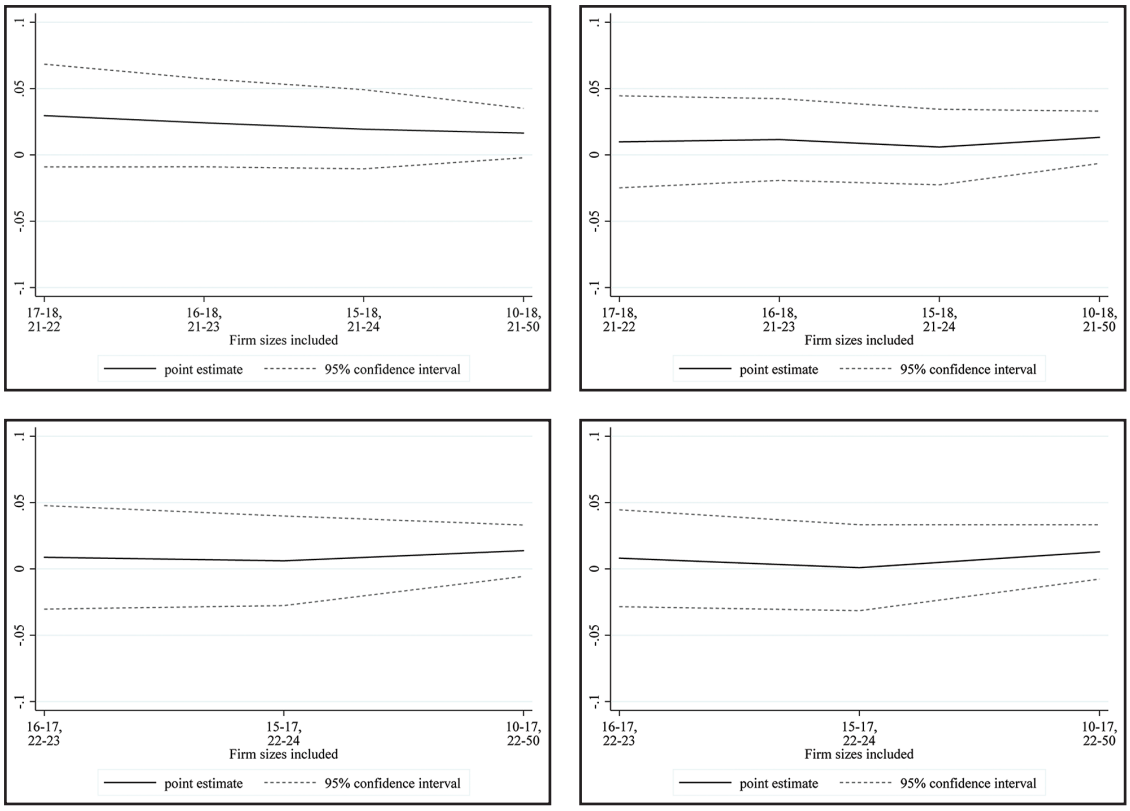
Figure 5 Policy effect on the number of new hires, varying firm sizes included. Panel A1: Policy effect, no cutout. Panel A2: Placebo test, no cutout. Panel B1: Policy effect, excluding firms sized 19–20. Panel B2: Placebo test, excluding firms sized 19–20. Panel C1: Policy effect, excluding firms sized 18–21 Panel C2: Placebo test, excluding firms sized 18–21.



(*continued*)

31 The allocation of workers to plants within multi-plant firms in the IDI is unreliable, so our preferred specification is at the firm as opposed to plant level.

Figure 5 (Continued)



Notes: The figure plots estimated policy effects and their 95% confidence intervals (left-hand panels) and placebo policy effects and their 95% confidence intervals (right-hand panels), varying the range of firm sizes in the sample. The policy effect is the coefficient on between * small firm from the corresponding specifications in Table A1 in Appendix. The placebo tests plot the coefficients on post * small firm. See Table A1 in Appendix for details.

caused a tiny and statistically insignificant 0.8% increase in the number of new hires by firms with 15–19 employees.[32] The coefficient is sufficiently precisely estimated that we can confidently rule out an economically significant policy effect; the 95% confidence interval extends only to a 3.5% increase in hires. The point estimate remains close to zero when the firm size range is varied. Table A1 in Appendix shows that the coefficient on *small firm* also tends to be close to zero (between 0 and −0.016) and with small standard errors. This means, reassuringly, that our model finds no large discrete difference in hiring between our treatment and control firms in the pre-policy period after controlling smoothly for firm size.

Panel A2 of Figure 5 shows the corresponding placebo tests for the above regressions, plotting the coefficient estimates on *post*small firm*. As explained previously, the sum of the coefficients on *small firm* and *post*small firm* captures the discrete difference in hiring between firms on either side of 20 employees after the second policy change, which we expect to be close to zero. Reassuringly, these placebo coefficients are close to zero and precisely estimated across the ranges of included firm sizes.

To additionally test whether large and small firms were on parallel trends in the period before the policy was announced in December 2008, Table A2 in Appendix replicates our

32 In a negative binomial regression, for a coefficient b, $\exp(b)$ gives the effect of a unit change in the independent variable on the dependent variable (rate of hires), so $\exp(b) = 2$ would imply an increase of 1 corresponds to a doubling of the hiring rate.

preferred specification from Figure 5 but limits the sample period from January 2005 to November 2008 and interacts a linear time trend with the *small firm* indicator. The coefficient on this interaction is precisely estimated as zero, showing that there was no meaningful preexisting difference in hiring trends between the treatment and control firms.

To minimize the effects of any misclassification of firms as treatment or control or endogenous selection of firms into the treatment group, Panels B and C of Figure 5 replicate Panel A, but exclude firms sized 19–20 and 18–21, respectively.[33] There is no marked change in the results; the estimated policy effect is slightly larger though still close to zero when excluding firms sized 19–20, and it is even smaller than in Panel A1 when excluding firms sized 18–21. In all cases, the estimated coefficients on *post*small*, the placebo policy effects, are close to zero. This suggests neither mismeasurement of firm size nor endogenous firm size is driving our results.[34]

As an alternative to our negative binomial regressions, we run three OLS specifications with firms sized 15–24, presented in columns 5–7 of Table A3 in Appendix. The first is a linear probability regression where the dependent variable is an indicator for the firm making any new hires. The coefficients on *between*small* and *post*small* are both small and insignificant, providing no evidence of a policy effect. The second is an OLS regression where the dependent variable is the log of new hires plus one. Here the coefficient on *between*small* of 0.019 is positive and statistically significant, suggesting an economically small 1.9% increase in hiring. However, it is similar to the coefficient on *post*small* of 0.013, which means that it is probably driven by small differential changes in unobservable characteristics of the treatment and controls firms, and it is unlikely to indicate a genuine policy effect.[35] The coefficient estimate on *small firm* is also statistically significant, highlighting the fact that even small effects can be statistically significant when our statistical power is high. The third is an OLS regression where the dependent variable is the percentage change in employment relative to the previous month. The coefficients on *between*small* and *post*small* are again small and insignificant, suggesting a tiny 0.093% point decrease in employment growth due to the policy.

It became public knowledge that trial periods were going to be introduced for small firms 3 months prior to the first policy change, and that they would be extended to all firms 7 months prior to the second policy change, so anticipation effects are a potential concern. A firm that anticipated becoming eligible to use trial periods in the future might have postponed hiring to take advantage of trial period policy, though substantial postponement seems implausible.[36] If some did occur, it would cause us to overestimate the extent to which small firms increased their hiring relative to large firms between the policy changes. Given we already estimate a near-zero effect, the potential for anticipation of the second policy change by large firms does not alter our conclusions.[37]

33 Full regression results are again shown in Table A1.

34 To further address endogenous selection, we replicated this analysis with the *small firm* variable defined by a firm's size prior to the first policy change. Results, unreported, again suggest a policy effect close to zero, with small standard errors.

35 Our results are also not affected by our treatment of outliers in the data, the way we measure firm size, or by allowing small firms' hiring to respond differently to GDP fluctuations than large firms' hiring, as shown in the first four columns of Table A3**.** In column 4, the interaction between *small firm* and quarterly GDP (in billions of NZD) is very close to zero and statistically insignificant; the estimate of the policy effect is essentially unchanged.

36 Anticipation effects are not a concern for the duration of employment analysis.

37 In addition, our baseline results are unchanged (unreported) and we see no differential hiring by period when we replicate the baseline specification for firms sized 15–24, but add to the *between* and *post* indicators an indicator for each post-announcement, pre-policy period.

Together, these results show any economy-wide increase in the number of new hires because trial period policy is tiny and economically insignificant, despite the fact that use of trial periods is fairly widespread, as discussed in Section 2.3.

5.1.2 **Very small firms**

Our preferred specifications include firms close to the 20-employee discontinuity only to keep treatment and control firms as similar as possible. However, one possibility is that very small firms are affected by trial period policy, while larger firms are not. We therefore estimate a similar specification where we include in our sample firms with 1–29 employees and allow the policy effect to differ for firms in each five-employee size band. The results of this regression are presented in Table 4. The coefficients on the interactions of *between* with the small size categories are all small, insignificant, and precisely estimated; there is no evidence that the policy affected new hiring by very small firms in an economically significant way. We note, however, that these results should be interpreted as suggestive only, because firms with 20–24 employees may not be a good control group for very small firms.

Table 4 Policy effect for very small firms

Negative binomial regression	
Dependent variable: Number of new hires	
Between * size 1–4	−0.003
	(0.016)
Between * size 5–9	0.015
	(0.016)
Between * size 10–14	0.017
	(0.016)
Between * size 15–19	0.003
	(0.017)
Between * size 25–29	0.015
	(0.020)
Post * size 1–4	−0.013
	(0.014)
Post * size 5–9	−0.005
	(0.015)
Post * size 10–14	−0.014
	(0.015)
Post * size 15–19	−0.004
	(0.015)
Post * size 25–29	−0.028
	(0.018)
Observations	*15,423,435*
% of nonzero hires	*15.9%*

Notes: The table presents results from a negative binomial regression at the firm-month level of the number of hires, similar to Table A1 in Appendix, but expanding the firm size sample to 1–29 employees. The regression also includes firm size category fixed effects, month-in-year fixed effects, log firm size and level 1 industry fixed effects. Standard errors, in parentheses, are robust and clustered at the firm level.

