The Effect of Trial Periods in Employment on Firm Hiring Behaviour

Nathan Chappell and Isabelle Sin

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The results in this paper are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI) managed by Statistics New Zealand. The opinions, findings, recommendations and conclusions expressed in this paper are those of the authors, not Statistics New Zealand, the Treasury, the Ministry of Business, Innovation and Employment, or Motu Economy & Public Policy Research.

Access to the anonymised data used in this study was provided by Statistics New Zealand in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business or organisation and the results in this paper have been confidentialised to protect these groups from identification. Careful consideration has been given to the privacy, security and confidentiality issues associated with using administrative and survey data in the IDI. Further detail can be found in the privacy impact assessment for the IDI available from www.stats.govt.nz.

The results are based in part on tax data supplied by Inland Revenue to Statistics New Zealand under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes. Any person who has had access to the unit-record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

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Abstract

An amendment to legislation in 2009 enabled New Zealand firms with fewer than 20 employees to hire new workers on trial periods. The scheme was subsequently extended to employers of all sizes. The policy was intended to encourage firms to take on more employees, and particularly more disadvantaged job seekers, by reducing the risk associated with hiring an unknown worker. We use unit record linked employer-employee data and the staggered introduction of the policy for firms of different sizes to assess the policy effect on firm hiring behaviour. We find no evidence that the policy affected the number of hires by firms on average, either overall or into employment that lasted beyond the trial period. We also do not find an effect on hiring of disadvantaged jobseekers. However, our results suggest that the policy increased hiring in industries with high use of trial periods by 10.3 percent.

JEL CLASSIFICATION J08; J63; J64

KEYWORDS 90-day trials; employment; labour market flexibility; firm hiring

Executive Summary

In March 2009, an amendment to the Employment Relations Act (2000) came into effect that introduced 90-day trial periods in employment for firms with fewer than 20 employees. A worker new to a small firm could be hired on a trial period, and for the first 90 days of his employment the legal requirements for dismissing him would be much reduced.

The policy was intended to stimulate employment by small firms by reducing the cost to them of hiring a person who turned out to be a bad fit for the job. Supporters of the policy claimed disadvantaged jobseekers would disproportionately benefit because employers would be more willing to take the risk of hiring them. Opponents anticipated costs such as a decrease in job stability, particularly for marginal workers. The policy was deemed a success, and in April 2011 the option of using trial periods was extended to firms of all sizes.

In this research we ask 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 use individual-level linked employer-employee data from Statistics New Zealand's Integrated Data Infrastructure, and take advantage of the natural experiment offered by the two policy changes. In particular, we look for a discrete difference in behaviour between firms above and below the 20-employee cutoff that opened up after the 2009 policy change, and closed again after the 2011 change.

We find no evidence that the ability to use trial periods significantly increases firms' overall hiring; we estimate the policy effect to be a statistically and economically insignificant 0.8 percent increase in hiring on average across all industries. However, within the construction and wholesale trade industries, which report high use of trial periods, we estimate a weakly significant 10.3 percent increase in hiring as a result of the policy.

We also find no evidence that the policy increased the probability that a new hire by a firm was a disadvantaged jobseeker for a range of definitions of disadvantaged jobseeker: beneficiaries, jobseeker beneficiaries, non-workers, recent migrants, youths under 25 years old, Māori or Pasifika under 25 years old, or education leavers. This result holds both over the economy as a whole, and in the high-use industries.

Trial period policy also does not appear to have affected the probability that a newly hired worker remained with her employer for at least two, five, 12 or 24 months; the policy seems not to have substantially increased short-term hiring.

Finally, we do not find any evidence that workers moving between employers were less likely to move to a trial period-eligible firm; the policy does not seem to have made workers less willing to change jobs.

We conclude that the main benefit of the policy was a decrease in dismissal costs for firms, while many employees faced increased uncertainty about their job security for three months after being hired.

Table of Contents

List of Tables

List of Figures

The Effect of Trial Periods in Employment on Firm Hiring Behaviour

1 Introduction

In response to the Global Financial Crisis (GFC) and the struggling job market in New Zealand, in late 2008 the government passed under urgency an amendment that introduced 90-day trial periods for new employees, a policy intended to stimulate employment and increase opportunities for disadvantaged jobseekers. From 1 March 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, the process for legally dismissing the employee is greatly simplified. The policy was deemed a success, and trial-period eligibility was extended to all firms on 1 April 2011.

Supporters of trial periods saw them as a way of boosting the economy, creating jobs, and giving new opportunities to struggling groups: firms benefit because they can cheaply dismiss workers who turn out to be a bad match for the role, while disadvantaged workers benefit because they get a chance to prove themselves to an employer who would see them as too risky if the costs of dismissal were higher. Opponents of the trial periods were concerned that, instead of helping new workers get a foot into the labour market, they would disadvantage those already struggling, encourage bad management practices, and lead to serial short-term hiring.

We evaluate the evidence for these claims by asking whether being allowed to use trial periods causes firms to change their hiring behaviour, either in terms of the total number of hires, the type of people they hire, or the stability of employment relationships. We focus on the effect of firms being 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. Our findings also contain lessons on the effects of labour market flexibility.

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. Intuitively, we compare how the hiring behaviour of small firms (15-19 employees) changed with the first policy change with how the hiring behaviour of large firms (20-24 employees) changed at the same time. Both sizes of firm were affected similarly by changes in economic conditions such as the GFC, but only small firms were affected by the policy change. Any difference in their change in hiring behaviour can thus be attributed to the policy. For additional confirmation, we also compare the changes in behaviour for the two firm sizes that occurred with the second policy change. Only the large firms were affected by this policy change, which eliminated the difference in trial period eligibility between large and small firms; any difference in behaviour that was the

caused by trial period policy should disappear at this point. We then use the same research design to look for a policy effect on the type of people hired and the duration a new employment relationship lasted.

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 labour market, such as recent beneficiaries, recent migrants, or young Māori and Pasifika people. However, we also fail to find any evidence that trial-period eligibility increases short-term hiring or makes workers reluctant to change jobs, two issues raised by critics of the policy. This suggests that trial periods allow firms to benefit from reduced costs associated with hiring and dismissals, without changing their behaviour; jobseekers are neither more nor less likely to find work, but may have to bear increased perceived uncertainty about their job security while on a trial period.

Our analysis primarily focuses on firms of size 15 to 24 employees because in this size range we have the cleanest identification of the policy effect. It is possible that the policy effect differed for firms outside this size range. Although we are unable to definitively rule this out, we conduct suggestive analysis and find little indication of a policy effect even in very small firms.

Although we find no effect of the policy for the economy as a whole, when we restrict attention to firms in construction or wholesale trade, two industries that state they frequently use trial periods, we find statistically weak evidence that trial periods may have increased firms' hiring of new employees by 10.3 percent. In contrast, in education and training, two industries that declare low use of trial periods, we find no significant policy effect. In neither high-use nor low-use industries do we find an effect on the hiring of disadvantaged workers or the stability of employment relationships.

The rest of the paper is organised as follows: Section 2 discusses previous research on trial periods in New Zealand and labour market flexibility in various international contexts, and relates our results to earlier findings; Section 3 gives background on trial periods in New Zealand; Section 4 describes the data used; Section 5 details the empirical strategy; Section 6 presents results from the econometric analysis; and Section 7 concludes and discusses the implications for policy.

2 Literature

Our findings contrast with previous research into trial periods in New Zealand. Three reports based on surveys conducted in late 2009, late 2011 and 2012–2013 together show that employers support trial period policy, and many claim the policy causes them to hire people they otherwise would not have hired. Surveys by the then-Department of Labour show that employers are generally aware of the law changes; that they like trial period policy; and that 28 percent of employers report they would not have filled the last position without a trial period (DOL 2012), suggesting trial periods are giving some employers the confidence to create roles that would not otherwise exist. In addition, around 40 percent of employers say they would not have hired their most recent employee without a trial period (DOL 2010; DOL 2012). This latter figure being larger than 28 percent suggests trial periods not only create new roles but also encourage the hiring of different types of people. Backing this up, qualitative survey results suggest there are benefits to disadvantaged groups such as youth and the unemployed, with some employers emphasising that they felt more comfortable taking hiring risks when they could use trial periods.

Similarly, in a more recent Ministry of Business, Innovation and Employment report (2014), around one third of surveyed employers report hiring someone they otherwise would not have hired. However, the in-depth qualitative interviews suggest that most employers still hire based on the demand for their goods and services, implying their total level of hiring may be little affected by the policy.

Our finding that trial period policy had no effect on firm hiring behaviour on average is perhaps surprising in light of the enthusiasm for the policy that firms show in the surveys, and their statements of its effect on their hiring. However, note that firms may overstate the extent to which the policy affected their hiring behaviour because they don't actually know how they would have hired in the counterfactual world in which the policy was absent. Because the policy reduces their costs, they may respond positively to questions about it in an inadvertent or even deliberate attempt to influence future policy.

Alternatively or in addition, trial periods could cause behavioural changes only in a small subset of firms or hiring decisions, and the average effect may be too small to see in the data even given the precision of our estimates. Use of casual or temporary contracts may reduce the need for trial periods, particularly for young workers. Furthermore, in some cases training costs may render trial periods irrelevant; some firms may be better off keeping an employee who is a bad match if training costs for new employees are high in order to avoid incurring the training costs again for the employee's replacement. We endeavour to explore these possibilities in Section 6.2 of this paper, and find some suggestive evidence that trial period policy may increase the number of new hires for industries such as construction and wholesale trade, with high use of trial periods. Regardless, we find no evidence of economy-wide effects of the sorts implied by the surveys.

New Zealand Institute of Economic Research (2011) uses aggregated administrative data and a difference-in-differences strategy to test whether the 2009 law change caused small firms to change behaviour relative to larger firms. This report suggests trial periods caused hiring and total job numbers to increase, though it uses data up until September 2009 only. Our attempts to replicate NZIER's findings suggest their positive finding stems from misattributing the effects of the Global Financial Crisis, in which large firms were more negatively affected, to trial period policy.

More broadly, our findings also contribute to the literature on labour market flexibility. There are two key reasons why changing the ease of dismissal may be important to firms. First, it allows them to adjust their labour input in response to changing conditions. For example, in the event of a downturn in demand for its products, a firm's optimal response may be to decrease its number of employees. But if laws limit its ability to dismiss employees, it cannot respond optimally to such external changes. Knowing this, firms may restrict their hiring below the optimal level under normal economic conditions to mitigate the risk of being stuck with too much labour in the event of a negative demand shock. Secondly, and distinctly, labour market flexibility reduces costs for firms in the event of a bad match. Firms hire without perfect information, and it may turn out that either a newly created role is unsuitable for the firm, or that a specific person hired is unsuitable for the role. Lowering the costs of dismissal may benefit firms through this match-quality channel.

There is a considerable literature testing whether a permanent change in the ease of dismissal affects the labour market through either of the two channels described above. In the United States, several studies analyse 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 employment flows (Autor et al. 2007).

Other studies focus on variation within a region, by comparing those affected by a law change with those not. Kugler (1999) estimates the effect of a decrease in firing costs in Colombia in 1990. She uses a comparison between workers in the affected formal sector and those in the unaffected informal sector to conclude that the change caused employment flows to increase. In contrast, von Below and Thoursie (2010) look at a Swedish policy change in 2001 that relaxed employment protection laws for small firms. They argue it had no effect on hiring or separations, suggesting that firms use other methods to get around such restrictive laws. In a similar vein, Bauer et al. (2007) use variation in worker-protection laws by time and firm size in Germany and find no effect on employment flows. They suggest 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.

This international literature suggests the effects of labour market flexibility depend on the context and specific nature of the laws. But regardless of the specifics, firms find ways to ease the burden of restrictive legislation, as discussed in Autor (2003) and von Below and Thoursie (2010).

Our research differs from previous studies in that the policy change we examine reduces the costs of dismissal for a short period after hiring only. It is unlikely to increase hiring through enabling firm labour demand to become more responsive to changes in economic conditions, and will affect hiring (if at all) primarily through the match quality channel. We are thus able to isolate the effect of a change in the match quality channel only, which studies of permanent changes in the ease of dismissal cannot. Although the benefits to hiring from this short-term decrease in dismissal costs are expected to be lower than the benefits of a permanent decrease, the costs to the employee in terms of decreased job security are also lower. We find no evidence the average firm increases hiring because of trial period policy, suggesting increased firm ability to adjust their labour input is the dominant source of increased hiring when dismissal costs are permanently decreased.

3 Background

3.1 Introduction of trial periods

Trial periods were introduced as part of the government's response to the Global Financial Crisis and economic troubles in New Zealand. In the context of the slowing growth and rising unemployment seen in late 2008, the then-Minister of Labour Kate Wilkinson 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 labour market.[1] The Employment Relations Amendment Act 2008 was passed under urgency in December 2008.[2] It introduced trial periods for firms with fewer than 20 employees, coming into effect 1 March 2009. Eligibility was extended to all firms on 1 April 2011.[3]

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

[2] http://www.lawlink.co.nz/articles.php?articleid=131

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

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; and
>
> (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 must be agreed to by both parties and signed before the employee begins work. Importantly, trial periods may only be used for employees who have not previously been employed by the firm.

3.2 Dismissing without versus with a trial period

Trial period policy is only expected to have an effect if it genuinely makes dismissing an employee easier. This subsection describes the legal requirements for dismissal with and without a trial period, and shows how trial periods lower the bar for dismissal; employers' views of the effectiveness of trial periods are discussed in the following subsection.

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.

A dismissal without a trial period must meet two standards of fairness. First, it must be substantively fair, meaning there was a valid reason for dismissal. Reasons for dismissal can generally be grouped into serious misconduct, which justifies summary dismissal, and less serious misconduct. Serious misconduct might include behaviour such as fighting, direct disobedience, or dishonesty; less serious misconduct might include behaviour such as absenteeism, unsatisfactory work performance, or using abusive language.[4] Second, a dismissal must be procedurally fair. The test, as laid out in section 103A of the 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 cases of serious misconduct); and consider any relevant explanations of the employee. [5] 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 and mediation fails, the next step is to apply to the Employment Relations Authority (ERA) for

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

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

a resolution. The ERA aims to provide a speedy resolution without concerning itself with the legal technicalities in the way a court would. Once the ERA has issued its determination, either party, if unsatisfied with the outcome, may appeal to the Employment Court.[6] If the process gets this far, it 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 behaviour 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", as has been clarified by a number of court cases. Good faith principles still apply to employment relationships covered by trial period provisions. The employer must have a reason for dismissal, such as lack of performance or incompatibility, and processes stated in the employment contract must be adhered to. The employee must be informed of the reasons for dismissal, though she need not be consulted or given a chance to explain or improve before the decision is made.[7] Even during a trial period, an employee can raise a personal grievance based on discrimination, harassment or other unfair behaviour.