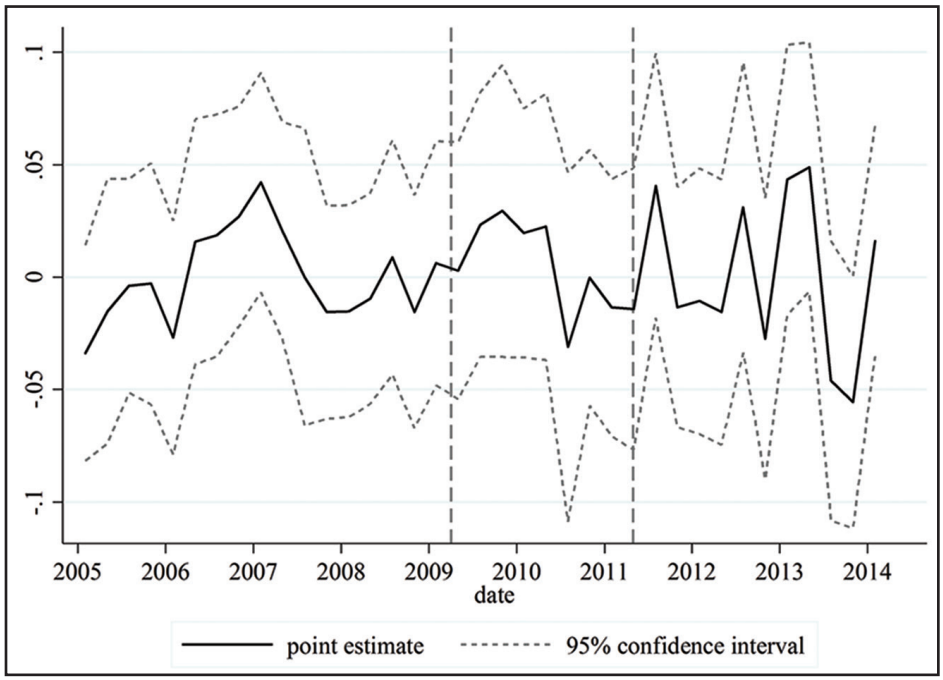
Although we find no effect on hiring for firms with 24 or fewer employees, it is theoretically possible that very large firms could be affected, whereas smaller firms are not. However, hiring and dismissal costs are relatively more important for smaller firms, and the potential cost of having an employee who is a poor fit for the job is greater for these firms, so we are confident that the policy also had no effect for very large firms.

5.1.3 **Time-varying effects**

Firms could have taken some time to learn about trial periods and how to use them after their introduction, in which case our regressions that look for one effect throughout the between-policy period would understate the true policy effect. We thus estimate a version of column 4 of the first panel of Table A1 in Appendix (including firms sized 15–24) where we allow the difference between small and large firms to differ in each 3-month period in our sample, not just in the three periods before, between, and after the policy changes.

Figure 6 plots the estimates from this regression of the discrete jump in hiring between small and large firms at each point in time, normalized to average 0 before the first policy change.[38] These estimates are all close to zero and statistically insignificant, giving further evidence of parallel trends in hiring prior to the policy changes. Furthermore, the variation around zero is similar in the between period to both before the first policy change and after the second policy change. There is no evidence that a policy effect developed over time after the first policy change.

Figure 6 Time-varying estimate of policy effect on number of new hires.



Notes: This figure plots the point estimate and 95% confidence interval of the coefficient on *date*small firm* in a regression that replicates column 4 in Panel A of Table A1 in Appendix, but allows the difference between small and large firms to vary each 3-month period.

38 That is, we estimate the coefficient on a dummy for each 3-month period interacted with *small firm*, and before plotting these subtract off the average of these coefficients for the pre-period.

5.1.4 **Heterogeneous effects**

We hypothesize that the benefits to firms of trial periods might be greater in certain types of industries, and we thus allow the policy effect to differ by various industry characteristics. The first characteristic is trial period use in the post-period. Some industries may benefit more from being able to use trial periods for unobservable reasons, and thus use them more frequently. We expect any policy effect to be greater in such industries. The second characteristic is the ubiquity of employer-funded training in the industry before the policy change; if employers have already invested in training new employees, they are less likely to want to dismiss them, and so benefit less from being able to do so. Our model predicts that hiring behavior will change more in response to the introduction of trial periods in industries where employment relationships tend to be long-lived. The next three characteristics are all intended to capture aspects of this. The third characteristic is the proportion of contracts in the industry which were permanent before trial period policy was introduced; the ability to dismiss a worker has less value when a worker who turns out to be a poor fit is on a temporary contract only. The fourth characteristic is the proportion of employment relationships in the industry which lasted at least 5 months in the pre-trial period years. The fifth characteristic is the degree of seasonality of employment in the industry; in very seasonal industries, a high proportion of workers are short term and firms benefit little from being able to dismiss them. We also test whether firms with high employment growth over the preceding 12 months are differentially affected by the policy change; firms with strong growth over a sustained period may be more likely to want new employees in new positions.

Table 5 explores possible heterogeneous effects on the number of new hires by replicating our main specification (column 4 of the first panel of Table A1 in Appendix, with firms sized 15–24) but interacting the policy effect with an interaction variable. For example, the interaction variable in the first column is the proportion of jobs in a level 2 industry starting on a trial period, measured using the 2012 SOWL. The coefficient on the interaction term of interest is 0.135, with a standard error of 0.139. The standard deviation of the interaction variable is 0.081, suggesting that a standard deviation increase in industry trial-period use is associated with a 0.01 (1% point) increase in the policy effect. This is economically small and statistically insignificant.

Similarly, the other columns of Table 5 show little evidence of economically meaningful heterogeneous effects when we look at: the proportion of jobs in a level 2 industry with no employer-funded training; the proportion of jobs in a level 2 industry with a permanent contract; the proportion of jobs in a level 2 industry lasting five or more months; the seasonality of a level 2 industry; and firm growth in employment over the previous year.[39] Together, these regressions suggest that our previous results were not masking large heterogeneity in effects among firms in different industries and with different characteristics. The results (unreported) are also unchanged when dropping firms in the following government-dominated level 1 industries: public administration and safety; education and training; and health care and social assistance.

39 The estimates for seasonality and firm growth in employment are borderline significant, though small in magnitude. They imply that a standard deviation increase in seasonality is associated with a 2% point increase in the policy effect, which is the opposite sign to our prediction; and a 50% point increase in firm growth is associated with a 1% point decrease in the policy effect ($0.5 \times -0.0253 = -0.01$).

Table 5 Heterogeneous policy effects, with various interaction variables

Dependent variable: No. of new hires interaction variable	Proportion of jobs in lvl 2 industry using a trial period (0–1)	Proportion of jobs in lvl 2 industry with no employer-funded training (0–1)	Proportion of jobs in lvl 2 industry with a permanent contract (0–1)	Proportion of jobs in lvl 2 industry lasting 5+ months, 2005–2008 (0–1)	Seasonality of lvl 2 industry (proportion of max to min hires in a season, March 2005–February 2009)	Firm growth in employment over previous 12 months (decimal)
Between * small firm * interaction variable	**0.135**	**0.070**	**0.273**	**−0.008**	**−0.060***	**−0.025***
	(0.139)	**(0.105)**	**(0.276)**	**(0.107)**	**(0.031)**	**(0.013)**
Post * small firm * interaction variable	0.163	0.040	0.492*	0.078	−0.088***	0.000
	(0.138)	(0.101)	(0.259)	(0.101)	(0.030)	(0.014)
Between * small firm	−0.021	−0.040	−0.238	0.011	0.097*	0.004
	(0.035)	(0.079)	(0.249)	(0.053)	(0.051)	(0.014)
Post * small firm	−0.036	−0.030	−0.443*	−0.038	0.131***	−0.003
	(0.034)	(0.074)	(0.235)	(0.051)	(0.048)	(0.014)
Between * interaction variable	−0.570***	−0.214**	−0.960***	−0.338***	0.077***	0.039***
	(0.113)	(0.086)	(0.238)	(0.094)	(0.026)	(0.010)
Post * interaction variable	0.038	0.091	0.151	0.002	−0.014	0.041***
	(0.115)	(0.083)	(0.231)	(0.089)	(0.025)	(0.012)
Small firm * interaction variable	−0.185**	−0.125*	−0.274	−0.051	0.053***	0.008
	(0.089)	(0.068)	(0.170)	(0.067)	(0.019)	(0.007)
Interaction variable	−15.960**	5.130**	−6.534**	−2.339**	0.567**	0.040***
	(7.516)	(2.351)	(3.114)	(1.124)	(0.273)	(0.006)
Small firm	0.028	0.077	0.233	0.011	−0.092***	−0.013
	(0.024)	(0.050)	(0.154)	(0.034)	(0.031)	(0.012)
Observations	*817,449*	*826,206*	*826,206*	*835,338*	*834,972*	*716,514*
Mean of interaction variable	*0.234*	*0.729*	*0.913*	*0.513*	*1.444*	*0.129*
Standard deviation of interaction variable	*0.081*	*0.115*	*0.050*	*0.124*	*0.410*	*0.826*

Notes: This table replicates and presents key coefficients from our baseline regression from Table A1 in Appendix for firms sized 15–24, and also includes a different interaction variable in each column to look for heterogeneous effects of trial period policy. The sample period is from January 2005 to March 2014. The first column measures trial period use using the 2012 Survey of Working Life (SOWL) while the second and third columns use the 2008 SOWL only. The last four columns use administrative data to form the interaction variable. In constructing the seasonality of level 2 industries, we sum the total number of a season's hires over March 2005–February 2009, and take the ratio of the season with most hires to the season with least hires. A firm has a "recent unfilled separation" if its size is less than it was in any of the previous 3 months. Firm growth in employment over the previous 12 months is calculated up to the last month's firm size. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

5.2 **Types of hires**

Our simple theoretical model predicts that trial period policy will increase the propensity of firms to hire individuals about whom they have less information relative to individuals whose productivity they know more precisely. We expect firms to have less credible information about individuals with less recent New Zealand labor market experience, such as recent education leavers, young people, and migrants. Many workers of unknown quality are also considered disadvantaged or vulnerable in the labor market, and thus the effect of the policy on such workers is of particular interest to policymakers.

In this section, we test the policy effect on the probability a new hire is a worker of unknown quality or a disadvantaged jobseeker of various types using the same basic identification strategy. The regressions here define an observation as a hire, and the dependent variable is an indicator for the hire being of a particular type of unknown quality. Our preferred specifications are ordinary least squares (OLS) regressions, but our results are robust to using probits.[40]

The controls in our preferred specification are the same as described in the previous section, with the addition of plant size (ln):

$$\begin{aligned} disadvant_{ijkmrt} = {} & \alpha_0 + \alpha_1\, small_{jt} + \alpha_4 between_t \times small_{jt} + \alpha_5 post_t \times small_{jt} \\ & + \alpha_6 \ln(firm\_size_{jt}) + \alpha_7 \ln(plant\_size_{jkt}) + \delta_{mr} + \gamma_t + \varepsilon_{ijkmrt} \end{aligned} \tag{8}$$

where *i* denotes individual, *j* denotes firm, *k* denotes industry (which is fixed over time for each firm), *m* denotes calendar month, *r* denotes level 3 industry, and *t* denotes month. Only new hires are potentially eligible to be hired on trial periods, so, as previously, we limit our attention to new hires at firms with 15–24 employees.[41] Here δ_{mr} are industry-calendar month fixed effects, γ_t are fixed effects for each month in our sample period, and ε is the error term. Note that *small* is still defined at the firm level, although here we also control for the size of the plant that hires the worker, because it is the firm size that determines policy eligibility. As discussed earlier, we cluster standard errors at the firm level.

Again, we interpret the coefficient on the interaction *between* × *small firm* as the policy effect, because it shows how the difference in hiring between firms above and below the cutoff changes when trial periods are introduced for firms below the cutoff only.

For a range of definitions of risky or disadvantaged jobseekers, Table 6 presents the results of a set of linear probability regressions in which an observation is a new hire and the dependent variable is an indicator for the employee being a risky jobseeker. The types of risky jobseeker considered are: people who have received benefit income in the past year, people who have received the jobseeker benefit in the past year, people who have not received wage or salary income in the past year, migrants who had their visas approved within the past 2 years, those under 25 years old, Māori or Pasifika under 25 years old, and those who left education in the past year.

In each case, the coefficient on *between*small firm* is small, statistically insignificant, and precisely estimated; there is no evidence that trial periods caused firms to be more likely to hire

40 Probit results are not reported.

41 We also ran regressions that focused on rehires. Results (not presented) were very similar and all small and insignificant.

Table 6 Policy effect on the probability a new hire was a disadvantaged jobseeker

Dependent variable: Indicator for hire type	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Maori or Pasifika under 25	Education leaver
Between * small firm	**0.003**	**0.003**	**0.004**	**−0.003**	**−0.003**	**−0.000**	**−0.001**
	(0.003)	**(0.002)**	**(0.003)**	**(0.004)**	**(0.004)**	**(0.002)**	**(0.002)**
Post * small firm	0.005	0.003	0.005*	−0.001	0.003	0.001	0.002
	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
Small firm	−0.001	−0.001	−0.003	−0.004	0.003	−0.002	0.000
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Firm size (ln)	−0.002	0.001	−0.002	0.009	0.004	−0.004	0.002
	(0.007)	(0.006)	(0.008)	(0.010)	(0.009)	(0.006)	(0.005)
Plant size (ln)	−0.002	0.000	0.004*	0.001	0.013***	0.009***	−0.004***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.001)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*893,292*	*893,292*	*893,292*	*893,292*	*890,352*	*890,352*	*770,415*
% hires of given type	*18.2*	*11.4*	*24.9*	*18.2*	*41.6*	*9.7*	*12.1*
R-squared	*0.030*	*0.031*	*0.035*	*0.092*	*0.122*	*0.027*	*0.010*

Notes: This table presents the coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for the hire being a particular type of disadvantaged jobseeker, as described in the column header. Recent migrants are those who had their visa stamped in the past 2 years. Hires are included only if the firms are of size 15–24 and the employee had not worked for the firm in the previous 5 years. The sample period is from January 2005 to March 2014 (December 2012 in the final column). Age is missing for a small proportion of individuals, which causes the lower observation count in columns 5 and 6. Small firms are those with fewer than 20 employees, which were eligible to use trial periods after the first policy change. Between and Post refer to the time periods relative to the two policy changes. A dummy for the plant having zero employment is also included. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, *$p < 0.10$.

any of these types of workers.[42] For example, column 1 shows people who have earned benefit income in the past 12 months make up 18% of all new hires. The point estimate of the policy effect on the probability a new hire is a former beneficiary is a small and insignificant 0.3% points. The estimated policy effects for the other types of risky hires are similarly small and insignificant. These results are robust to estimation by probit instead of OLS,[43] and to varying the firm size range included in the sample and exclusion of firms very close to the cutoff, as shown in Table A4 in Appendix.[44]

If trial period policy did increase the probability a hire was a risky worker, we are more likely to see this effect in the industries with certain characteristics (such as high trial-period use) and firms with certain characteristics. We thus replicate our analysis of the hiring of risky workers while interacting the policy effect with various interaction variables. Key coefficient estimates are presented in Table A5 in Appendix for different dependent variables and interaction variables.

We do not find evidence of large heterogeneity in the effect on hiring risky jobseekers. For example, the first column of the first panel of Table A5 in Appendix suggests that a standard deviation increase in the trial-period use of an industry is associated with a 0.2% point increase in the policy effect on the probability of hiring a recent beneficiary ($0.09 \times 0.019 = 0.002$). The other columns and panels tend to have similarly small point estimates.

Several point estimates are statistically significant, but they are economically small. For example, the second column of the third panel shows that a standard deviation increase in an industry's permanent contract use is associated with a small 0.4% point increase in the policy effect on the probability a new hire is a recent jobseeker beneficiary ($0.053 = 0.084$). The largest estimate is the fourth column of the second panel, showing that a standard deviation increase in permanent contract use is associated with a 1% point increase in the policy effect on the probability a new hire is a recent migrant. However, the result should be interpreted with caution because the placebo test is also positive and statistically significant; comparing the between period with the post-period gives an estimated interaction effect that is 49% smaller.

5.3 **Stability of employment relationships**

Trial period policy has the potential to affect the stability of employment relationships in several ways. One concern is that it could encourage firms to take employees on for a short period, dismiss them within 90 days, and then hire replacements also for a short period. This could be detrimental to such workers, who would never acquire a measure of job security. However, if these short-term employment opportunities were additional and did not crowd out longer-term jobs, the workers might be better off than in the counterfactual where they remained unemployed.

42 We also ran similar regressions for a range of other definitions of disadvantaged jobseeker: those who had benefit income in the past 2 years or the past 5 years, those with specific types of benefit income (sole parent, or supported living) in the past year, those who had not worked in the past 2 years or the past 5 years, migrants who had their visas approved in the past year or the past 5 years, and youths under 20 years old. Results (not presented) are similar.

43 Results are not reported but are available upon request.

44 In the case of the wide size range sample of firms with 10–50 employees, beneficiary hires and jobseeker beneficiary hires appear statistically significantly more likely with and without excluding firms close to the cutoff. However, the coefficients are small, indicating a 0.4% or 0.5% point increase in the probability a hire is a beneficiary, and the other size ranges do not show the same result. Note also that the estimates for this sample are more likely to be contaminated by differential time trends for very small or large firms.

If trial periods enabled poor employer–employee matches to end quickly, employment relationships that lasted beyond 90 days could actually be more stable subsequently than would have been the case absent trial periods.

Another risk is that trial period policy might have reduced the flexibility of the labor market by discouraging worker mobility; opponents of trial period policy argue that it could discourage people with existing jobs to move to other jobs, because if they were hired with trial periods they would lose their job security. If this effect did occur, we would expect employees who moved straight from one job to another being less likely to move to a firm that was eligible to use trial periods.

In this section, we conduct three separate pieces of analysis. We first test whether new employment relationships at trial period-eligible firms is less likely to last at least 2, 5, 12, or 24 months. We then test whether the policy affected the number of hires into longer-term positions. Finally, we test whether firms able to use trial periods hire fewer workers who come directly from prior employment.

5.3.1 **Duration of employment**

If trial period policy makes firms more likely to dismiss workers who are poor fits for their jobs, we will see an increase in the probability of dismissal in the first 3 months of employment and potentially a decrease in the rate of dismissal at longer employment duration. We analyze the duration of new employment relationships using regressions similar to the hiring-type regressions presented earlier. An observation is again a hire, but here the dependent variable is an indicator for the employment relationship lasting at least a given length of time. We study the policy effect on the probability that a new hire lasted at least 2, 5, 12, or 24 months. The basic controls are as in equation (8), but we also run specifications in which we control for a range of characteristics of the individual, including age, gender, migrant status, work and benefit history, ethnicity, and an indicator for having recently left education. Our sample is individuals hired by firms with between 15 and 24 employees, and standard errors are clustered at the individual level.

Table 7 presents the results of linear probability regressions where the dependent variable is an indicator for a new hire remaining in the job for at least a given length of time: 2, 5, 12, or 24 months.[45] About 77% of all new hires last at least 2 months, 48% last 5 months, 26% last 12 months, and 14% last 24 months.

We cannot know from our data the nature of the employment relationship, for example whether either party believes that the employment is intended to become long term or whether it is for a one-off piece of work. The high proportion of hires that last less than 2 months suggests that many new employer–employee relationships that we identify as hires may never be intended to turn into permanent employment. However, as this is equally true for all firm sizes before, between, and after the policy changes, it should not affect the conclusions drawn from our analysis.