In legal disputes the courts have tended to adhere to a strict interpretation of the law. For example, in *Hutchison v Canon New Zealand Limited*, verbal notice of dismissal was found to be insufficient when the employment agreement required written notice. Similarly, in *Hall v Smith Crane & Construction*, a dismissed employee was awarded over $38,000 in lost wages and compensation because the employment agreement was signed only after he started work, meaning he was not eligible to be hired on a trial period.[8]

At times, a firm may wish to dismiss an employee when external shocks lessen demand for its products, even if the employee suits the role. Legally, this is not a valid reason for dismissal, though in extreme cases restructuring so that current positions no longer exist can be used either to reduce labour or get to rid of under-performing employees. Trial periods increase the ease dismissing employees in order to downsize, but only to the extent that the firm has recent hires still in their trial periods who can be dismissed.

3.3 Employers' 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. For example, a year after trial periods were first introduced, 74 percent of surveyed employers knew that employees must consent to trial periods, and 70 percent knew employees retain protection against discrimination and

[6] http://www.findlaw.co.nz/articles/4296/unjustified-dismissal.aspx

[7] See Smith v Stokes Valley Pharmacy (2009) Ltd EC Wellington [2010] NZEMP 111, and http://www.lawlink.co.nz/articles.php?articleid=189

[8] For more details see https://duncancotterill.com/publications/strict-interpretation-of-90-day-trial-period-continues

harassment (DOL 2010). It is likely that firms that use trial periods are generally among those with greater knowledge about them.

Despite trial periods not being a "get out of jail free" card for employers, trial period policy is viewed as substantially reducing the cost and risk of dismissal, and therefore of taking on a new employee. The following quote from an employer shows how important the cost of dismissal can be to firms (DOL 2012):

> The trial period is very, very important on that, because you are taking a bit of a risk and it gives basically the employee, and employer, the opportunity to get over that risk.

Another employer's words convey a similar message (MBIE 2014):

> I think just in the very few cases we've terminated in the trial period it gives you that feeling of security that there's not going to be that comeback. There's always a risk whenever you dismiss and so I suppose it's just really reassured us through that process that we might have made a bad recruitment decision but it's not going to really come back and bite us.

These views are not universal though, with some employers viewing trial periods as a way of allowing poor management practices (DOL 2012):

> We don't do trial periods… We took the view that trial periods were not a license to be a bad employer. So basically, for them to be valid and to be used properly, they require a high level of supervision and maintenance and working with the employee, which should happen anyway, and they certainly shouldn't be an 'out'.

The fact that many employers like trial periods can be seen in survey evidence on their use: 59 percent of employers who hired in the previous year used a trial period (MBIE 2014). It seems that firms know about them, and often use them, and thus there is reason to believe trial period policy could have changed hiring behaviour on a large scale.

3.4 Employees' views on trial periods

Surveys tell us less about employees' views on trial periods. Qualitative interviews show employees lacked in-depth knowledge on 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). Perhaps the most important lesson is that trial periods are not generally seen as negotiable, but rather that job offers are conditional on accepting one; trial periods are entered as a standard clause in employment agreements (MBIE 2014). Hence although it is true that trial periods are consensual, it is not the case that a jobseeker will be offered a job and *then* asked if they would be happy to go on a trial. Absent assertive negotiations by the employee, a trial period is not something the employee has control over other than turning down a job offer.

Perhaps in light of this, MBIE (2014) reports that employees have mixed views on trial periods, with some accepting them as the new norm. There is also no evidence the policy fundamentally changed relationships between firms and workers. But presumably the policy harms employees who would have gained the job anyway, by making it easier for firms to dismiss them. These increased feelings of vulnerability are shown in qualitative interviews in DOL (2010), with one employee stating:

> [I would prefer not being on a trial period] because of the security behind it. Like you know you're not going to randomly lose your job for no apparent reason.

These findings suggest trial periods are widely viewed by employees as an unavoidable cost. The question is whether they cause some employees to get jobs that they would not have been offered absent trial period policy.

3.5 Use of trial periods

As mentioned previously, survey evidence suggests widespread use of trial periods. Between 50 and 60 percent of surveyed hiring firms have used a trial period with at least one employee (DOL 2010; DOL 2012; MBIE 2014). Use of trial periods varies somewhat by industry. Appendix Figure 4, which we reproduce from MBIE (2014), shows the percentage of employees who were hired on a trial period, among those who started their jobs in the last year. Appendix Table 14 shows in more detail reported trial period use by industry, from the 2012 Survey of Working Life. The values here are lower on average than the 50 to 60 percent of firms that stated they have hired using a trial period, because firms may use trial periods for some employees but not others.

The two industries with highest trial-period use are construction and wholesale trade.[9] At the other end of the spectrum, education and training have an especially low prevalence of trial periods, with fewer than 15 percent of hires involving one. These differences by industry motivate parts of our empirical analysis, where we examine effects separately for high-use industries (construction and wholesale trade) and low-use industries (education and training). If any policy effect exists, it should be most evident in industries whose firms regularly use trial periods.

Not unexpectedly, trial periods are used far more widely for permanent positions than other contract types, with 43 percent of employees in permanent jobs starting on a trial period compared with 16 percent in temporary jobs (MBIE 2014). Similarly, 41 percent of employees on an individual employment agreement started on a trial period, compared with 24 percent of those on a collective employment agreement.

There are also differences by occupation in the likelihood of starting on a trial period. Trial periods are most common for technicians and trade workers, managers, sales workers, and machinery operators and drivers, all of which have over 40 percent trial usage (MBIE 2014). These high-use occupations are consistent with the finding that trial periods are most prevalent in construction and wholesale trade. Low-use occupations include clerical and administrative workers, at 30 percent, and professionals, at 22 percent.

Finally, the majority of employers use trial periods to check the employee's ability, with over 50 percent of surveyed firms giving this reason (MBIE 2014). The next most common reasons are to check the employee's fit with the workplace (23 percent) and to manage risk (20 percent). This evidence supports the hypothesis that employers use trial periods to get matches they are happy with when hiring employees about whom they have limited information.

3.6 Other policy changes targeted at small firms

One concern for any difference-in-differences 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.

[9] Though the small sample means the difference in trial period use in these industries is not statistically significantly different from the overall average of 36 percent.

In the wake of the Global Financial Crisis, other policies were introduced to especially 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.[10] The new laws came into effect at several different dates, the earliest of which was 1 April 2009, right after our first policy change of 1 March 2009. However, even if these tax laws differentially changed the hiring behaviour 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 these tax changes affected firms sized 15-19 substantially differently to those sized 20-24, and so our treatment and control groups should have been affected similarly. As an additional check, we look for any policy effect to disappear after the 2011 policy change; changes driven by trial period policy will be eliminated after 2011, whereas any differential changes driven by this tax policy will not.

Another policy change was the 2013 Apprenticeship Reboot package, designed to encourage apprenticeships by merging them all into one scheme and giving financial aid to employers and apprentices.[11] It is possible this would affect smaller firms more if small firms tend to hire apprentices. But again, most of our analysis compares firms with 15-19 employees to those with 20-24 employees, and it is difficult to see how the Apprenticeship Reboot could have affected the hiring of either group very differently. Furthermore, this policy change occurred in 2013, well after our second policy change. At worst, it could cloud our check that the trial period policy effect disappears after the second policy change in April 2011.

Finally, throughout 2009 the government fast-tracked $500 million of publicly-funded building projects with the aim of creating jobs and stimulating the economy.[12] Though not specifically designed to aid small firms, small firms may have reaped disproportionate benefit from these projects. However, given that we focus on the narrow band of firm sizes 15 to 24, our treatment (sizes 15-19) and control (sizes 20-24) groups are likely to have been affected very similarly. More broadly, stimulus spending in the wake of the GFC should not affect our estimates.

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 covers at a monthly level essentially every employment relationship in New Zealand. To these data are linked a variety of other administrative data at the individual and firm level.

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 are available until December 2013 only.

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

[11] For more details see http://www.beehive.govt.nz/release/reboot-signs-8000-new-apprentices

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

Our key variables come 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 five 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 is also the more relevant measure because trial periods may only be used for employees who have never worked for the firm before, so any change in hiring behaviour should be seen in this group.[13]

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. Firm size at the time an employee is hired is the relevant size measure for trial period eligibility. 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 does 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 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 fill out an EMS. We derive firm size this way because, from a legal perspective, the 20-employee threshold is a head count measure at the firm (employer) level. For analysis involving individual plants, plant size is defined analogously as a start-of-month head count.

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

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 categorise 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.[14] 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 relating 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.[15] For example, parts of our analysis

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

[14] 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 employers do not consider foreign experience to be a perfect substitute for New Zealand experience.

[15] 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.

look at whether a new employee lasted five or more months with the firm, meaning 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.

For some analysis we split hires based on the earnings quartile of the employee. Earnings quartiles are based on monthly wages, and are calculated annually among hires of any type (new or re-hire) at firms of any size.[16] For each person hired we calculate average monthly earnings during the first six interior months (neither the first nor last month of employment), and categorise hires into earnings quartiles based on these values. Because we use interior months only to calculate earnings, we cannot allocate employees who were at a firm for fewer than three months to earnings quartiles. Note we also do not have data on Full Time Equivalents (FTEs) worked, so highly paid employees who work few hours in the month are allocated to low earnings quartiles.

The EMS does have certain limitations. In particular, we cannot tell the nature of the employment agreement (for example, 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 generate additional information about hires using the links between the EMS and other data sources. The IDI contains information on gender, age and ethnicity.[17] These allow us to identify hires who are under 25 years of age, and those who are under 25 and Māori or Pasifika. 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 dataset. Similarly, we use tables of visa data to identify 'recent migrants', who had their visa approved in the previous two years.[18] Finally, administrative data on secondary and tertiary education enrolments from the Ministry of Education allows us to identify recent education leavers, defined as those who attended school or university in the year before but not in the year after being hired.

The IDI also contains industry and region information for firms. Industry classifications come from Australian and New Zealand Standard Industrial Classification (ANZSIC) 2006 codes, and are consistent for a firm over time. These divide firms into 19 divisions at the broadest level, as listed in Appendix Table 1, and for much of our analysis we use more detailed 3-digit ANZSIC 2006 codes, which divide firms into 203 disaggregated industries. Information on region is at the plant level; we aggregate plants up to the firm level and use these data to construct regional council employment shares for some specifications.[19]

Two 3-digit 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 firm size range we focus on, but school education employers are included in our data in large numbers. The reasons for these hiring spikes are unclear, but we are confident they do not reflect an employer response to trial period policy. To ensure they do not drive our

[16] Note in particular that these quartiles are not relative to employees overall, who are likely to have higher earnings on average than employees new to their firm.

[17] 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.

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

[19] The allocation in the IDI of employees within a firm to plants within the firm is known to be of low quality, thus our preferred specification is at the firm rather than plant level.

findings, throughout our regression specifications we include a dummy variable for firms or hires in each of these industry-months.

4.2 Descriptive statistics

To minimise unobservable differences between our treatment (small) firms and control (large) firms, for most specifications we limit our sample to firms with 15 to 24 employees.[20] Table 1 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, but the majority of employment and hiring is in firms with 25 or more employees. If trial period policy did affect hiring behaviour, 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 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 1: 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 five years. Total counts are rounded to the nearest 100.

Table 2 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.

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

Table 2: Descriptive statistics for treatment and control firms by period

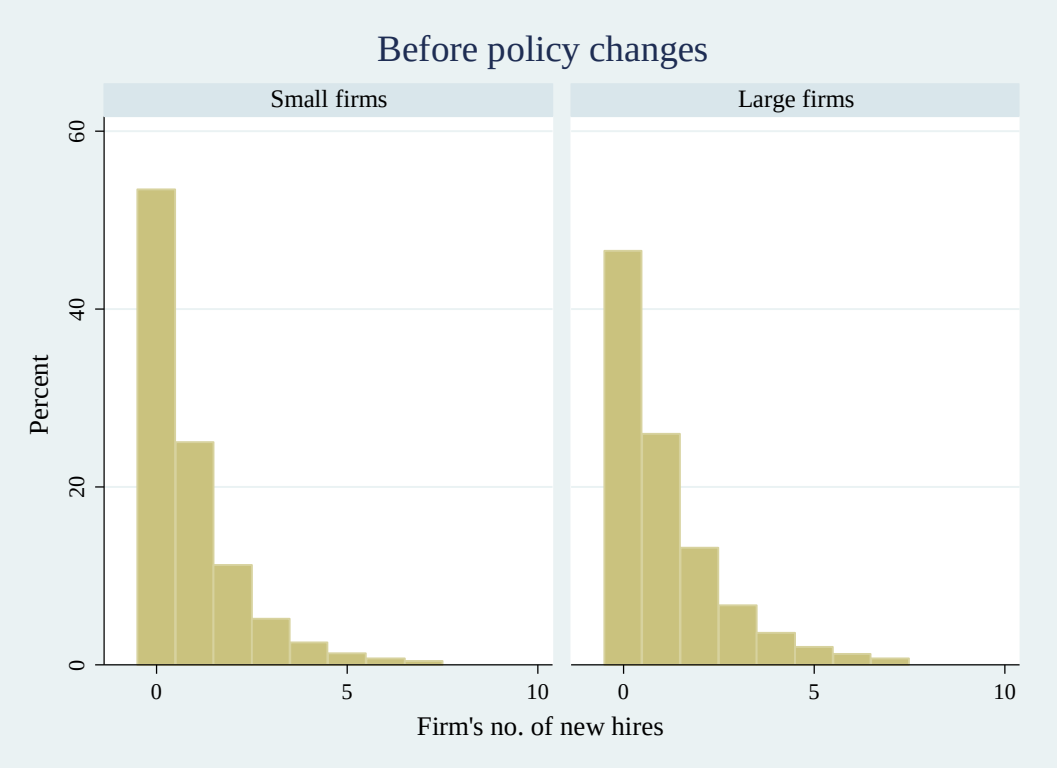
	Firms with 15-19 employees			Firms with 20-24 employees		
Period relative to policy changes:	Pre	Between	Post	Pre	Between	Post
Average number of firms employing each month	4,884	4,710	4,753	2,754	2,648	2,733
Average firm size	16.8	16.8	16.8	21.8	21.8	21.8
% of firms with multiple plants	13.8%	13.7%	13.3%	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 non-seasonal 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 prev. yr	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 NZ on a visa in the past two 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 Māori 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%
Among new hires that lasted 3+ months:						
% in the lowest quartile of earnings distribution	24.8%	24.7%	24.2%	23.5%	23.7%	22.0%
% in the second quartile of earnings distribution	26.3%	26.8%	26.1%	25.4%	26.3%	26.0%
% in the third quartile of earnings distribution	26.0%	25.2%	26.4%	26.3%	25.9%	26.7%
% in the top quartile of earnings distribution	23.0%	23.1%	23.2%	24.9%	24.1%	25.3%