We see from the first row of Table 7 that the coefficients on *between*small firm*, which estimate the policy effect, are all very small and insignificant. For example, the coefficient in the first column is 0.003, which suggests the policy effect was a 0.3% point increase in the

45 Note we refer to duration in terms of the number of calendar months in which the employment relationship existed, because we cannot identify the exact start or end dates. Thus a relationship lasted "at least two calendar months" if the employee was paid by the employer in at least two consecutive calendar months.

Table 7 Policy effect on the distribution of employment duration of new hires

Dependent variable: Indicator for employment lasting at least	2 months	2 months	5 months	12 months	24 months
Between * small firm	**0.003**	**0.002**	**−0.002**	**−0.001**	**−0.002**
	(0.002)	**(0.002)**	**(0.003)**	**(0.002)**	**(0.002)**
Post * small firm	0.007**	0.006**	0.000	−0.003	−0.001
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Small firm	−0.001	−0.003	−0.003	−0.001	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Firm size (ln)	0.026***	0.014**	0.029***	0.025***	0.015***
	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)
Plant size (ln)	−0.015***	−0.013***	−0.019***	−0.009***	−0.005***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Age		0.013***	0.018***	0.015***	0.012***
		(0.000)	(0.000)	(0.000)	(0.000)
Age squared (per 100)		−0.015***	−0.020***	−0.016***	−0.012***
		(0.000)	(0.000)	(0.000)	(0.000)
Female		0.007***	0.006***	−0.006***	−0.011***
		(0.001)	(0.001)	(0.001)	(0.001)
On jobseeker benefit in previous year		−0.026***	−0.071***	−0.070***	−0.051***
		(0.002)	(0.002)	(0.002)	(0.001)
On sole parent benefit in previous year		−0.027***	−0.051***	−0.053***	−0.040***
		(0.003)	(0.003)	(0.003)	(0.002)
On supported living benefit in previous year		−0.056***	−0.090***	−0.082***	−0.056***
		(0.005)	(0.006)	(0.004)	(0.003)
On other benefit type in previous year		−0.031***	−0.055***	−0.055***	−0.035***
		(0.003)	(0.004)	(0.003)	(0.002)
Recent migrant		0.001	−0.033***	−0.040***	−0.032***
		(0.002)	(0.002)	(0.002)	(0.001)
No wage or salary income in previous year		0.017***	0.041***	0.045***	0.034***
		(0.001)	(0.002)	(0.001)	(0.001)
Worked at a different firm the previous month		0.015***	0.046***	0.055***	0.040***
		(0.001)	(0.001)	(0.001)	(0.001)
Maori		−0.012***	−0.028***	−0.027***	−0.020***
		(0.001)	(0.002)	(0.001)	(0.001)
Pasifika		−0.009***	−0.016***	−0.015***	−0.007***
		(0.002)	(0.002)	(0.002)	(0.002)
Asian		0.004*	0.007***	−0.002	−0.005***
		(0.002)	(0.002)	(0.002)	(0.002)
Other ethnicity		−0.006***	−0.012***	−0.010***	−0.008***
		(0.002)	(0.002)	(0.002)	(0.002)
Education leaver (in past year)		0.021***	0.027***	0.021***	0.009***
		(0.001)	(0.002)	(0.002)	(0.001)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes
Calendar month * lvl 3 industry fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	*723,084*	*671,073*	*671,073*	*671,073*	*671,073*
% of hires of given type	*76.8%*	*77.8%*	*48.3%*	*26.0%*	*14.2%*
R-squared	*0.118*	*0.125*	*0.146*	*0.123*	*0.099*

Notes: This table presents the coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for employment lasting at least a given length of time, as described in the column header. Hires are included only if the firm is of size 15–24 and the employee had not worked for the firm in the previous 5 years. The sample period is from January 2005 to April 2012. Small firms are those with fewer than 20 employees, which were eligible to use trial periods after the first policy change. Between and Post refer to the time periods relative to the two policy changes at the date of hire. A dummy for the plant having zero employment is also included. Standard errors, in parentheses, are robust and clustered at the individual level. The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, **$p < 0.05$, *p < 0.10..

probability an employment relationship lasted at least 2 months, relative to an average of 77%. The other coefficients are similarly small and provide no evidence that employees hired by a firm eligible to use trial periods have their tenure with their employer affected by this fact.[46]

For all durations but 2 months, the placebo effect is also tiny and insignificant; for 2 months, the placebo effect is statistically significant at the 5% level, but economically small.

Although we do not find a policy effect on duration of employment, a number of the other controls in our regressions are economically and statistically significant. Hires made by larger firms are slightly more likely to last until each of the milestones. The age profile suggests that young people tend to stay in jobs for shorter periods than middle-aged people, with those in their mid-40s most likely to reach each milestone. Gender differences are statistically significant, but small in magnitude. Previous beneficiaries of all types are substantially less likely to remain in a job for each length of time. Migrants are as likely as locals to remain in a job for 2 months, but less likely to reach 5, 12, or 24 months. Perhaps paradoxically, both those who have not worked in the past year and those who were employed at a different firm the previous month are somewhat more likely to reach each milestone. Ethnic differences are small, though those who report Māori ethnicity (potentially in addition to other ethnicities) are marginally more likely to leave the employment after a short period. Finally, those who recently left education are slightly more likely to stay in employment for longer.

Increased freedom to dismiss new hires could have flow-on effects for hires at the same firm who are not eligible for trial periods. We thus also estimate the effect of the policy on the duration of employment for employees hired by trial-period-eligible firms who have been employed by the same firms previously. These employees are ineligible to be hired on trial periods. Table A6 in Appendix replicates Table 7 for these hires. It estimates that the policy effect on the probability a rehire remained with his employer for 2, 5, 12, or 24 months was approximately zero. Across specifications, the coefficients on *between*small* are between −0.002 and 0.003, indicating a 0.2% point decrease to a 0.3% point increase in the probability the worker survived until the milestone; these values are economically small and statistically insignificant.

Risky jobseekers are probably more likely to be hired on trial periods, and they could be most at risk of repeated short-term employment spells. We thus study the effect of trial periods on duration of employment separately for each type of risky hire. These results are presented in Table A7 in Appendix. They suggest no significant decrease in duration for any type of risky jobseeker.

5.3.2 **Number of long-lasting hires**

Since we might think workers are not harmed if the number of short-term jobs increases provided the number of long-term jobs does not decrease, we also estimate the policy effect on the total number of new employment relationships lasting at least five calendar months, i.e., longer than a trial period.[47] These regressions replicate the quantity of hires regressions, but use as the dependent variable the number of new hires lasting at least 5 months. The sample is firms sized between 15 and 24 employees, and standard errors are clustered at the firm level.

46 We include an extra specification without demographic controls only for the 2-month regression. Results where the dependent variable is a dummy for lasting 5, 12, and 24 months are very similar with and without demographic controls. Our specifications without these controls are available on request.

47 We are unable to observe the exact dates an employee joined and left a firm. Using five calendar months ensures the employee was employed for more than 90 days.

Note separations can occur either because an employee leaves voluntarily, or because he is dismissed, and we are unable to distinguish the two reasons for separation in our data. Employees may have been less likely to leave voluntarily after trial periods were introduced for small firms because moving into a new job could mean a loss of job security. However, any such effect is likely to be similar for employees at firms of sizes 15–19 and those at firms of sizes 20–24, because employees at either size of firm may move to a larger or smaller firm. Around the time of the first policy change, the Global Financial Crisis may have decreased the willingness of employees to leave their employment, but it is expected to have affected employees at large and small firms similarly. Any policy effect we identify here is therefore likely to be driven by a change in employers' dismissal behavior.

Column 1 of Table 8 presents the results of a regression at the firm-month level where the dependent variable is the number of hires into relationships lasting at least 5 months. The coefficient on *between*small firm* is small and insignificant, providing no evidence that long-term hires were affected by the policy; the placebo policy effect, the coefficient on *post*small firm*, has similar magnitude. This confirms more directly our inference that the creation of long-term employment relationships was not significantly affected by the policy change.

5.3.3 **Worker mobility**

We next investigate whether the policy made employees reluctant to leave their existing jobs to take up jobs at trial-period-eligible firms. We do this in the second column of Table 8 by testing

Table 8 Policy effect on long-term hires and employees moving between firms

Dependent variable	New hires who lasted 5+ months	New hires who were employed elsewhere previous month
Between * small firm	**0.006**	**−0.002**
	(0.015)	**(0.017)**
Post * small firm	0.006	−0.003
	(0.014)	(0.015)
Small firm	−0.013	−0.016
	(0.012)	(0.013)
Firm size (ln)	1.000***	0.948***
	(0.037)	(0.041)
Month-in-year fixed effects	Yes	Yes
Calendar month * lvl 3 industry fixed effects	Yes	Yes
Observations	*835,362*	*835,362*
% of nonzero hires of given type	*30.1%*	*29.8%*

Notes: This table presents the coefficients from negative binomial regressions at the firm-month level where the dependent variable is the firm's number of hires as described in each column header. New hires are those who had not worked for the firm in the previous 5 years. The sample period is from January 2005 to March 2014, and firms included are those sized 15–24. Small firms are those with fewer than 20 employees, which were eligible to use trial periods after the first policy change. Between and Post refer to the time periods relative to the two policy changes. Standard errors, in parentheses, are robust and clustered at the firm level. The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$.

whether policy-eligible firms hired fewer employees who had come directly from another job.[48] The dependent variable is the number of new hires who worked at a different firm the month before they were hired. The sample is firms with between 15 and 24 employees, and standard errors are clustered at the firm level. The small and insignificant coefficient on *between*small firm* suggests firms that were eligible for trial periods did not find this inhibited their hiring of employees directly out of existing employment.

A number of explanations could explain this lack of finding. Most simply, employees might not be aware of trial periods or might not feel that their new jobs are genuinely at risk even if they are hired with a trial period. Alternatively, previously employed workers may be able to negotiate being hired without a trial period even at firms that standardly use them, or could be offered remuneration high enough to compensate for their temporarily insecure employment. Testing for either of these latter mechanisms is beyond the scope of the current research, though the surveys discussed in Section 2.3 suggest that trial periods are typically non-negotiable in practice; when a firm decides to use a trial period, the job offer is conditional on accepting it.

5.4 **Temporary contract substitution**

One potential explanation for our lack of significant policy effect on essentially any of the margins we examine is that firms previously used temporary contracts to test new employees of unknown quality, and merely switched to using trial periods for this purpose when they became available. The use of temporary contracts as a method of reducing employment protection has been highlighted by Bentolila et al. (2019) in their discussions of dual labor markets. Although using temporary contracts to trial new employees is technically illegal, this may neither be well-known among employees and small employers, nor be widely enforced. If employers did use temporary contracts in this way, dismissal costs may have changed little, and we would expect trial period policy to have had little effect on firm hiring behavior. To explore the possible substitution between trial periods and temporary contracts, we use data from the 2008 and 2012 SOWL, which includes information on contract type, to look at the relationships between the types of worker–firm matches likely to involve temporary contracts pre-policy, and those likely to involve trial periods or temporary contracts post-policy.

First, we run a hire-level regression in which we regress an indicator for being hired on a temporary contract pre-policy (2008) on various employee and job characteristics. Using SOWL data from the post-period (2012), we then use these results to predict the probability each new employee would have been on a temporary contract had she been hired in the pre-period. Finally, we use this predicted probability as an explanatory variable in hire-level regressions using post (2012) data only in which the dependent variable is either an indicator for being hired on a temporary contract or an indicator for being hired with a trial period.

In the final stage regression predicting being hired on a temporary contract, a coefficient on temporary contract probability close to 1 would indicate that the types of workers being

48 We are unable to identify an employee's reason for leaving prior employment.

hired on temporary contracts are similar before and after trial periods were available; a coefficient much smaller than 1 would indicate a change in the use of temporary contracts. In the final stage regression predicting being hired on a trial period, a positive coefficient on temporary contract probability would indicate the characteristics that pre-policy made a worker likely to be hired on a temporary contract made him likely to be hired on a trial period post-policy. In combination, a coefficient much smaller than 1 in the final stage temporary contract regression and a positive coefficient in the final stage trial period regression would suggest a degree of substitution between trial periods and temporary contracts.

Columns 1 and 2 of Table A8 in Appendix present results from the first type of regression, showing the characteristics associated in the pre-period with a new employee being hired on a temporary contract. We use logit regressions where the dependent variable is an indicator for being on a temporary contract in 2008, and the covariates are measures of age, gender, qualification levels, and ethnicity, and in column 2 also level 1 industry and level 1 occupation fixed effects.[49, 50]

The first two columns of Table 9 regress an indicator for a 2012 employee being on a temporary contract on her predicted probability of being on a temporary contract in the pre-period from either the parsimonious (column 1) or full control (column 2) model in Table A8 in Appendix. The point estimate of 1.156 in column 1 means a 10% point increase in the predicted probability of being on a temporary contract based on 2008 relationships is associated with a 15.6% point increase in the probability of being on a temporary contract in 2012; we cannot reject that this coefficient is equal to 1. Column 2 tells a qualitatively similar story. These results suggest that the characteristics associated with being on a temporary contract are similar in 2008 and 2012, i.e., before and after trial periods are available.

Columns 3 and 4 instead use as the dependent variable an indicator for being on a trial period in 2012. The coefficients estimates here are negative, large, and statistically significant. For example, column 4 suggests that a 10% point increase in the predicted probability of having a temporary contract in 2008 (based on individual and job characteristics) is associated with a 12.3% point *decrease* in the probability of being hired on a trial period in 2012. That is, the characteristics that made a person likely to be on a temporary contract in 2008 are strongly negatively correlated with them being hired with a trial period in 2012.

As additional verification for these results, we use a similar methodology to look at whether the characteristics that predict being hired with a trial period in 2012 predict being hired on a temporary contract in 2008. The first and second set of regressions for this analysis are given in columns 3 and 4 of Table A8 in Appendix and the columns 5 and 6 of Table 9, respectively. The point estimates of −0.270 and −0.521 in the second set are again negative and statistically significant.

Together, these results suggest the types of jobs that used temporary contracts in 2008 tended to still use temporary contracts in 2012; the types of jobs that used temporary contracts in 2008 did not tend to use trial periods in 2012; and the types of jobs that used trial periods

49 We use logit because the mean of the dummy dependent variable in columns 1 and 2 is around 0.2, so OLS results in a number of predicted probabilities outside the range [0,1]. However, results are similar when we instead use OLS (unreported).

50 We classify a contract as temporary if it is fixed-term, seasonal, or casual. Casual employment is not explicitly defined in New Zealand employment legislation, but refers to an agreement with no guaranteed hours of work and no ongoing expectation of employment. See https://www.employment.govt.nz/starting-employment/who-is-an-employee/types-of-employee/ for details.

Table 9 Substitution between trial periods and temporary contracts

Dependent variable	On temporary contract, 2012	On temporary contract, 2012	On trial period, 2012	On trial period, 2012	On temporary contract, 2008	On temporary contract, 2008
Predicted probability of temporary contract from 2008 data, parsimonious	1.156***		−1.436***			
	(0.220)		(0.223)			
Predicted probability of temporary contract from 2008 data, full controls		1.364***		−1.230***		
		(0.151)		(0.147)		
Predicted probability of trial period from 2012 data, parsimonious					−0.270**	
					(0.135)	
Predicted probability of trial period from 2012 data, full controls						−0.521***
						(0.086)
Year	2012	2012	2012	2012	2008	2008
Observations	2,397	2,385	2,397	2,385	1,686	1,686
R-squared	0.014	0.043	0.015	0.025	0.003	0.023
Mean of dependent variable	0.207	0.207	0.376	0.375	0.200	0.200

Notes: This table presents results from OLS regressions at the job level, exploring how being on different contract types correlates with the predicted probability of being hired on a certain contract type from a separate survey year. The dependent variable in each column is given in the column header. Predicted probabilities use the individual characteristics and job characteristics of each observation in the sample, and use the model of Table A8 in Appendix for the prediction. The sample is limited to those hired in the previous year. Standard errors, in parentheses, are robust.

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

in 2012 did not tend to use temporary contracts in 2008. These results are inconsistent with temporary contracts in 2008 being used extensively to trial new employees and subsequently being replaced by trial periods in this purpose.[51]

5.5 Limitations and caveats to interpretation

Our empirical strategy provides the cleanest identification of the policy effect for firms with nearly 20 employees. However, one possibility is that very small firms (or very large firms, though this is less likely to be the case) were affected more by trial period policy than were firms with nearly 20 employees. Human resource costs related to hiring and dismissals are generally more of a burden for smaller firms, so it is a plausible hypothesis that very small firms were affected more by the policy. Our main specifications do not estimate the effect on firms with fewer than 15 employees. However, we run several robustness checks where we attempt to identify the policy effect on smaller firms. First, we expand the firm size range in our regressions out to 10–50 employees. Second, we expand the size range from 1 to 29 employees, and replace our small firm dummy and its interactions with a set of indicator variables for the size bands in this range (1–4, 5–9, 10–14, 15–19, and 25–29, omitting the category 20–24) and their interactions. None of these regressions suggest a significant policy effect on small firms, though the evidence they provide should thus be interpreted as suggestive only.

There are several ways in which the hiring behavior of treated (small) firms could affect the hiring behavior of control (large) firms that should be borne in mind when interpreting our results. These arise from the fact that both types of firms hire from the same pool of jobseekers. Supposing the first policy change caused small firms to increase their hiring, the additional workers hired by small firms would therefore not be available to be hired by large firms. This could actually decrease hiring by large firms if other jobseekers were not perfect substitutes for those who were made unavailable by the policy. Over several years of elevated hiring by small firms, this effect could have been accentuated.[52] Similarly, if treated firms increased their hiring of a particular type of disadvantaged worker, there would be fewer good workers of this type to be hired by control firms. The pool of jobseekers could also have been affected by increased employee reluctance to leave secure employment or decreased employer reluctance to dismiss current employees.

All these potential mechanisms suggest that control firms may have actually been treated by the first policy change in a way that would cause us to overestimate the effect of the policy on treated firms. However, the magnitude of any of these effects on large firms is likely to be much smaller than the policy effect on small firms, so should not be of material importance in the absence of very large policy effects on small firms. In practice, we find policy effects close to zero throughout the article.

51 This is also consistent with estimated aggregates from the 2008 and 2012 Survey of Working Life; a higher proportion of jobs were on temporary contracts in 2012 than 2008, despite trial periods not being available in 2008. It is difficult to reconcile this with a story of widespread crowding-out of temporary contracts due to trial periods. Although we note that the surveys are not completely comparable because the 2008 survey was in March while the 2012 survey was in December. December is the summer season in New Zealand, with many temporary workers.

52 Through a similar mechanism, if small firms increased their hiring substantially soon after the first policy change, they could have depleted the pool of desirable jobseekers, causing their own hiring at a later date to be lower.

Finally, our regressions that investigate quantity of hiring test for an effect of the policy on number of monthly hires for a firm of given size. Supposing the policy instead caused a one-time increase in firm size, our analysis may not be able to pick up this effect.[53]

6 Conclusions

In this research, we estimate how the option of using trial periods has affected the quantity of hiring by firms, the types of individuals hired, and the stability of employment relationships.

We find that the policy had little to no effect on the quantity of hiring by firms on average across industries. We also find that trial period policy had no significant effect on the probability that an individual hired was young, a recent education leaver, a recent migrant, a recent beneficiary, young and Māori or Pasifika, or a person who had not worked in the preceding year. That is, these types of workers with less labor market exposure did not seem to disproportionately benefit from (or pay the employment costs of) the policy.

We investigate whether the policy affected the duration of employment relationships and find no evidence of this overall. Finally, we find no evidence that employees moving between jobs were less likely to move to trial-period-eligible firms; it does not appear that the policy decreased the willingness of workers to change jobs.