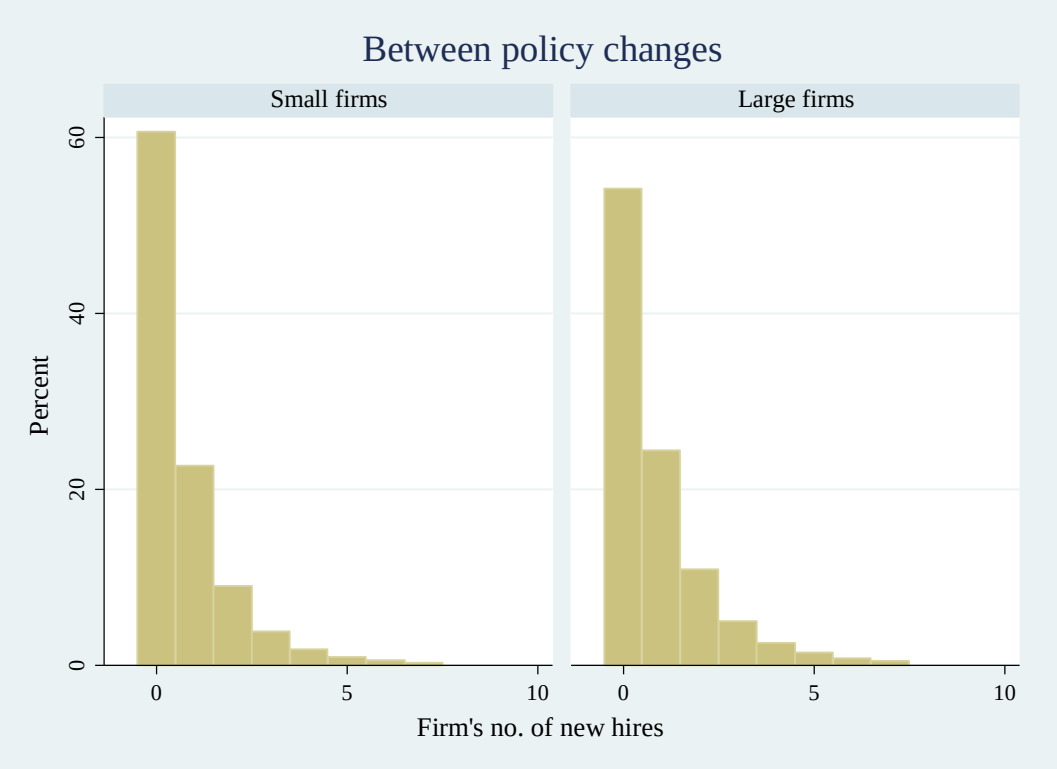
Notes: A non-seasonal employee is one who has not worked for the firm in the previous year. A new employee is one who has not worked for the firm in the previous five years. The earnings distributions are calculated annually among hires only, for those staying at least three months. The sample for hires staying at least five months is from January 2005 to October 2013, five months before our last month of data. The sample for education leavers is limited to January 2005 to December 2012, for data-coverage reasons. All other statistics are derived from the period January 2005 to March 2014. All percentages are derived from counts that have been randomly rounded to base 3.

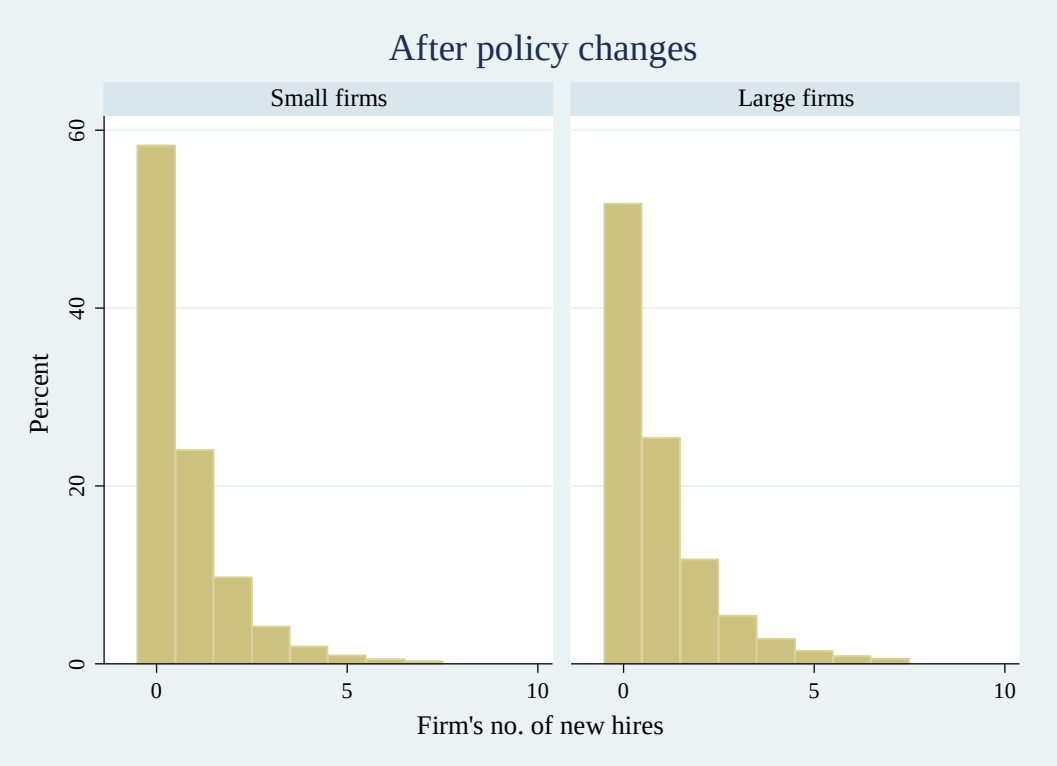
A large number of firms hire each month. Fifty-four to 68 percent of firms make any hires in a month, and 42 to 56 percent of firms hire new employees each month. The difference between new hires and overall hires is likely to reflect phenomena such as seasonal workers who return to the same employer each year, and casual employees. Hiring rates are 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. Figure 1 shows in more detail the distribution of the number of new hires for small and large firms in the three periods.

Figure 1: Hire counts by firm size in each period







Notes: This figure shows the distribution of number of new hires per month in each period for small firms (15-19 employees) and large firms (20-24 employees).

The fourth panel of Table 2 shows the percentage of new hires that are various types. Across firm sizes and periods, the majority of new hires result in short-term employment only; 43 to 47 percent of new employment relationships last five months or longer. The percentage is very similar in small and large firms both in the pre and post periods, and is slightly higher in small firms than large 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 percent respectively, and declines somewhat for both firm sizes in subsequent periods.

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

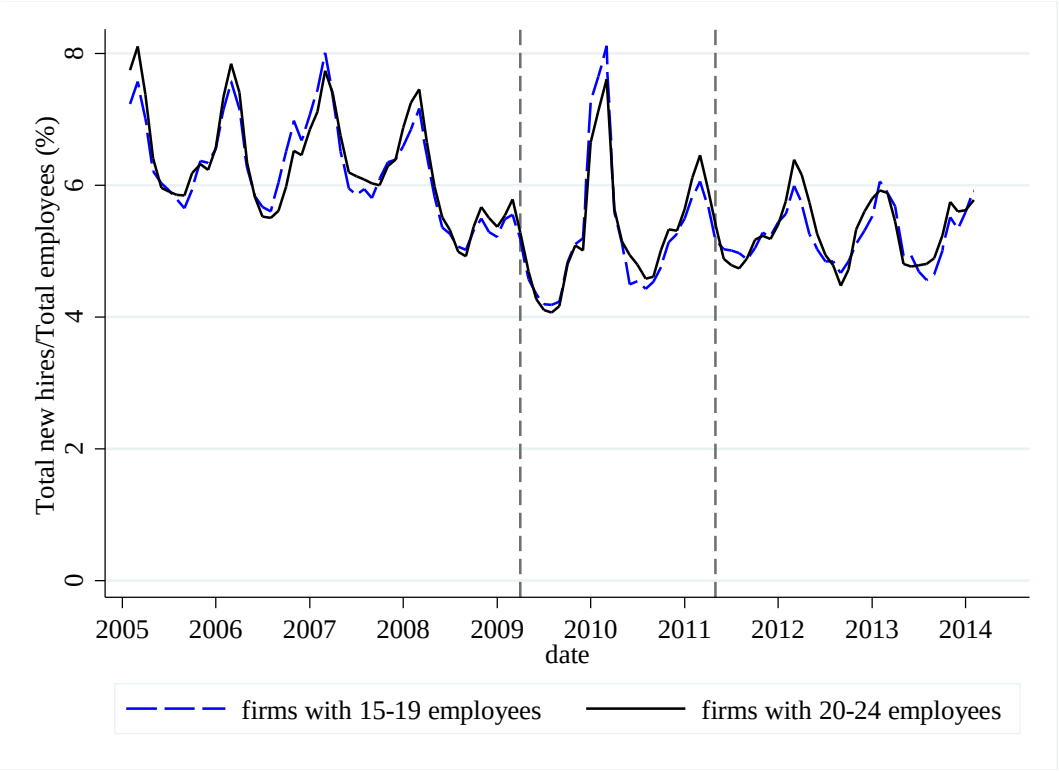
The final panel of Table 2 shows the proportion of new hires falling into each quartile of the earnings distribution. By construction, these numbers are mostly close to 25%, but they deviate from it for three main reasons. First, because new hires may have a different distribution of earnings to re-hires. Second, because the distribution of earnings by new hires at firms with 20-24 employees need not be identical to the distribution at firms with 15-19 employees. Third, because the distribution need not be the same at firms with 15-24 employees as at firms outside this size range. The differences between small and large firms in our sample suggest that in the pre period small firms were somewhat more likely to hire low-paid workers, whereas large firms were somewhat more likely to hire high-paid workers.

4.3 Patterns of hiring over time

Figure 2 shows the monthly behaviour of the total number of new hires scaled by the total number of employees, for our treatment and control groups.[21] The vertical lines show the introduction of the two policies. A policy impact would appear as a gap that opened up between the two lines after the first policy change, and closed again once trial-period eligibility was extended to all firms. The figure shows no evidence of a policy effect on the number of new hires for small firms; the two lines of hiring behaviour are not only parallel, but virtually coincide in all periods. Similarly, Appendix Figure 1 and Appendix Figure 2 show little evidence of any effect on the number of hires lasting five or more months, nor on hires coming from other jobs. We examine any policy effect on the number of hires more rigorously in Section 6.1.

[21] Three-month moving averages are presented for ease of viewing.

Figure 2: New hires as a percentage of total employees for treatment and control firms

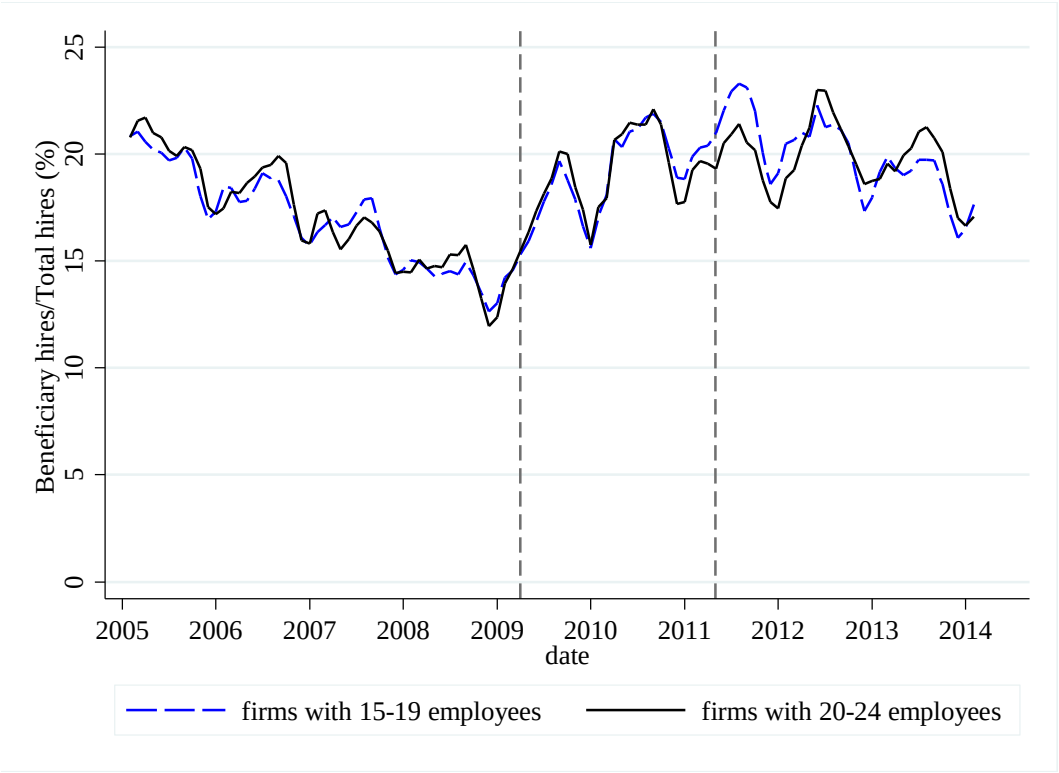


Notes: Values are three-month moving averages. Vertical grey lines indicate the policy changes.