The availability of trial periods would not be expected to significantly affect firm hiring decisions if trial periods were not widely used, and many reasons exist why firms might not use them. First, trial periods might not be used if they do not genuinely reduce dismissal costs, either through enabling simplified dismissal procedures or by reducing the legal risk from dismissal. For instance, trial periods might not reduce dismissal costs if there were substantial uncertainty about how courts would apply the new trial period law in practice. Second and relatedly, firms may not use trial periods if they do not *perceive* them as lowering the risk or cost of dismissal. However, we provide evidence from surveys of firms that trial periods are perceived as reducing dismissal costs.

Third, firms may not use trial periods if employees react negatively to them. This could be through walking away from a job offer with a trial period clause, or accepting the offer but having lower morale or employer loyalty, which could result in lower effort and productivity on the job. Fourth, if recruitment processes are very costly or new hires require expensive training, firms may get little value from the ability to dismiss a recent hire. Finally, employers may not use trial periods if they gain little useful information about employee productivity from the trial period. This may be due to employers being able to accurately estimate a worker's productivity based on the interview process alone, or because employees exert higher effort during their trial periods than after they gain greater job security.

Despite the potential reasons not to use trial periods, in 2012 we find a substantial 37.6% of employees were hired with a trial period, which is 47.4% of employees hired on a permanent contract. This shows that the lack of effect of trial period policy was not due to firms not using trial periods.

Firms that use trial periods may do so for a range of reasons. The reason we argue is most likely to test the employee in the role before committing to hiring her permanently. However,

53 Although Figure 6 does not suggest this was the case because it does not shown even a short-term increase in quantity of hiring.

trial periods could also be used to increase the employer's ability to adjust its labor input in response to shocks, to motivate employees in exert more effort, or to dissuade employees from undesirable workplace behaviors. We argue that these latter motivations are less likely to drive firm trial period use because they function for the first 90 days of employment only. One way around this would be for an employer to serially hire new employees and dismiss most before the end of their trial periods. However, our empirical tests show no evidence that short-term hiring increases where trial periods are available.

We interpret our results as showing that any effect of trial period policy in New Zealand on firm hiring or dismissal behavior has been economically tiny at the economy level, despite a substantial proportion of employees being hired with trial periods and firms believing that trial periods reduce dismissal costs. It seems that the primary effects of the policy were to reduce the cost to firms of continuing their pre-policy behavior, while requiring many employees to shoulder the cost of an increase in perceived initial uncertainty about their job security. However, we find no evidence that actual job security decreased. The main burden to employees may thus be the psychological cost of lower perceived security, and this cost could fall in the long term as employees learn that job insecurity has not increased significantly.

There are a number of possible explanations for the overall lack of policy effect: the policy may not have reduced dismissal costs as much as policymakers believe; 90 days may not be long enough for employers to evaluate new employees; employees might exert higher effort during their trial periods, making trials ineffectual at informing employers of employees' long-term productivity; and before trial periods being available, firms had at their disposal several alternative types of temporary employment arrangements that allowed employers to evaluate hires before committing to permanent employment relationships.[54] Furthermore, a 2016 survey estimates that 8.6% of New Zealand employees report working without a written employment contract.[55] These percentages are even higher when looking at seasonal and other temporary workers (15–44%). However, our analysis suggests that trial periods were not used as substitutes for temporary contracts and survey results suggest that firms are enthusiastic about trial periods and believe that they allow them to cheaply dismiss workers who turn out to be poor fits for their jobs.

In some cases, high training or recruitment costs for new employees might make firms reluctant to dismiss new employees who turn out not to be good matches because they will incur these costs again for any replacement hire, and they risk facing the same issue again. In instances when the employee turns out to be an extremely bad match for the position, the firm may dismiss him regardless of whether he is on a trial period.[56] To the extent that this is why trial periods have little effect, it suggests that there are limits on how effective a policy that changes short-term dismissal costs can be at affecting hiring behavior, and alternative policies such as subsidies for training might be necessary to boost hiring.

54 Although the Employment Relations Act (2000) explicitly prohibits use of fixed-term contracts to test employees for suitability for permanent employment, knowledge of this prohibition among employers and employees could well be low, and this may happen in practice.

55 http://www.stats.govt.nz/browse_for_stats/income-and-work/employment_and_unemployment/improving-labour-market-statistics/union-memship-emplymt-agmt.aspx

56 We use SOWL data to estimate whether the effects of trial periods on hiring were larger in industries with more employer-funded employee training, but estimates are insufficiently precisely estimated to draw any conclusions.

Declarations

Availability of data and materials

This article uses confidential unit record data that can be accessed only through Statistics New Zealand (SNZ) by approved researchers working at secure locations. If the article were accepted, we would be able to provide all the code required to reproduce our results from the data held by SNZ and could advise any interested researchers on the application process for accessing the data.

Competing interests

The authors declare that they have no competing interests.

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Authors' contributions

IS and NC together extracted and cleaned the data, analyzed the data, and wrote the paper. Both authors have read and approved the final manuscript.

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Appendix

Proof of Proposition 3.2.1. The fact applicant *b* is hired when there are no trial periods means $\mu_b > \mu_a$, and the expected NPV of hiring *b* without a trial period is positive and greater than the expected value of hiring *a* without a trial period. Thus the firm will definitely hire one candidate, and if a candidate is hired *without* a trial period it will be candidate *b*.

Note that the expected value of hiring an employee with a trial period is increasing in both μ and σ:

$$E[NPV^{hire\_trial}] = \mu - c + (1 - \Psi(0))\frac{1+\delta}{\delta}\left(\mu + \sigma\frac{\phi(\frac{-\mu}{\sigma})}{1-\Phi(\frac{-\mu}{\sigma})}\right),$$

thus

$$\frac{\delta(E[NPV^{hire\_trial}])}{\delta\sigma} = \frac{(1+\delta)e^{\frac{-\mu^2}{2\sigma^2}}}{\sqrt{2\pi\delta}} > 0$$

and

$$\frac{\vartheta(E[NPV^{hire\_trial}])}{\vartheta\mu} = 1 + 3\delta + (1+\delta)erf[\tfrac{\mu}{\sqrt{2}\sigma}]$$

$\geq 2\delta > 0$ because $erf[x] \geq -1$ $(x \in \mathbb{R})$

Hence the expected value of hiring *b* with a trial period is higher than that of hiring *a* with a trial period, and if a candidate is hired with a trial period it will be candidate *b*. QED

Proof of Proposition 3.2.2.

$$\lim_{\sigma\to\infty}\frac{\partial(E[NPV^{hire\_trial}])}{\partial\sigma} = \frac{(1+\delta)}{\sqrt{2\pi\delta}}$$

thus $E[NPV^{hire\_trial}]$ increases without limit as σ increases. Thus, because $E[NPV^{hire\_trial}]$ is finite for all $\mu \in \mathbb{R}$ and positive σ and the support of σ_b is unbounded above, there exists sufficiently large σ_b such that $E[NPV^{hire\_trial}(\mu_a, \sigma_b)] > E[NPV^{hire\_trial}(\mu_a, \sigma_a)]$ and $E[NPV^{hire\_trial}(\mu_b, \sigma_b)] > \mu_a$. QED

Proof of Proposition 3.2.3. Since neither applicant is hired when there are no trial periods, we know μ_a and μ_b are both negative, and thus the hire when trial periods are available will be a hire using a trial period. Since the derivatives of the expected NPV of hiring with a trial period with respect to μ and σ are both positive, if applicant *b* also has higher mean then she will be hired. By the independence of the distributions of the means and standard deviations, this occurs 50% of the time covered by this proposition. From the proof of Proposition 3.2.2, if applicant *a* has a higher mean, there are some combinations of mean and variance such that *b* is hired. Thus the overall probability that *b* is hired in this case is greater than 0.5. QED

Table A1 Policy effect on the number of new hires, varying firm sizes included

Dependent variable	Number of new hires				
Firm sizes included:	18–21	17–22	16–23	15–24	10–50
Between * small firm	**0.008**	**0.009**	**0.009**	**0.008**	**0.012**
	(0.019)	**(0.017)**	**(0.015)**	**(0.014)**	**(0.009)**
Post * small firm	−0.005	−0.002	0.003	0.000	0.010
	(0.017)	(0.015)	(0.014)	(0.013)	(0.010)
Small firm	−0.014	−0.000	−0.018	−0.014	−0.016*
	(0.016)	(0.013)	(0.012)	(0.011)	(0.009)
Observations	*314,637*	*478,503*	*650,934*	*835,362*	*2,495,838*
Firm sizes included:	17–18, 21–22	16–18, 21–23	15–18, 21–24	10–18, 21–50	
Between * small firm	**0.030**	**0.024**	**0.019**	**0.016***	
	(0.020)	**(0.017)**	**(0.015)**	**(0.010)**	
Post * small firm	0.010	0.012	0.006	0.013	
	(0.018)	(0.016)	(0.015)	(0.010)	
Small firm	0.032	−0.032	−0.020	−0.019*	
	(0.028)	(0.019)	(0.016)	(0.010)	
Observations	*322,119*	*494,553*	*678,978*	*2,339,457*	
Firm sizes included:	16–17, 22–23	15–17, 22–24	10–17, 22–50		
Between * small firm	**0.009**	**0.006**	**0.014**		
	(0.020)	**(0.017)**	**(0.010)**		
Post * small firm	0.008	0.001	0.013		
	(0.019)	(0.017)	(0.010)		
Small firm	−0.126***	−0.043*	−0.022*		
	(0.039)	(0.025)	(0.012)		
Observations	*336,297*	*520,725*	*2,181,201*		

Notes: This table presents the coefficients from negative binomial regressions at the firm-month level where the dependent variable is the firm's number of new hires. New hires are those who had not worked for the firm in the previous 5 years. All regressions also control for log firm size, month-in-year fixed effects, and calendar month * level 3 industry fixed effects. The sample period is from January 2005 to March 2014. Small firms are those with fewer than 20 employees at the start of the month, which were eligible to use trial periods after the first policy change. Between and Post refer to the time periods relative to the two policy changes. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, *$p < 0.10$.