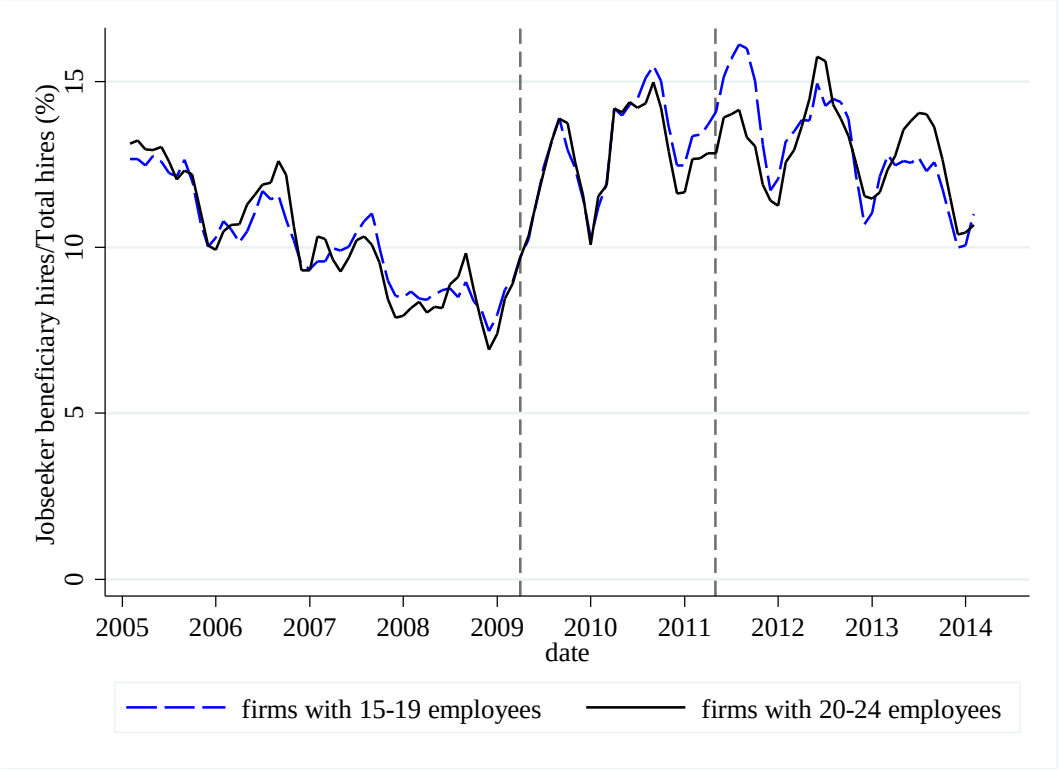
The panels of Figure 3 graph the probability a new hire is each type of disadvantaged jobseeker. If trial period policy increased the likelihood of disadvantaged groups being hired relative to other groups, we would see a divergence in the two lines in the period between the policy changes. In none of these graphs do we see evidence that trial period policy increased the likelihood a given hire is a recent beneficiary; was recently on a jobseeker benefit; recently left school or university; is a recent migrant; had not worked in the past year; is under 25 years old; or is Māori or Pasifika and under 25 years. The lines of hiring behaviour tend to coincide, and any difference is noisy and short-lived. We examine any policy effect on the type of hires more rigorously in Section 6.7.

Figure 3: Disadvantaged hires as a percentage of new hires for treatment and control firms