Table A2 Test for parallel trends in the pre-announcement period

Dependent variable	Number of new hires
Time trend * small firm	**0.0002**
	(0.0005)
Time trend	−0.0087
	(0.0054)
Small firm	−0.0143
	(0.0181)
Log of firm size	0.9632***
	(0.0448)
Calendar month * level 3 industry fixed effects	Yes
Observations	359,046

Notes: This table presents coefficients from negative binomial regressions at the firm-month level for firms with 15–24 employees, as in Table A2 in Appendix, but limits the sample period from January 2005 to November 2008, includes a linear time trend, and interacts the linear time trend with the *small firm* indicator. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$.

Table A3 Policy effect on the total number of new hires, robustness checks

Robustness check	Excluding anomalous industries	Excluding firm months with extremely high hiring	Measuring firm size excluding all working proprietors	Interact *small firm* with GDP	OLS regression	OLS regression	OLS regression
Dependent variable	Number of new hires	Number of new hires	Number of new hires	Number of new hires	Dummy for any new hires	Log (new hires + 1)	Percentage change in employment (%)
Between * small firm	**0.018**	**0.012**	**0.005**	**0.016**	**0.006**	**0.019*****	**−0.093**
	(0.015)	**(0.012)**	**(0.012)**	**(0.017)**	**(0.004)**	**(0.005)**	**(0.093)**
Post * small firm	0.006	0.001	0.002	0.010	0.004	0.013***	0.021
	(0.014)	(0.011)	(0.012)	(0.024)	(0.004)	(0.005)	(0.083)
Small firm	−0.021*	0.001	−0.008	0.023	−0.002	−0.011***	−0.045
	(0.012)	(0.009)	(0.010)	(0.082)	(0.003)	(0.004)	(0.083)
Firm size (ln)	0.952***	1.023***	0.941***	0.930***	0.258***	0.359***	−0.360
	(0.036)	(0.028)	(0.030)	(0.034)	(0.009)	(0.011)	(0.236)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Small firm*quarterly GDP interaction				Yes			
Observations	*781,725*	*827,883*	*810,189*	*835,362*	*835,362*	*835,362*	*824,547*
% with nonzero-dependent variable	*45.8%*	*46.1%*	*61.6%*	*46.6%*	*46.6%*	*46.6%*	*62.6%*

Notes: The first four columns of this table present the coefficients from negative binomial regressions at the firm-month level where the dependent variable is the firm's number of hires who had not worked for the firm in the previous 5 years. For firms sized 15–24 only. Columns 5–7 change the estimation method to OLS, with the dependent variable described in the column headers. The regressions replicate those in the fourth column in Panel A of Table A1 in Appendix except as noted in the column headers. The dropped industries in column 1 are central government administration and school education. Firm months whose number of hires is more than half their start-of-month employment are dropped in column 2. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, *$p < 0.10$.

Table A4 Policy effect on the probability a new hire was a disadvantaged jobseeker, varying firm-size band

Dependent variable: Indicator for hire type	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Maori or Pasifika under 25	Education leaver
Firms sized 18–21							
Between * small firm	**0.001**	**0.004**	**0.004**	**0.005**	**−0.005**	**−0.007***	**−0.004**
	(0.004)	**(0.004)**	**(0.005)**	**(0.006)**	**(0.005)**	**(0.003)**	**(0.003)**
Post * small firm	0.001	0.003	0.002	0.006	−0.009*	−0.006**	0.004
	(0.004)	(0.003)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)
Small firm	0.001	−0.002	0.003	−0.009*	0.011**	0.001	0.003
	(0.004)	(0.003)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Observations	*350,265*	*350,265*	*350,265*	*350,265*	*349,173*	*349,173*	*302,172*
Firms sized 10–50							
Between * small firm	**0.005****	**0.004****	**−0.001**	**0.001**	**0.001**	**0.002**	**0.001**
	(0.002)	**(0.002)**	**(0.002)**	**(0.003)**	**(0.003)**	**(0.002)**	**(0.001)**
Post * small firm	0.001	−0.000	0.004*	0.004	0.002	−0.001	0.003**
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.001)
Small firm	−0.001	−0.000	0.002	−0.002	0.002	−0.002	−0.000
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)
Observations	*2,728,668*	*2,728,668*	*2,728,668*	*2,728,668*	*2,719,995*	*2,719,995*	*2,352,420*
Firms sized 15–24, excluding 19–20							
Between * small firm	**0.005**	**0.004**	**0.002**	**−0.006**	**−0.002**	**0.002**	**−0.000**
	(0.003)	**(0.003)**	**(0.004)**	**(0.005)**	**(0.004)**	**(0.003)**	**(0.002)**
Post * small firm	0.005	0.003	0.006*	−0.003	0.006	0.003	−0.000
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.002)	(0.002)

(*continued*)

Table A4 (Continued)

Dependent variable: Indicator for hire type	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Maori or Pasifika under 25	Education leaver
Small firm	−0.003	−0.000	−0.005	−0.001	−0.000	−0.003	−0.001
	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
Observations	*718,575*	*718,575*	*718,575*	*718,575*	*716,181*	*716,181*	*619,950*
Firms sized 10–50, excluding 18–21							
Between * small firm	**0.005****	**0.004****	**−0.002**	**0.001**	**0.002**	**0.004****	**0.002**
	(0.002)	**(0.002)**	**(0.002)**	**(0.004)**	**(0.003)**	**(0.002)**	**(0.001)**
Post * small firm	0.001	−0.001	0.004**	0.004	0.003	−0.001	0.004**
	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.001)
Small firm	−0.001	−0.001	0.004**	−0.001	0.001	−0.002	−0.001
	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.001)
Observations	*2,378,403*	*2,378,403*	*2,378,403*	*2,378,403*	*2,370,822*	*2,370,822*	*2,050,248*

Notes: This table presents key coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for the hire being a particular type of disadvantaged jobseeker, as described in the column header. The panels of the table replicate Table 6 for different samples of firm size. See the notes to Table 6 for further details and to see the additional controls included. Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

**$p < 0.05$, *$p < 0.10$.

Table A5 Heterogeneous policy effects on the probability a new hire was a disadvantaged jobseeker

Dependent variable: Indicator for hire type	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Maori or Pasifika under 25 years old	Education leaver
Panel A: Trial period use, employee training, permanent contract use, and long-term jobs							
Between * small firm * proportion of jobs starting on trial period	**0.019**	**0.023**	**−0.058****	**−0.028**	**0.001**	**−0.008**	**−0.046****
	(0.023)	**(0.019)**	**(0.026)**	**(0.022)**	**(0.028)**	**(0.018)**	**(0.020)**
Post * small firm * proportion of jobs starting on trial period	0.048**	0.046***	−0.023	−0.075***	−0.012	0.010	−0.022
	(0.021)	(0.017)	(0.023)	(0.020)	(0.025)	(0.016)	(0.022)
Observations	*873,837*	*873,837*	*873,837*	*873,837*	*870,972*	*873,837*	*754,212*
Mean proportion of jobs in industry on a trial period	*0.225*	*0.225*	*0.225*	*0.225*	*0.225*	*0.225*	*0.224*
Between * small firm * proportion of jobs with no training	**−0.005**	**−0.007**	**−0.002**	**−0.062*****	**0.034**	**0.004**	**−0.012**
	(0.017)	**(0.014)**	**(0.019)**	**(0.017)**	**(0.021)**	**(0.013)**	**(0.015)**
Post * small firm * proportion of jobs with no employer-funded training	−0.004	0.002	−0.006	−0.042***	0.020	−0.002	−0.001
	(0.016)	(0.013)	(0.018)	(0.015)	(0.019)	(0.012)	(0.017)
Observations	*884,805*	*884,805*	*884,805*	*884,805*	*881,901*	*884,805*	*763,449*
Mean proportion of jobs with no employer-funded training	*0.744*	*0.744*	*0.744*	*0.744*	*0.744*	*0.744*	*0.743*
Between * small firm * proportion of jobs starting on permanent contract	**0.059**	**0.084****	**0.031**	**0.185*****	**−0.035**	**0.019**	**−0.086****
	(0.041)	**(0.034)**	**(0.046)**	**(0.040)**	**(0.051)**	**(0.032)**	**(0.035)**
Post * small firm * proportion of jobs starting on permanent contract	0.089**	0.062**	−0.010	0.094***	−0.039	0.055**	−0.076**
	(0.036)	(0.029)	(0.040)	(0.035)	(0.044)	(0.027)	(0.038)
Observations	*884,805*	*884,805*	*884,805*	*884,805*	*881,901*	*884,805*	*763,449*
Mean proportion of jobs starting on permanent contract	*0.891*	*0.891*	*0.891*	*0.891*	*0.891*	*0.891*	*0.891*
Between * small firm * proportion of jobs lasting 5+ months	**0.033****	**0.041*****	**−0.008**	**0.053*****	**−0.020**	**0.013**	**−0.013**
	(0.015)	**(0.012)**	**(0.017)**	**(0.014)**	**(0.018)**	**(0.011)**	**(0.013)**
Post * small firm * proportion of jobs lasting 5+ months	0.045***	0.031***	0.011	0.013	−0.029*	0.019*	−0.018
	(0.013)	(0.011)	(0.015)	(0.013)	(0.016)	(0.010)	(0.014)

(*continued*)

Table A5 (Continued)