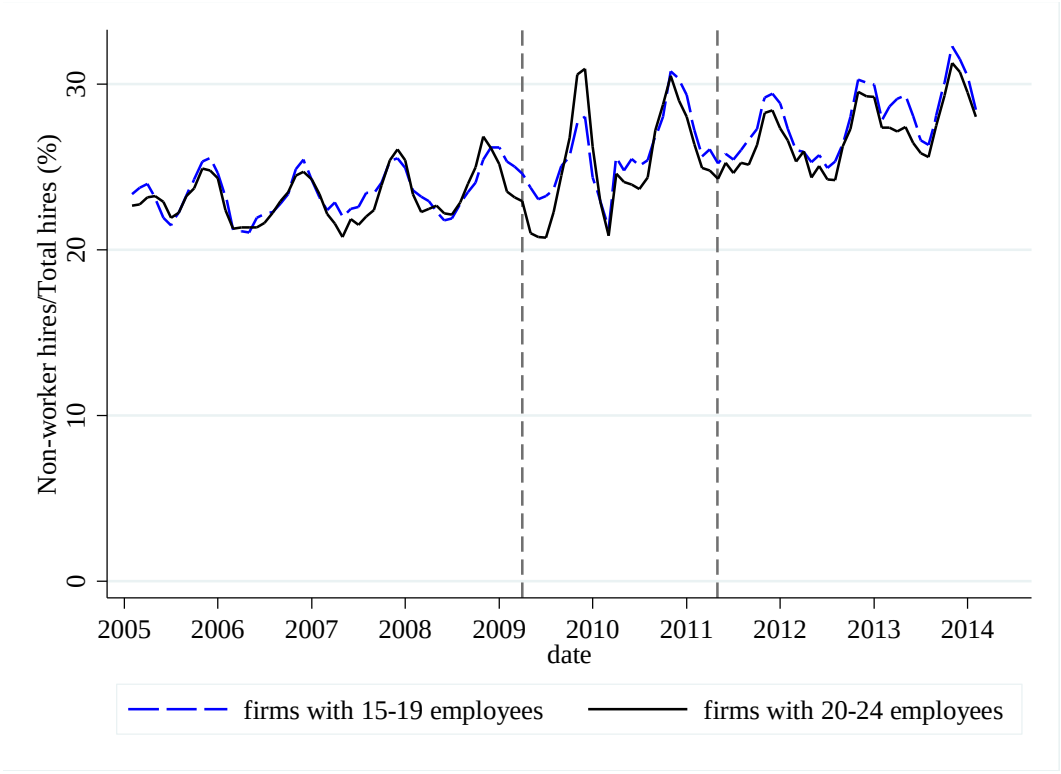
Panel A: Beneficiaries



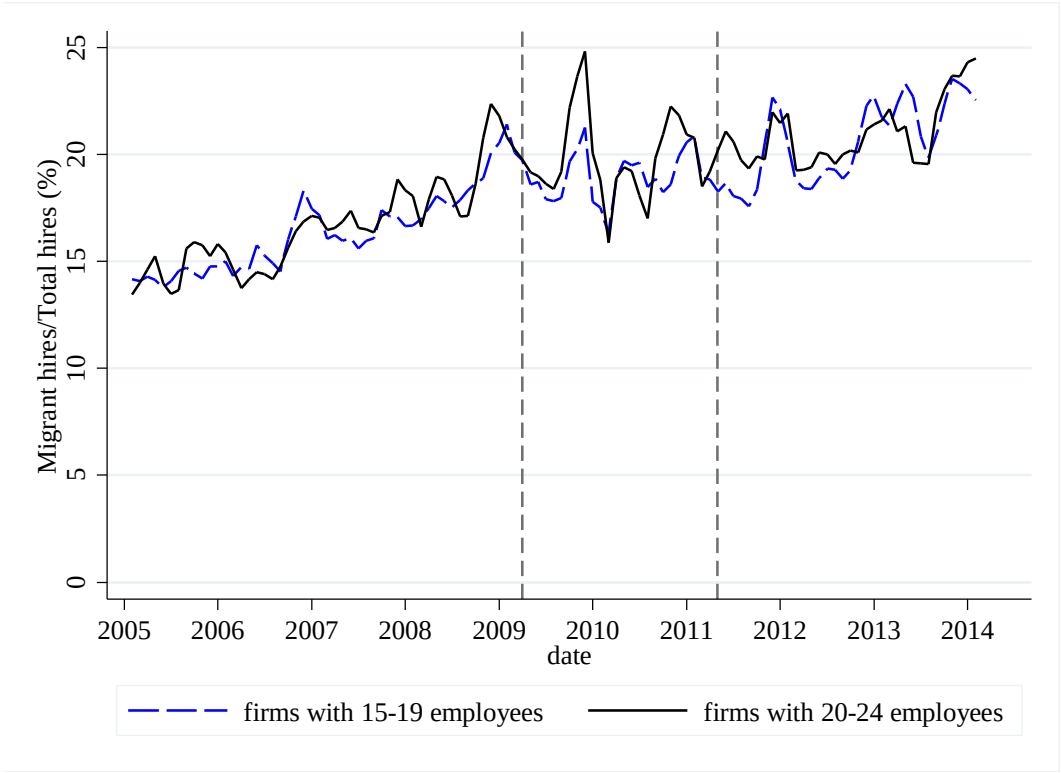
Panel B: Jobseeker beneficiaries



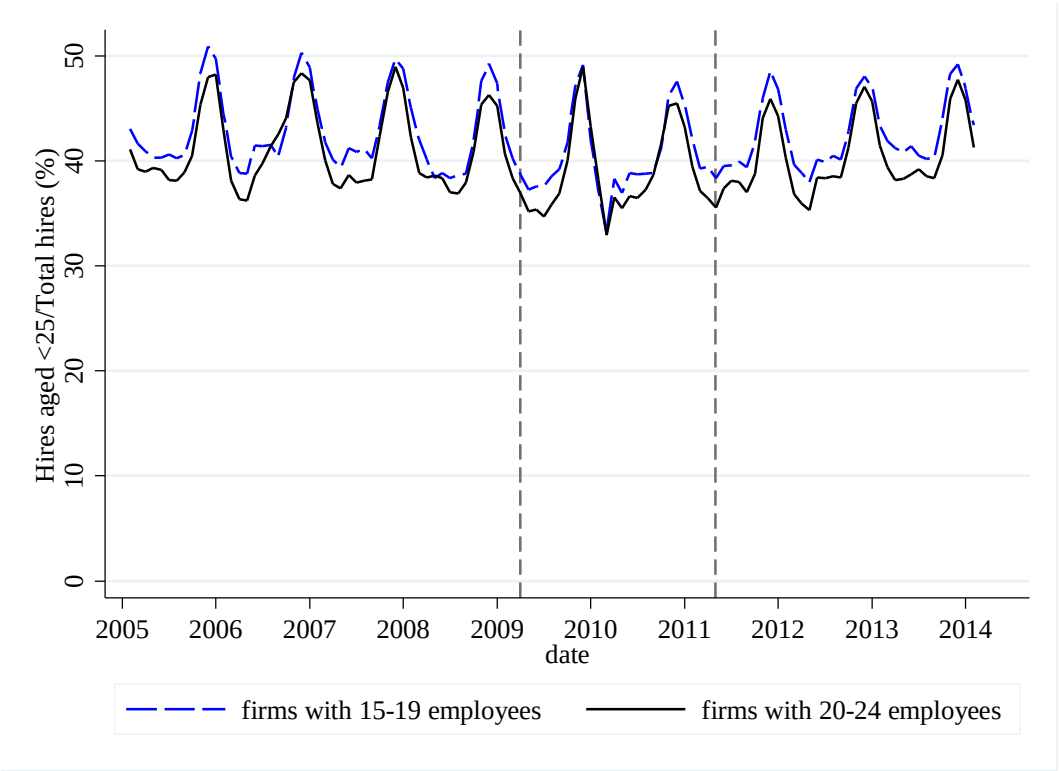
Panel C: Non-workers



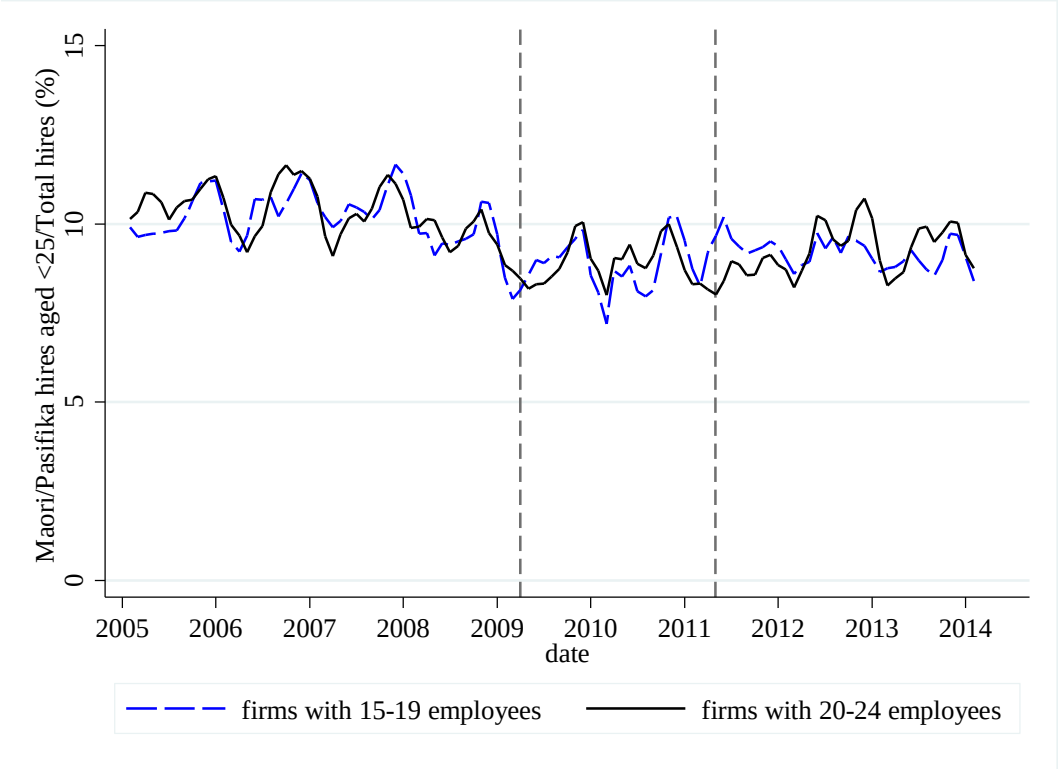
Panel D: Migrants



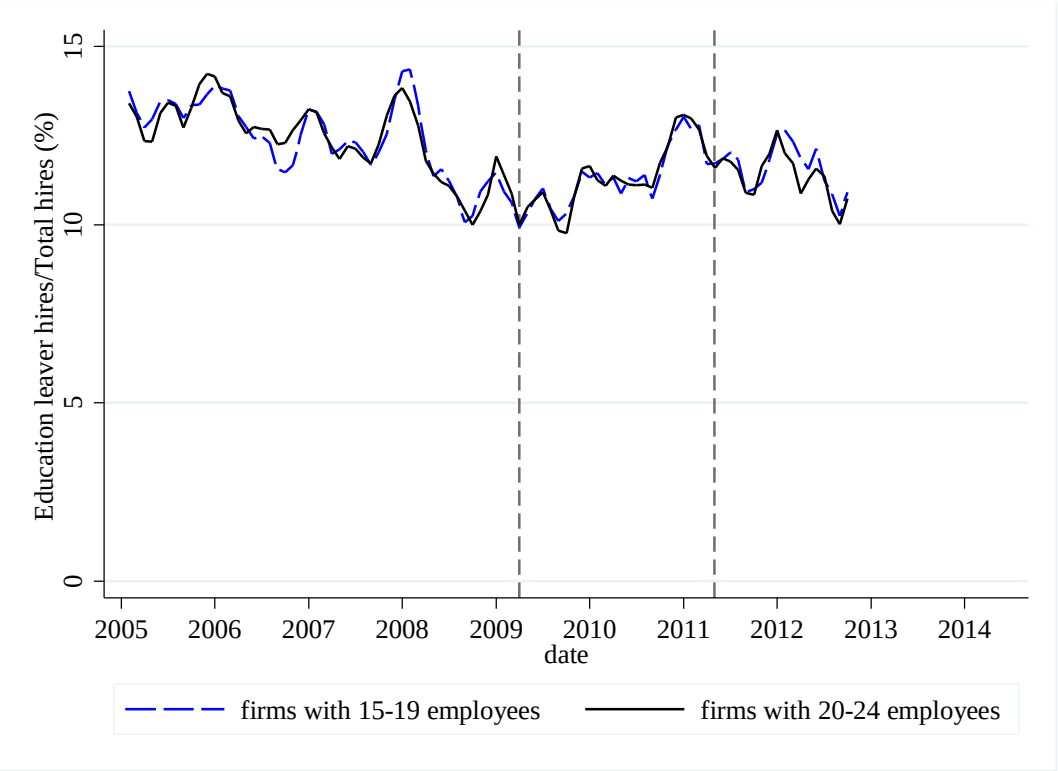
Panel E: Youths under 25 years old



Panel F: Māori or Pasifika under 25 years old



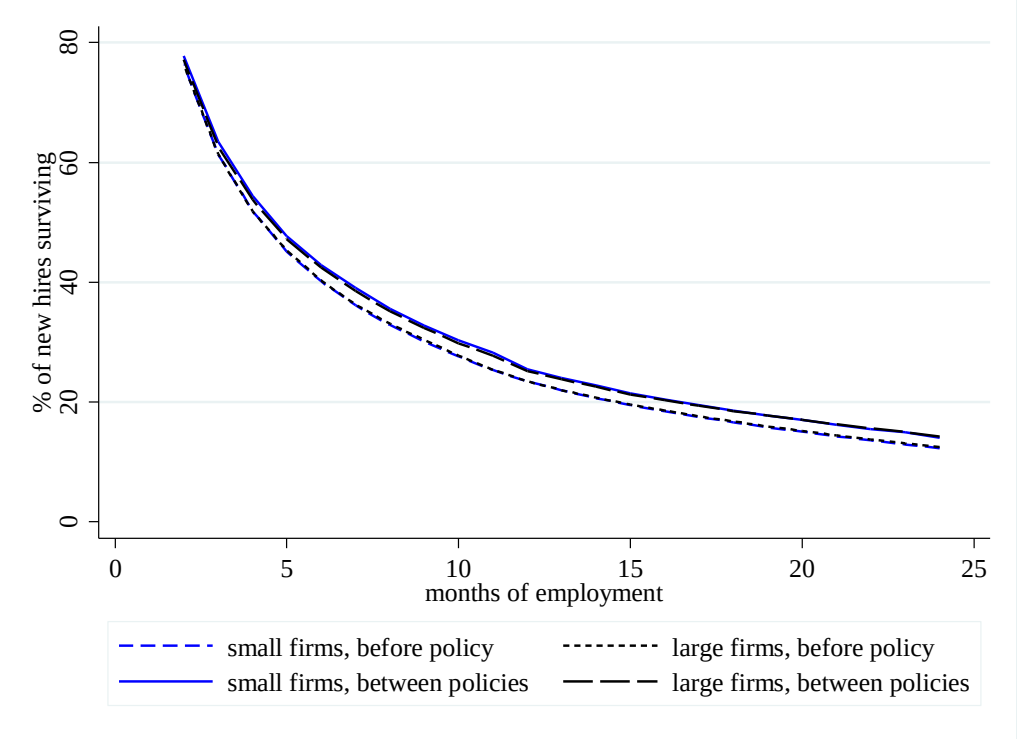
Panel G: Education leavers



Notes: Values are three-month moving averages. Vertical grey lines indicate the policy changes.

Finally, Figure 4 graphs the percentage of new hires who survive (ie, neither leave nor are dismissed) for different durations. This is presented separately for small and large firms, before and between the policy changes. If trial period policy decreased employment stability, we would expect to see the small-firm line for the between period fall more steeply initially than the large-firm line for the same period. However, in both periods the small- and large-firm lines are nearly identical, showing no difference in hire duration between the two groups. Both curves shift outward relative to the before-policy period, indicating that, if anything, employment stability was higher during the GFC for people hired by firms in our sample. We examine any effects on the stability of employment more rigorously in Section 6.3.

Figure 4: Survival of employment relationships in treatment and control firms by period



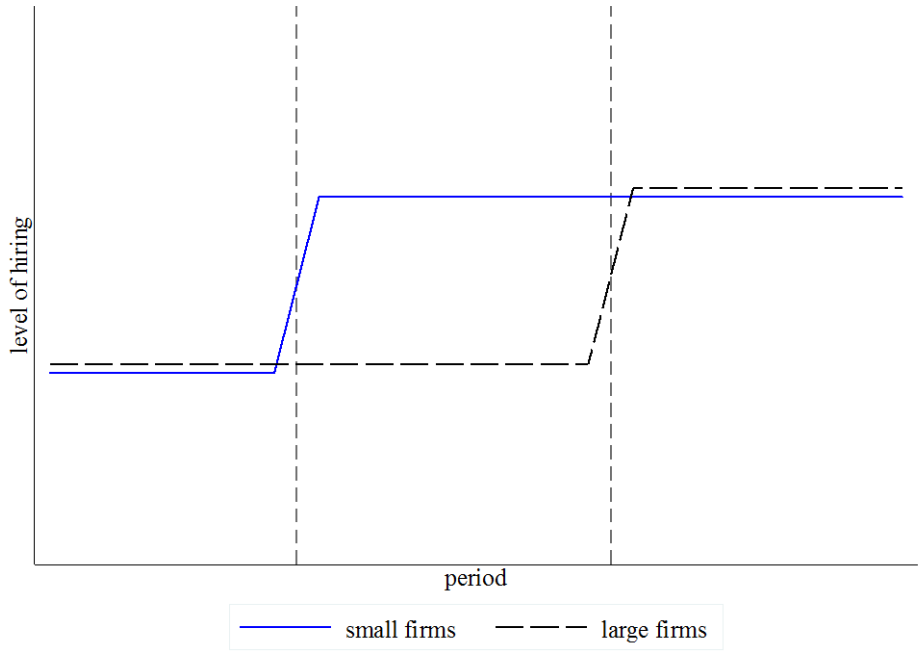
Notes: Figure shows the percentage of new hires surviving for the stated duration.

5 Empirical Strategy

We use the double natural experiment generated by the introductions of trial periods for small firms and subsequently for all firms to estimate how the ability to use trial periods affects a firm's hiring behaviour. We estimate the policy effect as the difference in hiring between firms with just under 20 employees and those with just over that did not exist previously, but that appeared 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 a means to check 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 was indeed an effect of the policy.

Figure 5 illustrates this idea with a stylised example. The solid blue line indicates the hiring behaviour of firms with fewer than 20 employees and the dashed black line the behaviour of those with 20 or over. The two vertical grey lines indicate the timing of the two policy changes. Before the first policy change, firms above and below the cutoff are very similar in behaviour. Upon the first policy change, the figure shows the hiring of small firms as shifting upwards. We would interpret the size of any such vertical shift as the magnitude of the policy effect. Upon the second policy change, the figure shows firms above the cutoff experiencing a matching vertical shift, as we would expect to see if the observed change in small firms' behaviour were the result of the policy.

Figure 5: Illustration of identification of policy effect



Notes: This is a stylised example of what a policy effect on the level of hiring would look like.

A major advantage of this methodology is that we won't mistakenly attribute to the policy any time trends in hiring that affected all firms equally. This is particularly important over the time period we study, which covers the development of the GFC and the subsequent recovery.

In our preferred specification, we use firms in the size range 15 to 24 employees only, to ensure those in the treatment and control groups are as similar as possible on unobservable dimensions. This means that even if there have been different time trends in hiring for very small and large firms, these are very unlikely to contaminate our results because firms in our two comparison groups are similar in size.

Firms may have taken some time after the policy changes to learn about trial periods and start to use them; we run some specifications in which we allow the discrete jump in hiring behaviour between small and large firms to vary each quarter in our sample, which would allow us to see such a pattern, were it present.

As discussed in Section 4.1, we consider a hire to be any employee who is paid a wage or salary by a firm in one month, but not the previous month. However, this measure includes employees with casual or periodic working arrangements, seasonal employees and other employees who have previously been employed by the same firm, as well as employees who are genuinely new to the firm. In most of our analysis we focus solely on the last group, "new" hires. These are the people whose characteristics and abilities are not well known to the firm, and thus who are riskier to take on. Furthermore, they are the only ones who are eligible to be hired on trial periods. If the policy has an effect, it is most likely to appear in behaviour relating to new hires.

Not all firms use trial period 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, because

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 data do not identify which firms or employment relationships use trial periods. Third, firms may use trial periods for some new hires but not others, so trial period use is not a cleanly defined concept at the firm level.

5.1 Quantity of hires

Our regressions that estimate the policy effect on the quantity of hiring by firms follow the general formulation explained above, 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 properly account for the count structure of the dependent variable.[22]

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

$$E(hires_{jt}) = exp(\alpha_0 + \alpha_1 small_{jt} + \alpha_2 between_t + \alpha_3 post_t + \alpha_4 between_t \times small_{jt} + \alpha_5 post_t \times small_{jt} + \alpha_6 ln(size_{jt})) \quad \textbf{(1)}$$

where *j* denotes firm and *t* denotes month.[23] 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 behaviour as firms get larger.

The coefficient α_4, on the interaction *between x 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, to be close to zero. Similarly we expect α_5 to be close to zero because the sum $\alpha_1 + \alpha_5$ gives the discrete difference between firms above and below the cutoff after all firms can use trial periods.

We cluster standard errors at the firm level to account for within-firm correlation over time.

In our preferred specification, we replace the indicators *between* and *post* with a set of fixed effects for each month in our sample period. This allows for changes over time that affect all firms equally, such as those driven by changes in economic conditions. We also include a set of calendar month fixed effects for each level 3 industry. This is a very flexible specification, which allows each industry to have its own seasonal pattern of hiring.

5.2 Probability of hiring a disadvantaged worker

Our analysis that estimates the policy effect on the hiring of disadvantaged jobseekers uses the same basic identification strategy, but a different type of regression. The regressions here define an observation as a hire, and the dependent variable is an indicator variable for the hire being of a particular disadvantaged type. We use linear

[22] We test and reject the null hypothesis that a poisson regression is the appropriate specification for our data.

[23] 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. However, we do run some specifications at the plant level.

probability regressions (OLS) for our main analysis, and conduct robustness checks in which we instead use probit regressions.

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

$$disadvant_{ijkmrt} = \alpha_0 + \alpha_1 small_{jt} + \alpha_4 between_t \times small_{jt} + \alpha_5 post_t \times small_{jt} + \alpha_6 ln(firm\_size_{jt}) + \alpha_7 ln(plant\_size_{jkt}) + \delta_{mr} + \gamma_t + \varepsilon_{ijkmrt} \quad \textbf{(2)}$$

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. 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 *small* is still defined at the firm level, although here we also have the size of plant that hires the worker, because it is the firm size that determines policy eligibility.

Again, we interpret the coefficient on the interaction *between x 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.

Similarly to above, we cluster standard errors at the firm level.

5.3 Duration of employment

Our regressions that analyse duration of employment are similar to the hiring-type regressions described above. 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 two, five, 12 or 24 months. The basic controls are as in equation ***(2)***, 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.

In our preferred specification we limit our sample to individuals hired by firms with between 15 and 24 employees. We cluster standard errors at the individual level.

5.4 Limitations and caveats to interpretation

Our empirical strategy provides the cleanest identification of the policy effect for firms with close to 20 employees, because this is the only part of the firm size range where we have a difference in trial period eligibility at a given point in time for firms that are otherwise similar in size. For this reason, our preferred specification includes only firms in the size range 15 to 24.

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 15 or more 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 preferred 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 to 50 employees. Second, we expand the size range to 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.

These latter regressions show how hiring by firms in each band differs from that of firms with 20-24 employees, and whether this difference changes with the first policy change or after the second. If trial period policy caused firms with fewer than 10 employees to increase their hiring, but not larger firms, we would expect to see positive coefficients in the interactions *between*sizes 1-4* and *between*sizes 5-9*, and similar positive coefficients on *post*sizes 1-4* and *post*sizes 5-9*. The reason these post interactions would not be expected to be zero, as their equivalents in our preferred specification, is that in this scenario the second policy change had no effect on the comparison group, large firms; if the first policy did not affect medium-sized firms just below the 20-employee cutoff, then nor will the second policy change affect firms above the 20-employee cutoff.

Note this pattern is indistinguishable from what we would see if very small firms decreased their hiring less because of the GFC, from which the economy has not fully recovered. The evidence we can present on the policy effect on very small firms should thus be interpreted as suggestive only.

There are several ways in which the hiring behaviour of treated (small) firms could affect the hiring behaviour 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.[24] 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 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.

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.[25]

[24] 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.

[25] Though Figure 6 does not suggest this was the case because it doesn't show even a short-term increase in quantity of hiring.

6 Results

6.1 Number of new hires

This section investigates whether trial period eligibility causes the average firm to increase the number of people it hires. Across specifications, we find the policy effect to be a precisely estimated zero. That is, our point estimates of the policy effect are close to zero and have small standard errors, meaning we can be confident the true policy effect is not far from zero.

Table 3 presents the results from negative binomial regressions at the firm-month level where the dependent variable is the number of new hires made by the firm during the month. We focus here on new hires, meaning individuals who have not worked for the firm before, because employees who have previously worked for an employer cannot be rehired by that employer with a trial period. We thus expect any increase in firm hiring caused by the policy to occur among new hires.

Column (1) presents a parsimonious specification, as shown in equation ***(1)***, where the only controls are an indicator for small firms, indicators for the between-policy period and the post-policy period, the interactions of small firms and these two period indicators, and the log of firm size. A policy effect would show up as a sizeable coefficient on the interaction of *between* and *small firm*.

Here we include only firms with 15 to 24 employees, so as to minimise unobservable differences between those firms subject to the first policy change and those subject to the second.

We see the coefficient on log firm size is large, positive, and statistically significant. Its magnitude of 0.994 indicates that a 10 percent increase in firm size corresponds to a 9.9 percent increase in hires.[26] In addition to this smooth relationship between firm size and hires, our specification allows for a discrete step in this relationship between firms with 19 employees and those with 20. The coefficient of -0.012 on *small firm* shows that before the first policy change the size of this step was negative 1.2 percent. This is an economically small difference, and is statistically insignificant.[27]

The large negative coefficients on the *between* and *post* period dummies show that, at least for large firms, levels of hiring after the 2009 policy change were lower on average than before, reflecting the change in economic conditions with the GFC.

[26] $(1.1)^{0.9}-1 = 0.099$

[27] 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. However, coefficients that are close to zero can approximately be interpreted as the percentage change in the dependent variable for each unit change in the independent variable.

Table 3: Policy effect on the number of new hires, varying controls and firm-size band

Firm sample:	Sizes 15-24	Sizes 15-24	Sizes 15-24	Sizes 18-21	Sizes 10-50	Sizes 15-24, excl 19, 20	Single-plant, sizes 15-24	Sizes 15-24, plant-level
Dependent variable: Number of new hires	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Between * Small firm	**-0.003**	**0.008**	**0.008**	**0.008**	**0.012**	**0.019**	**0.008**	**0.016**
	(0.018)	**(0.014)**	**(0.014)**	**(0.019)**	**(0.009)**	**(0.015)**	**(0.015)**	**(0.014)**
Post * Small firm	-0.003	0.000	0.000	-0.005	0.010	0.006	-0.010	-0.002
	(0.017)	(0.013)	(0.013)	(0.017)	(0.010)	(0.015)	(0.014)	(0.013)
Small firm	-0.012	-0.014	-0.015	-0.014	-0.016*	-0.020	-0.009	-0.009
	(0.015)	(0.011)	(0.011)	(0.016)	(0.009)	(0.016)	(0.012)	(0.011)
Firm size (ln)	0.994***	0.939***	0.937***	0.856***	0.964***	0.934***	0.948***	0.665***
	(0.044)	(0.034)	(0.034)	(0.128)	(0.010)	(0.044)	(0.035)	(0.034)
Plant size (ln)								0.000
								(0.001)
Between	-0.219***							
	(0.015)							
Post	-0.172***							
	(0.014)							
Month-in-year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Council employment shares			Yes					
Observations	*835,362*	*835,362*	*835,362*	*314,637*	*2,495,838*	*678,978*	*709,782*	*1,049,718*
% of non-zero hires	*46.6%*	*46.6%*	*46.6%*	*47.8%*	*45.0%*	*46.3%*	*47.0%*	*38.7%*
Alpha	1.730***	1.174***	1.166***	1.146***	1.176***	1.166***	1.145***	1.429***
	(0.019)	(0.013)	(0.013)	(0.015)	(0.011)	(0.013)	(0.013)	(0.016)

Notes: This table presents the coefficients from negative binomial regressions at the firm-month level (or plant-month level in column (8)) where the dependent variable is the firm's number of hires who had not worked for the firm in the previous five years. The sample period is January 2005 to March 2014. 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. Alpha is the coefficient in the dispersion function; rejection of alpha=0 implies rejection of a poisson relationship. Standard errors, in parentheses, are robust and clustered at the firm level (columns (1) to (7)) or plant level (column 8). Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

The coefficient on *between*small firm*, which captures the difference between the change in hiring for small firms relative to for large firms with the first policy change, is our estimate of the policy effect on new hires by firms. It is very close to zero and is precisely estimated. Its magnitude, -0.003, suggests the policy decreased hiring by 0.3 percent. Similarly, the coefficient on *post*small firm*, which captures the discrete step between small and large firms' hiring after the second policy change relative to before the first, is very small.

We also present our estimate of alpha for this regression, alpha being the coefficient in the dispersion function. We can reject the null that alpha is zero at conventional significance levels, which implies a poisson functional form is insufficient and the negative binomial that we run is preferable.

Column (2) of Table 3 adds two sets of fixed effects. The first set is for each month in our sample period, which allows for changes in hiring over time that occur for all firms equally, such as those driven by the business cycle. The second set is for month of the year interacted with level three industry. These allow for seasonal patterns in hiring that differ for each industry. Despite the high flexibility of this specification, our main result is not affected: we precisely estimate a policy effect that is close to zero.

Column (3) of Table 3 adds a set of controls that capture the geographic distribution of employment with the firm. Specifically, we control for the share of employment located within each regional council, allowing for the possibility that firms on either side of the eligibility cutoff tend to locate in different areas with different hiring propensities. Again, this does not affect our results.[28] As an alternative to our negative binomial regressions, we run two OLS specifications, presented in columns 4 and 5 and Appendix Table 2. The first is a linear probability regression with the dependent variable 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 with the dependent variable the log of new hires plus one. Here the coefficient on *between*small* is positive and significant (though economically small). However, it is similar to the coefficient on *post*small*, which makes it unlikely to indicate a genuine policy effect.

Columns (4) to (7) of Table 3 replicate column (2), but vary the firm sizes included in the specification. Column (4) uses a narrow band of firm sizes, column (5) uses a wide band, column 6 excludes firms right by the cutoff, and column (7) excludes firms with multiple plants. Our main result of no policy effect is robust to these alternative samples. Column (8) differs from the rest of this table in that an observation is a plant in a month, rather than a firm in a month. This allows us to control for plant size, which may affect hiring independently of the total size of the firm. Policy eligibility is still determined at the firm level, though, and so our sample is of plants in firms with 15-24 employees. The conclusion does not change.

Together, these results show no economy-wide increase in the number of new hires due to trial period policy, despite the fact that use of trial periods is fairly widespread, as outlined in Section 2.

[28] Our results are also not affected by our treatment of outliers in the data, or the way we measure firm size, as shown in the first three columns of Appendix Table 2.

Table 4: Policy effect for very small firms

	Neg. bin. regressions of the quantity of hiring		OLS regressions of prob. a new hire is of disadvantaged group type:					
Dependent variable:	Number of new hires	Number of new hires that lasted 5+ months	Beneficiary in past year	Not worked in past year	Immigrated to NZ in past 2 years	Under 25 years old	Māori or Pasifika under 25	Education leaver
Between * Size 1-4	-0.003	0.030*	0.002	-0.007*	-0.002	0.014***	0.006**	0.002
	(0.016)	(0.017)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.002)
Between * Size 5-9	0.015	0.034*	0.000	-0.003	-0.002	0.008*	0.006**	0.001
	(0.016)	(0.018)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.002)
Between * Size 10-14	0.017	0.023	0.000	-0.001	-0.000	-0.001	0.002	-0.002
	(0.016)	(0.018)	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Between * Size 15-19	0.003	0.009	-0.000	0.002	-0.001	-0.007	-0.001	-0.004
	(0.017)	(0.020)	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Between * Size 25-29	0.015	0.009	0.002	0.005	-0.004	0.006	0.001	-0.003
	(0.020)	(0.022)	(0.005)	(0.005)	(0.006)	(0.005)	(0.003)	(0.003)
Post * Size 1-4	-0.013	0.033**	0.000	-0.002	0.006	0.013***	0.003	0.002
	(0.014)	(0.014)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)
Post * Size 5-9	-0.005	0.017	0.001	0.006*	0.004	0.006	0.001	-0.000
	(0.015)	(0.014)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
Post * Size 10-14	-0.014	-0.011	-0.001	-0.000	-0.000	0.003	-0.002	0.002
	(0.015)	(0.015)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
Post * Size 15-19	-0.004	-0.005	0.005	0.001	-0.001	0.004	-0.001	0.004
	(0.015)	(0.015)	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.003)
Post * Size 25-29	-0.028	-0.029	-0.004	-0.005	-0.001	0.003	-0.004	-0.001
	(0.018)	(0.019)	(0.004)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)

Continued on following page

Dependent variable:	Number of new hires	Number of new hires that lasted 5+ months	Beneficiary in past year	Not worked in past year	Immigrated to NZ in past 2 years	Under 25 years old	Māori or Pasifika under 25	Education leaver
Coefficients continued from previous page								
Firm size (ln)	0.793***	0.825***	0.005**	-0.010***	0.005*	0.010***	0.002	0.002*
	(0.005)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Plant size (ln)			-0.000	0.005***	0.005**	0.008***	0.007***	-0.003***
			(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Firm size category fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1-digit industry fixed effects	Yes	Yes						
Calendar month * 3-digit industry FE			Yes	Yes	Yes	Yes	Yes	Yes
Observations	*15,423,435*	*15,423,435*	*2,116,056*	*2,116,056*	*2,116,056*	*2,104,680*	*2,104,680*	*1,823,559*
% of non-zero hires	*15.9%*	*8.5%*						
% hires of given type			*18.4%*	*26.0%*	*16.8%*	*41.5%*	*9.1%*	*12.2%*
R squared			*0.016*	*0.020*	*0.055*	*0.077*	*0.013*	*0.005*

Notes: The first two columns of this table present results from negative binomial regressions at the firm-month level of the number of hires, similar to column (2) of Table 3, but expanding the firm size sample to 1 to 29 employees. Subsequent columns present the results from linear probability regressions at the hire level that the employee is a disadvantaged worker, as in Table 6, but similarly expanding the sample to hires by firms with 1 to 29 employees. Columns (3) to (8) use a random 50% sample of the data for empirical tractability. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Our preferred sample includes only firms with 15 to 24 employees to keep treatment and control firms as similar as possible. However, one possibility is that firms with at least 15 employees are too large to be affected by trial period policy, whereas very small firms are affected. We therefore estimate a similar specification where we include in our sample firms with 1 to 29 employees, and allow the policy effect to differ for firms in each 5-employee size band. The results of this regression are presented in column (1) of Table 4.[29] The coefficients on the interactions of *between* with the small size categories are all small and insignificant; there is no evidence that the policy affected new hiring by very small firms.

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.

So far we have looked at the policy effect on new hires, and ignored re-employment of a prior employee by the same employer. New hires are where we would expect to see any increase in hiring, but total hires is the more relevant measure for looking at the aggregate effect on the economy. We thus replicate Table 3, replacing our dependent variable with the number of hires of any type. These regressions are presented in Appendix Table 3. They show no policy effect on overall hires.

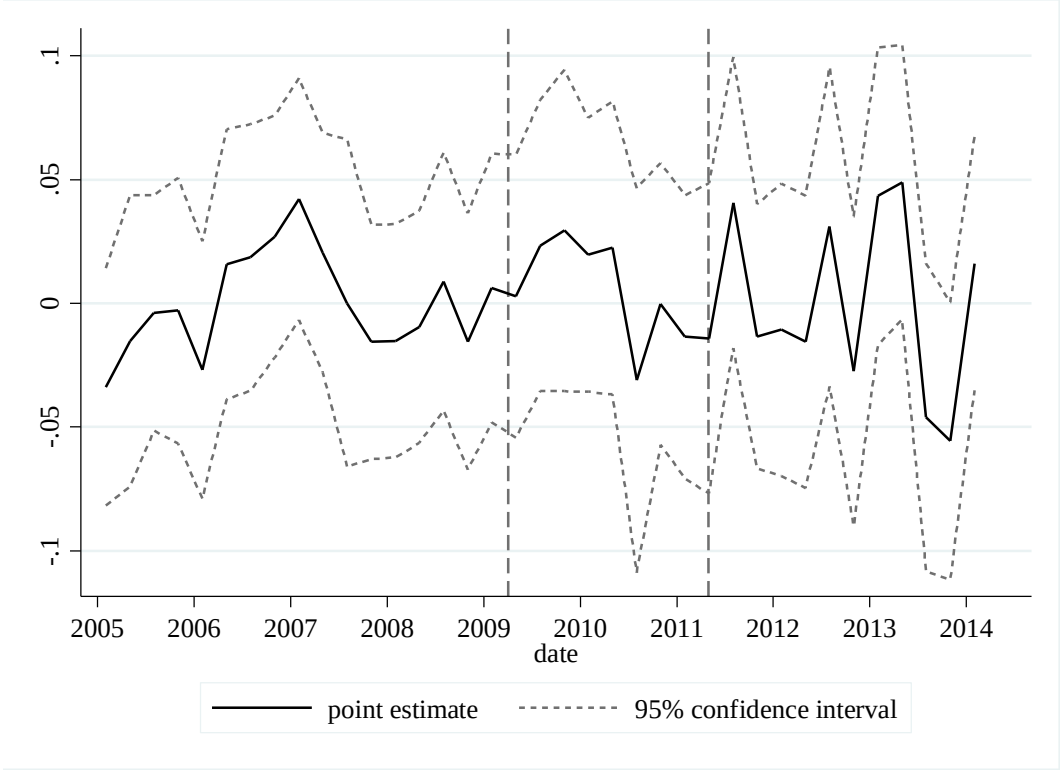
Firms could have taken some time to learn about trial periods and how to use them after their introduction, in which case our regressions that look for one effect throughout the between-policy period would understate the true policy effect. We thus estimate a version of column (2) of Table 3 where we 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.[30] These estimates are all close to zero and statistically insignificant. There is no evidence that a policy effect developed over time after the first policy change.