Dependent variable: Indicator for hire type	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Maori or Pasifika under 25 years old	Education leaver
Observations	*893,259*	*893,259*	*893,259*	*893,259*	*890,319*	*893,259*	*770,415*
Mean proportion of jobs lasting 5+ months	*0.448*	*0.448*	*0.448*	*0.448*	*0.448*	*0.448*	*0.449*
Panel B: Seasonality and firm growth							
Between * small firm * seasonality of industry	**−0.003**	**−0.007***	**0.001**	**−0.001**	**−0.000**	**−0.001**	**0.006**
	(0.005)	**(0.004)**	**(0.005)**	**(0.004)**	**(0.006)**	**(0.004)**	**(0.004)**
Post * small firm * seasonality of industry	−0.010**	−0.008**	0.002	0.009**	−0.002	−0.005*	0.008**
	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)
Observations	*893,040*	*893,040*	*893,040*	*893,040*	*890,100*	*893,040*	*770,241*
Mean seasonality of industry	*1.578*	*1.578*	*1.578*	*1.578*	*1.578*	*1.578*	*1.578*
Standard deviation of seasonality of industry	*0.472*	*0.472*	*0.472*	*0.472*	*0.472*	*0.472*	*0.472*
Between * small firm * firm growth over previous 12 months	**−0.000**	**−0.001**	**0.005****	**−0.003***	**−0.001**	**−0.001**	**−0.000**
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.001)**	**(0.002)**
Post * small firm * recent firm growth	0.005**	0.003*	0.004*	−0.001	0.003	−0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Observations	*720,147*	*720,147*	*720,147*	*720,147*	*717,873*	*720,147*	*605,694*
Mean firm growth over previous 12 months	*0.198*	*0.198*	*0.198*	*0.198*	*0.198*	*0.198*	*0.195*
Standard deviation of Firm growth over previous 12 months	*1.386*	*1.386*	*1.386*	*1.386*	*1.382*	*1.386*	*1.417*

Notes: This table presents key coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for the hire being a particular type of disadvantaged jobseeker, as described in column headers. Regressions are the same as those in Table 6, except each panel includes the interaction of the policy effect and placebo effect with a modifying variable. The modifying variables in the first five panels are at the level 2 industry level. See Table 5 for further descriptions of the modifying variables, and Table 6 for definitions of hire types. Hires are included only if the firm is of size 15–24 and the employee had not worked for the firm in the previous 5 years. The sample period is from January 2005 to March 2014 (December 2013 in the final column). Standard errors, in parentheses, are robust and clustered at the firm level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table A6 Policy effect on the distribution of employment duration of rehires

Dependent variable: Indicator for employment lasting at least	2 months	5 months	12 months	24 months
Between * small firm	**0.002**	**−0.002**	**−0.001**	**−0.002**
	(0.002)	**(0.003)**	**(0.002)**	**(0.002)**
Post * small firm	0.006**	0.000	−0.003	−0.001
	(0.003)	(0.004)	(0.003)	(0.003)
Small firm	−0.003	−0.003	−0.001	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Employee demographic controls	Yes	Yes	Yes	Yes
Month-in-year fixed effects	Yes	Yes	Yes	Yes
Calendar month * level 3 industry fixed effects	Yes	Yes	Yes	Yes
Observations	*390,963*	*390,963*	*390,963*	*390,963*
% hires of given type	*61.9%*	*35.1%*	*15.3%*	*8.4%*
R-squared	*0.121*	*0.181*	*0.077*	*0.061*

Notes: This table presents key coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for employment lasting at least a given length of time, as described in the column header. Hires are included only if the firm is of size 15–24 and the employee *had* worked for the firm in the previous 5 years. The table replicates Table 7 with the same covariates in each column, but observations are individuals who had previously been employed by the same firm rather than the converse. Standard errors, in parentheses, are robust and clustered at the individual level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

**$p < 0.05$.

Table A7 Policy effect on the distribution of employment duration of different hire types

Dependent variable: Indicator for employment lasting at least:	2 months	5 months	12 months	24 months
Panel A: Beneficiaries, jobseekers, nonworkers, and migrants				
Recent beneficiary hires				
Between * small firm	**−0.002**	**−0.002**	**−0.001**	**−0.001**
	(0.005)	**(0.006)**	**(0.005)**	**(0.004)**
Post * small firm	0.006	−0.008	−0.012*	−0.005
	(0.007)	(0.008)	(0.007)	(0.005)
Small firm	−0.002	0.004	0.005	0.002
	(0.005)	(0.006)	(0.005)	(0.003)
Observations	*129,507*	*129,507*	*129,507*	*129,507*
Recent jobseeker hires				
Between * small firm	**−0.001**	**−0.004**	**0.004**	**0.003**
	(0.007)	**(0.008)**	**(0.006)**	**(0.005)**
Post * small firm	0.002	−0.013	−0.010	−0.008
	(0.008)	(0.010)	(0.008)	(0.006)
Small firm	0.005	0.009	0.006	0.007
	(0.006)	(0.007)	(0.006)	(0.004)
Observations	*80,904*	*80,904*	*80,904*	*80,904*
Recent nonworking hires				
Between * small firm	**−0.000**	**−0.003**	**−0.002**	**−0.002**
	(0.005)	**(0.006)**	**(0.005)**	**(0.004)**

(*continued*)

Table A7 (Continued)

Dependent variable: Indicator for employment lasting at least:	2 months	5 months	12 months	24 months
Post * small firm	0.010	0.001	−0.009	−0.005
	(0.006)	(0.007)	(0.007)	(0.005)
Small firm	−0.002	−0.006	0.003	0.000
	(0.004)	(0.005)	(0.005)	(0.004)
Observations	*147,909*	*147,909*	*147,909*	*147,909*
Recent migrant hires				
Between * small firm	**0.004**	**0.003**	**−0.007**	**−0.005**
	(0.006)	**(0.008)**	**(0.006)**	**(0.005)**
Post * small firm	−0.002	−0.015	−0.018**	−0.013**
	(0.008)	(0.010)	(0.008)	(0.007)
Small firm	0.002	0.008	0.012**	0.009**
	(0.006)	(0.007)	(0.006)	(0.005)
Observations	*84,780*	*84,780*	*84,780*	*84,780*
Panel B: Youth, young Māori and Pasifika, and education-leaver hires				
Young (under 25 years) hires				
Between * small firm	**0.007***	**−0.001**	**0.001**	**−0.002**
	(0.004)	**(0.004)**	**(0.004)**	**(0.003)**
Post * small firm	0.005	−0.003	−0.005	−0.005
	(0.005)	(0.006)	(0.005)	(0.004)
Small firm	−0.005	−0.001	−0.003	0.000
	(0.003)	(0.004)	(0.003)	(0.002)
Observations	*274,461*	*274,461*	*274,461*	*274,461*
Young Māori and Pasifika hires				
Between * small firm	**0.007**	**−0.003**	**−0.000**	**0.004**
	(0.008)	**(0.009)**	**(0.007)**	**(0.005)**
Post * small firm	−0.005	−0.014	−0.009	−0.009
	(0.010)	(0.012)	(0.009)	(0.007)
Small firm	0.004	0.003	−0.006	0.001
	(0.007)	(0.008)	(0.006)	(0.004)
Observations	*70,128*	*70,128*	*70,128*	*70,128*
Recent education-leaver hires				
Between * small firm	**0.004**	**−0.004**	**0.004**	**−0.001**
	(0.006)	**(0.008)**	**(0.007)**	**(0.006)**
Post * small firm	0.003	−0.013	−0.013	−0.001
	(0.008)	(0.010)	(0.009)	(0.007)
Small firm	−0.008	0.006	0.004	−0.001
	(0.006)	(0.007)	(0.006)	(0.005)
Observations	*87,813*	*87,813*	*87,813*	*87,813*

Notes: The panels of this table present key coefficients from linear probability regressions at the hire level where the dependent variable is an indicator for employment lasting at least a given length of time, as described in the column header. This table replicates Table 7, but only for the columns with more extensive controls, and regressions are limited to new hires at firms sized 15–24 of the types described in italics in the panel headers. Standard errors, in parentheses, are robust and clustered at the individual level.

The bold variable highlights the policy estimate of interest, in this table and in subsequent tables.

**$p < 0.05$, *$p < 0.10$.

Table A8 Predicting temporary contracts and trial period jobs

Dependent variable	On temporary contract, 2008	On temporary contract, 2008	On trial period, 2012	On trial period, 2012
Aged 50+ years	0.037	0.022	−0.078***	−0.067**
	(0.028)	(0.026)	(0.026)	(0.026)
Aged 15–24 years	0.065***	0.083***	−0.025	−0.050**
	(0.025)	(0.026)	(0.024)	(0.025)
Male	−0.028	−0.035	0.027	0.005
	(0.019)	(0.022)	(0.020)	(0.023)
Degree or higher qualification	0.066**	0.045	−0.182***	−0.117***
	(0.032)	(0.032)	(0.025)	(0.029)
School qualification	0.003	0.016	−0.011	−0.019
	(0.026)	(0.026)	(0.027)	(0.027)
No qualification	0.011	−0.004	−0.052*	−0.071**
	(0.027)	(0.026)	(0.030)	(0.029)
Does not know qualification	0.064	0.133	−0.084	−0.082
	(0.126)	(0.137)	(0.139)	(0.133)
Professional occupation		−0.005		−0.085**
		(0.037)		(0.043)
Technicians and trade		0.048		0.055
		(0.050)		(0.045)
Community and personnel service		0.015		−0.003
		(0.040)		(0.048)
Clerical and admin		0.030		−0.040
		(0.040)		(0.045)
Sales workers		0.118**		−0.001
		(0.054)		(0.046)
Machinery operators and drivers		0.051		0.004
		(0.056)		(0.055)
Laborers		0.087**		−0.035
		(0.041)		(0.042)
Other occupation		–		0.080
				(0.317)
Ethnicity fixed effects	Yes	Yes	Yes	Yes
Level 1 industry fixed effects	No	Yes	No	Yes
Observations	*1,686*	*1,680*	*2,409*	*2,406*
Mean of dependent variable	*0.200*	*0.201*	*0.376*	*0.376*

Notes: This table presents average marginal effects from logit regressions at the job level, predicting whether a new job uses a temporary contract in the first two columns, or a trial period in the last two columns. The sample is limited to those hired in the last year. Columns 1 and 2 use the 2008 Survey of Working Life (SOWL) only, while columns 3 and 4 use the 2012 SOWL only. Ethnicity fixed effects are self-reported and control for being Maori/Pasifika, Asian, other, or European. In the case of multiple reported ethnicities, ethnicity is prioritized in the order of the previous paragraph. The omitted category for qualification level is subdegree tertiary qualifications. The omitted category for occupation is managers. Level 1 industries group firms into 20 broad industries. Standard errors, in parentheses, are calculated using the delta method.

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.