[29] The second column of this table is discussed below in Section 6.3; subsequent columns are discussed in Section 6.7.

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

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 (2) of Table 3, but allows the difference between small and large firms to vary each 3-month period.

6.2 Number of new hires for subpopulations of firms

Firms differ along many dimensions, some of which may affect the extent to which they use trial periods and can benefit from them. In this section we investigate whether specific types of firms increased their hiring because of trial period policy. Although an increase in hiring in certain types of firms, such as firms in certain industries, would not have generated the broad benefits of a significant increase in average hiring economy-wide, workers with skills relevant to the affected firms or industries could still benefit from increased job opportunities.

The IDI does not provide any information on whether a firm uses trial periods or whether an employee was hired on a trial period. However, MBIE (2014) shows that the reported use of trial periods varies by industry. Chart 1 from that report, which we replicate in Appendix Figure 4, shows a high proportion of employees in construction and wholesale trade are hired with trial periods, whereas a low proportion of employees in education and training are hired with trial periods. If trial periods increased hiring anywhere, we expect them to have increased hiring in these high-use industries. We therefore replicate some of our previous analysis separately for these high-use industries. For comparison, we also replicate the analysis for the low-use industries, education and training, where we expect to see no effect.

Table 5: Policy effect on hires in high-use and low-use industries, and on long-term hires and employees moving between firms

Dependent variable:	New hires	New hires that lasted 5+ months	New hires	New hires that lasted 5+ months	New hires that lasted 5+ months	New hires who were employed elsewhere prev. month
Industries/Firms included:	Construction and wholesale trade		Education and training		All	
Between * Small firm	**0.098****	**0.067***	**0.012**	**0.051**	**0.006**	**-0.002**
	(0.038)	**(0.041)**	**(0.032)**	**(0.036)**	**(0.015)**	**(0.017)**
Post * Small firm	0.043	0.004	-0.046	-0.041	0.006	-0.003
	(0.033)	(0.033)	(0.033)	(0.038)	(0.014)	(0.015)
Small firm	0.001	-0.014	0.030	0.036	-0.013	-0.016
	(0.030)	(0.032)	(0.028)	(0.034)	(0.012)	(0.013)
Firm size (ln)	0.990***	0.973***	0.851***	0.957***	1.000***	0.948***
	(0.085)	(0.094)	(0.087)	(0.102)	(0.037)	(0.041)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*130,056*	*130,056*	*80,496*	*80,496*	*835,362*	*835,362*
% of non-zero hires of given type	*39.4%*	*27.2%*	*55.1%*	*28.0%*	*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. High-use industries are construction and wholesale trade, based on MBIE (2014). Low-use industries are education and training. New hires are those who had not worked for the firm in the previous five years. The sample period is January 2005 to March 2014, and firms included are those sizes 15 to 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

In the first column of Table 5, we replicate our preferred specification, but limit our sample to firms in the high-use industries. The coefficient on *between*small firm* is less precisely estimated with the smaller sample, but increases in magnitude to an economically significant 0.098 and is statistically significant at the 5% level. The coefficient in *post*small firm* is smaller in magnitude, at 0.043, and is statistically insignificant; most of the difference that opens up between small and large firms with the first policy change disappears again after the second policy change. Taken at face value, the point estimate suggests firms in these two industries increased their hiring by 10.3 percent[31] as a result of trial period policy. Based on average monthly hiring in these industries between January 2005 and February 2009, this corresponds to an additional 95 hires each month, or over 1,100 hires each year at firms in construction and wholesale trade with 15 to 24 employees. If the effect size were the same for all sized firms in these industries,[32] this would mean an annual increase of around 12,100 hires in construction and wholesale trade, though many of these employment relationships would last for short periods only. In contrast, we find a small and insignificant increase of 1.2 percent in the low-use industries, as presented in column (3) of Table 5.

However, statistical tests yield false positives at the 5 percent level one in 20 times. To further investigate whether the positive and significant coefficient for high-use industries is likely to represent a genuine policy effect, in Panel A of Appendix Figure 6 we replicate Figure 6, which presents estimates of a time-varying policy effect, for the high-use industries. The point estimate rises above zero for a sustained period between the policy changes, though the 95 percent confidence interval typically encompasses zero, and falls (somewhat noisily) again after the second policy change. The timing of these changes is broadly consistent with the rise and subsequent fall being driven by the two policy changes. We interpret this as weak evidence of an economically significant policy effect for construction and wholesale trade. Panel B of Appendix Figure 6 repeats this for the low-use industries. As expected, there is no evidence of a policy effect for education and training.

Appendix Figure 5 presents results from our preferred specification replicated for each level 1 industry. The coefficients on *between*small* and their confidence intervals are presented in Panel A; the coefficients on *post*small* are presented in Panel B. This figure shows a positive and significant policy effect in Construction, and negative and significant policy effects in Electricity, Gas, Water and Waste Services, and Transport, Postal and Warehousing. Several of the coefficients on *post*small* are also significant at the 5 percent level. However, with this number of estimates the probability of a false positive is high, so these results should not be over-interpreted.

6.3 Number of new hires who remained beyond the trial period

Because trial periods make it easier for an employer to dismiss a worker within her first 90 days with the firm, the rate of hiring into stable employment relationships, defined as those that last beyond 90 days, may have been affected differently to the rate of overall hiring. We have shown that the overall rate of new hires was not affected by the policy, so if the policy increased early dismissals it could have actually decreased hiring into stable

[31] exp(0.098) – 1 = 10.3%

[32] Note this is a strong assumption. If the policy effect is weaker for larger firms, the overall number is likely to be smaller.

employment. Here we investigate this possibility by looking at the effect of trial periods on hires where the employment lasted at least five calendar months.[33]

Separations can occur either because an employee leaves voluntarily, or because he is dismissed, and we are unable to distinguish the two types of separations 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. Any such effect is likely to be similar for employees at firms of sizes 15 to 19 and those at firms of sizes 20 to 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 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 behaviour.

Column (5) of Table 5 presents the results of a regression where the dependent variable is the number of hires into relationships lasting at least five months. The coefficient on *between*small firm* is small and insignificant, providing no evidence that long-term hires were affected by the policy. See Section 6.8 for more detailed analysis of the effect of the policy on the distribution of employment duration.

The second column of Table 4 repeats this analysis, but includes firms of sizes from 1 to 29 employees, and allows the policy effect to differ for firms in different size categories. As discussed in Section 5.4, identification of the effect for very small firms is less clean than identification for firms close to the cutoff, so these results should be interpreted as suggestive only. The coefficients on the interactions *between*size 1-4* and *between*size 5-9* are positive and borderline significant. The economic magnitudes of the point estimates are small, suggesting around a 3 percent increase in hiring as a result of the policy. The coefficient on the interaction *post*size 1-4* is of similar magnitude, but that on *post*size 5-9* is somewhat smaller and insignificant.

Taken together, these results are consistent with an economically small but positive effect on hiring into long term employment relationships by firms with fewer than 10 employees. However, in the context of not finding an effect on new hiring overall, the direction of this effect is the opposite to expected. If anything, trial period policy is expected to increase the number of dismissals within 90 days of hiring, whereas these results, taken at face value, suggest a small decrease in dismissals. The results are also consistent with no effect of the policy, coupled with a time trend in economic conditions that differentially affected the willingness of very small firms to dismiss employees, or the willingness of employees in very small firms to leave their jobs. The latter could have been a result of the GFC; Mare and Fabling (2013) show that overall separations actually *decreased* in this period, presumably driven by a reduction in voluntary separations that outweighed the increase in involuntary separations. Our results could be explained by a differential willingness to leave employment in tough economic times for workers in very small versus larger firms.

Column 2 of Table 5 presents the results of regressions estimating the policy effect on new hires that lasted at least five months run separately for the high-use industries, construction and wholesale trade. The coefficient on *between*small* for high-use industries is 0.067, which suggests long term hires in these industries increased by 6.9%

[33] 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.

as a result of the policy. The coefficient is statistically significant at the 10% level and is economically significant. The comparison between the estimated policy effect here and the estimated policy effect for all new hires in these industries is also potentially informative: new long term hires seem to have increased, but less than new hires overall, suggesting there could also have been an increase in the dismissal rate of recently hired workers. However, these differences are unlikely to be statistically significant. The duration of employment relationships for these firms is examined further in Section 6.8.

Column 4 of Table 5 presents results of a similar regression run for firms in the low-use industries, education and training, only. The estimated policy effect here is an imprecisely estimated and statistically insignificant 5.2 percent increase in long term hiring.

Although the coefficients in these industry-specific regressions are not precisely estimated, they are consistent with the policy increasing both short and long term hiring in high-use industries but not in low-use industries.

6.4 Reluctance to leave secure employment

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 to see employees who move straight from one job to another being less likely to move to a firm that was eligible to use trial periods.

We look for such an effect in column (6) of Table 5. The dependent variable is the number of new hires who worked at a different firm the month before they were hired. 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 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.

6.5 Hiring in different parts of the earnings distribution

In this section we test whether trial period policy increased firm hiring in any quartile of the earnings distribution. Disadvantaged workers, the intended beneficiaries of the policy, are more likely to be lower earners; if they are disproportionately affected, we would expect a greater policy effect on hires of lower paid workers.

The four columns of Appendix Table 4 present our preferred specification estimated separately for each quartile of the earnings distribution. We can measure monthly earnings in cases where an employee remained with the firm during at least three consecutive calendar months only, so we use as our dependent variable the number of hires in the earnings quartile that lasted at least three months.[34] This measure falls somewhere between total new hires and new hires into long term employment relationships.

[34] See the Data section for details of how workers' earnings and our earnings quartiles were calculated.

None of these earnings quartiles show an economically or statistically significant increase in hiring as a result of the policy.

More subtly, firms that use trial periods might have to offer higher wages to similar workers, at least initially, in order to compensate them for their lack of job security. If such an effect were large enough to shift a considerable number of hires between wage quartiles, we would expect to see it in the regressions presented here. Specifically, we would see firms eligible to hire on trial periods hiring more in higher wage quartiles and less in lower wage quartiles. However, small wage increases as compensation for job insecurity would likely not be noticeable here, and we don't conduct analysis that specifically tests for them.

6.6 Change in employment

Employment both at the firm level and the economy level depends on the separation rate as well as the hiring rate. Separations for employees on trial periods could be affected by trial period policy because employees are less costly to dismiss; separations for employees not currently on trial periods could be affected because dismissed employees are less risky to replace. To test whether the policy caused a change in the balance of hiring against separations, in this section we use a similar strategy to above to test the policy effect on change in employment.

Our dependent variable is percentage change in employment at the firm over the month. Change in employment can take negative values, so here we use simple OLS regressions. We control for a fifth order polynomial in firm size instead of log firm size as in previous regressions, because here the log specification does not provide a good fit for the data.

Appendix Table 5 presents the results from specifications that otherwise parallel those in Table 3. The columns vary by controls included and the firm sizes included in the sample.

Here the coefficient on *between*small firm* is interpreted simply as the effect of the policy on the monthly percentage change in employment. The mean percentage change in employment for firms in our sample is 0.4%, with a standard deviation of 7.8%. The estimated policy effects are mostly statistically significant and negative, but in economic terms they are small. The largest in magnitude is 0.599, meaning the estimated policy effect was a 0.6 percentage point decrease in monthly percentage change in size, which is only 0.08 of a standard deviation.

Although the magnitude is small, the consistently negative estimated policy effect is a trifle mystifying. Note however that the coefficients on *post*small firm* are also negative and significant, and in most cases nearly as large as the coefficients on *between*small firm*. This suggests some other change in the economy is driving these coefficients; the Global Financial Crisis is an obvious candidate.

Overall, these results provide no evidence that firms' rate of growth in number of employees was increased by trial period policy.

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	Māori 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 firm is size 15 to 24 and the employee had not worked for the firm in the previous five years. The sample period is 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

6.7 Types of hires

We have shown trial period policy has no economically significant effect on the numbers of hires firms make on average. In this section we investigate its effect on the composition of the hires firms do make. If disadvantaged jobseekers disproportionately benefited from the policy, as some of its supporters claimed they would, we would see an increase in the likelihood a hire was some type of disadvantaged jobseeker. Only new hires are potentially eligible to be hired on trial periods, so, as previously, we primarily focus on whether *new* hires are disadvantaged jobseekers.

For a range of definitions of disadvantaged jobseeker, 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 disadvantaged jobseeker. Hires at firms with 15 to 24 employees are included in the sample. The types of disadvantaged 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 two 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 any of these types of workers.[35] For example, column 1 shows people who have earned benefit income in the past 12 months make up 18 percent of all new hires. The point estimate of the policy effect on the probability a new hire is a former beneficiary is an insignificant 0.3 percentage points. The estimated policy effects for the other types of disadvantaged hires are similarly small and insignificant. These results are robust to estimation by probit instead of OLS, as shown in Appendix Table 6, and to varying the firm size range included in the sample, as shown in Appendix Table 7.[36]

To test whether the policy caused firms to substitute new workers of disadvantaged types for workers whom they had employed previously (and thus who were not eligible to be hired on trial periods), we replicate Table 6, but expand observations to be any hires rather than just new hires, and define the dependent variable as an indicator for the worker being both new to the firm and of the disadvantaged type. These regressions, which again do not show an economically significant policy effect, are presented in Appendix Table 8.

Our identifying assumptions are most likely to hold for firms close in size to the 20-employee cutoff, but smaller firms may have been more affected by trial period policy. In columns (3) to (8) of Table 4, we present versions of the disadvantaged type regressions in which the firm sample is expanded to all firms with 1 to 29 employees, and firms in different size bands are allowed to be affected differently by the policy. These results should be interpreted with caution, because very small firms may have been differently affected by the changing economic conditions around 2009 relative to firms with 20 or

[35] 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.

[36] In the case of the wide size range sample, firms with 10 to 50 employees, beneficiary hires and jobseeker beneficiary hires appear statistically significantly more likely. However, the coefficients are small, indicating a 0.4 or 0.5 percentage point increase in the probability a hire is a beneficiary, and the other size ranges do not show the same result. Note also the estimates for this sample are more likely to be contaminated by differential time trends for very small or large firms.

more employees. There is no evidence that very small firms increased their hiring of beneficiaries, non-workers, migrants, or education leavers. However, the coefficients on *between*small* are positive and statistically significant for firms with fewer than 10 employees in the regressions studying youths under 25 and Māori and Pasifika under 25. For the former, the coefficients are 0.014 and 0.008 for firms with 1-4 and 5-9 employees respectively, implying a 0.8 to 1.4 percentage point increase in the probability a new hire at these firms is young (a 1.9 to 3.4 percent increase relative to the mean of 41.5 percent). These coefficients are both 0.006 for young Māori and Pasifika, implying a 0.6 percentage point increase (6.6 percent relative to the mean of 9.1 percent).

As explained in Section 5.4, if these positive coefficients indicated a genuine policy effect we would expect the corresponding coefficients on *post*firm size* to be similar. This is somewhat the case for youths, but less so for young Māori and Pasifika. Furthermore, differential time trends for larger and smaller firms could generate a similar pattern. Overall, these regression results are consistent with the policy causing a small increase in the probability a hire at a firm with fewer than 10 employees is under 25, but we are unable to rule out alternative explanations.

Disadvantaged jobseekers make up a higher proportion of hires among lower paid employees, so we are more likely to see an effect on the proportion of disadvantaged jobseeker hires in the lower parts of the earnings distribution. We thus replicate Table 6 for each quartile of the earnings distribution, using our preferred sample of firms with 15 to 24 employees, and limiting our dependent variable to include new hires who lasted at least three months only. These results are presented in Appendix Table 9.

For hires in the lowest earnings quartile, the coefficients on *between*small firm* are all small and insignificant except for in the regression looking at those who haven't worked in the past year ("non-workers"). The coefficient here is 0.016, which is significantly different from zero at the 5% level. However, one in every twenty coefficients estimated will come out as significant at the 5% level purely by chance when the true effect is zero. Additional evidence cautions against interpreting this coefficient as a significant policy effect. The coefficient on *small firm* in this regression is -0.009, which is larger than in the other regressions presented, though it remains insignificant. This means that after the first policy change the difference between small and large firms in the probability that a new hire was a non-worker was only 0.7 percentage points,[37] relative to an average of 34 percent. Economically speaking, this is a small difference, if indeed it is non-zero.

In the middle two earnings quartiles, all the coefficients of interest are small and insignificant, and in the top earnings quartile all but one are; the coefficient -0.009 in the regression for the employee being under 25 years old. It is significant only at the 10% level, and its sign is the opposite of that predicted by theory. The statistical significance is very likely to merely represent a false positive.

If trial period policy did increase the probability a hire was a disadvantaged worker, we're more likely to see this effect in the industries with high use of trial periods. We thus replicate our analysis of the hiring of disadvantaged workers in the high-use industries, construction and wholesale trade, and for comparison also in the low-use industries, education and training. These results are presented in the top panel of Appendix Table 10.[38]

[37] (0.016 – 0.009)*100

[38] High-use and low-use industries are just two subsets of industries where we expect to see the extremes of results. To put these results in context, we also replicate the hiring type analysis individually for each level 1 industry. The coefficients on *between*small*

A comparison of these two tables shows that for every type of disadvantaged worker except education leavers, the probability a hire was that type of disadvantaged worker was higher in the high-use industries than in the low-use industries. Although this doesn't constitute proof, it is consistent with disadvantaged workers being more likely to be hired with trial periods.

Appendix Table 10 presents very weak evidence that trial periods may have made firms in high-use industries 1.5 percentage points more likely to hire people who had received a jobseeker benefit in the previous 12 months, and 1.4 percentage points more likely to hire Māori and Pasifika under 25 years old. However, the coefficients on *small firm* in these regressions are of similar magnitude. These are expected to be approximately zero, so their similarity to the coefficients of interest suggests the positive estimated policy effects probably merely represent noise. As further evidence along these lines, the estimated policy effect was of comparable magnitude and borderline significant, but negative, for education leavers.

The policy effects on hiring of disadvantaged workers in low-use industries, presented in the lower panel of Table 7 are more precisely estimated; all are small and insignificant except for the case of education leavers. The estimated policy effect for education leavers is a 0.9 percentage point increase, which is small and significant at the 10% level only. Coupled with the fact the coefficient on *small firm* in this regression is of the same magnitude but opposite sign, this result is very likely a false positive.

6.8 Duration of employment

In this section we investigate how trial period policy affected the length of time a newly hired worker could expect to stay in his job. One potential concern is that trial periods 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 would be detrimental to such workers, who would never acquire a measure of job security. On the other hand, 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.

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. We look at the periods 2 months, 5 months, 12 months, and 24 months.[39] About 77 percent of all new hires last at least two months, 46 percent last 5 months, 24 percent last 12 months, and 13 percent last 24 months.

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and *post*small* for each type of disadvantaged jobseeker and each industry are presented in the panels of Appendix Figure 7. The policy effects are less precisely estimated in these regressions because the number of observations is much smaller. The number of statistically significant coefficients is similar to what we would expect purely by chance, and these are split relatively evenly between the coefficients on *between*small* and *post*small*. Overall, these results do not provide convincing evidence that any type of disadvantaged jobseeker gained from trial periods in any industry.

[39] 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	5 months	12 months	12 months	24 months	24 months
Between * Small firm	**0.003**	**0.002**	**0.000**	**-0.002**	**-0.001**	**-0.001**	**-0.002**	**-0.002**
	(0.002)	**(0.002)**	**(0.003)**	**(0.003)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**
Post * Small firm	0.007**	0.006**	0.001	0.000	-0.003	-0.003	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Small firm	-0.001	-0.003	-0.003	-0.003	0.000	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Firm size (ln)	0.026***	0.014**	0.032***	0.029***	0.026***	0.025***	0.014***	0.015***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)	(0.005)	(0.005)
Plant size (ln)	-0.015***	-0.013***	-0.022***	-0.019***	-0.011***	-0.009***	-0.007***	-0.005***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.013***		0.018***		0.015***		0.012***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared (/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)
Māori		-0.012***		-0.028***		-0.027***		-0.020***
		(0.001)		(0.002)		(0.001)		(0.001)

Continued on following page

Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
Coefficients continued from previous page								
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	Yes	Yes	Yes
Calendar month * 3-digit industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*723,084*	*671,073*	*723,084*	*671,073*	*723,084*	*671,073*	*723,084*	*671,073*
% hires of given type	*76.8%*	*77.8%*	*46.1%*	*48.3%*	*24.4%*	*26.0%*	*13.2%*	*14.2%*
R squared	*0.118*	*0.125*	*0.142*	*0.146*	*0.112*	*0.123*	*0.084*	*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 size 15 to 24 and the employee had not worked for the firm in the previous five years. The sample period is 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

We can't know from our data the nature of the employment relationship, for example whether either party believes 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 two 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 percentage point increase in the probability an employment relationship lasted at least two months, relative to an average of 77 percent. The other coefficients are similarly small. It does not appear that employees hired by a firm eligible to use trial periods have their tenure with their employer affected by this fact.

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-forties 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 two months, but less likely to reach five, 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.

Appendix Table 11 replicates Table 7 for these hires. It estimates the policy effect on the probability a re-hire remained with his employer for two, five, 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 percentage point decrease to a 0.3 percentage point increase in the probability the worker survived until the milestone; these values are economically small and statistically insignificant.

We next examine whether trial periods affected the employment duration of new hires in high-use or low-use industries. The results of these regressions are presented in Appendix Table 12. In the case of high-use industries, several of the coefficients on *between*small* are negative and significant at the 10% level, suggesting that after the first policy change new hires at small firms in these industries were less likely to survive for five or more months. However, the coefficients on *post*small* are also negative and are substantially larger in magnitude for every duration. If the policy were the reason for the significant coefficients on *between*small*, the coefficients on *post*small* would be close to zero. This suggests other changes in the construction or wholesale trade industries were occurring that affected the stability of employment differentially for firms with 15 to 19 employees relative to those with 20 to 24 employees. One possible explanation for the large *post*small* coefficients is the Christchurch earthquake, which occurred just before the second policy change. These strong differential time trends mean we are unable to draw any conclusions about the policy effect on the stability of employment in construction and wholesale trade.

For the low-use industries education and training, the *between*small* coefficients are negative and significant at the 5% level for our 24-month specifications. However, this likely represents noise; the *post*small* coefficients for the 12-month and 24-month durations are negative and of similar magnitude (although not statistically significant for the 24-month specification). In addition, if the policy were decreasing the long-term duration of new hires, then we also ought to see an effect on the likelihood of a new hire lasting 5 months or 12 months; there are no obvious reasons for trial period policy to decrease the probability employment lasts 24 months while having no effect on the probability it lasts a year or less.

Disadvantaged jobseekers are probably more likely to be hired on trial periods, and 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 disadvantaged hire. These results are presented in Appendix Table 13. They suggest no significant effect on duration for any type of disadvantaged jobseeker.

6.9 Reconciliation with NZIER (2011) findings

NZIER (2011) use aggregated linked employer-employee data to estimate that trial periods increased the annual proportional change in hiring by an average of nearly six percentage points for eligible firms across the economy. A number of possible explanations exist for the difference between this finding and our result of no significant effect on hiring on average.

First, their study was conducted when less data were available; they had access to only six months of data after the first policy change. To match this "post" data, they limited their sample to the second and third quarters of the year even during the pre-policy period. In column (1) of Table 8, we repeat our preferred specification (as in column (2) of Table 1), but limiting our sample to the months of data that NZIER used. The difference in samples alone does not explain the difference in results. Here we find the estimated policy effect is a 0.8 percent increase in hires, which is much smaller than NZIER's estimate and is statistically insignificant.

NZIER used firms of all sizes, so next we expand our firm size sample similarly, while keeping to the months of data they used. Columns (2) and (3) present two different specifications for this sample. Column (2) allows the magnitude of the policy effect to be different for firms in each size band, whereas column (3) restricts the policy effect to be the same for firms of all sizes. In column (2) the coefficients on *between*size category* are more positive for smaller firms and more negative for larger firms. This pattern is consistent with larger firms being more negatively affected than small firms by the Global Financial Crisis, and is inconsistent with a positive effect of trial period policy; no discrete difference between firms above and below the eligibility cut-off appears after the first policy change. In column (3), where we impose the restriction that all small firms were affected equally by the policy change and use all large firms as the comparison group, we estimate the policy caused a striking and misleading 21.4 percent increase in hiring.

Rosenberg (2011) emphasises that external shocks differentially affecting very small and large firms could affect NZIER's estimation of the policy effect, which is a concern because exporters of manufactured goods were especially hurt by the GFC and tend to be large. In contrast, small firms tended to fare better in the aftermath, around the same time trial periods were introduced. NZIER are conscious of this issue, stating:

> If economic conditions from April 2009 to September 2009 were different for [small- and medium-sized enterprises] than for larger firms, then the policy impacts reported here may be over- or understated. (NZIER, 2011)

Our results suggest this explains NZIER's positive policy estimate; it was driven by a combination of aggregated data, limited time periods, and treatment and control groups that were differentially affected by the GFC.

Table 8: Reconciliation with NZIER findings

Firm sample:	Sizes 15-24	All firm sizes	All firm sizes
Dependent variable: number of new hires	(1)	(2)	(3)
Between * Small firm	**0.008**		**0.194*** **
	(0.024)		**(0.051)**
Between * Size 1-5		**0.108**	
		(0.078)	
Between * Size 6-9		**0.054**	
		(0.039)	
Between * Size 10-19		**0.010**	
		(0.041)	
Between * Size 50-99		**-0.019**	
		(0.103)	
Between * Size 100+		**-0.189*** **	
		(0.062)	
Small firm	0.018		
	(0.018)		
Firm size (ln)	1.016***	0.846***	
	(0.063)	(0.020)	
Firm size category dummies (20-49 omitted):			
Size 1-5		-0.354***	-0.373***
		(0.059)	(0.055)
Size 6-9		-0.316***	-0.346***
		(0.041)	(0.040)
Size 10-19		-0.157***	-0.196***
		(0.036)	(0.033)
Size 50-99		0.110	0.107*
		(0.067)	(0.057)
Size 100+		0.182***	0.138**
		(0.062)	(0.061)
Month-in-year fixed effects	Yes	Yes	Yes
Calendar month * 3-digit industry FE	Yes		
Level 1 industry fixed effects		Yes	Yes
Observations	*182,250*	*4,256,367*	*4,256,367*
% of non-zero hires	*46.8%*	*29.6%*	*29.6%*

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 who had not worked for the firm in the previous five years. The sample period is the second and third quarters of the year only, from 2006 to 2009. Small firms are those with fewer than 20 employees, which were eligible to use trial periods after the first policy change. Between refers to the time period after the first policy change. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

7 Conclusions

In March 2009, an amendment to the Employment Relations Act (2000) came into effect that introduced 90-day trial periods in employment for firms with fewer than 20 employees. A worker new to a small firm could be hired on a trial period, and for the first 90 days of her employment the legal requirements for dismissing her would be much reduced. The 2009 policy was introduced partly in response to the Global Financial Crisis, with the hope the policy would stimulate employment by small firms by reducing the cost to them of hiring a person who turned out to be a bad fit for the job. Potential anticipated costs included a decrease in job stability, particularly for marginal workers.

The policy was deemed a success, and in April 2011 the option of using trial periods was extended to firms of all sizes.

Surveys suggest the trial period policy has considerable support among firms. Trial periods seem to be widely used, and yet dismissals within the 90 days are relatively infrequent. Preliminary analysis by NZIER of aggregated linked employer-employee data from the first six months after the policy's introduction found the policy had "…improved labour market flexibility, increased hiring activity, and lifted total job numbers."

In this research we revisit the questions of 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 use individual-level linked employer-employee data from Statistics New Zealand's Integrated Data Infrastructure, and take advantage of the natural experiment offered by the two policy changes. In particular, we look for a discrete difference in behaviour between firms just above and just below the 20-employee cutoff that opened up after the 2009 policy change, and closed again after the 2011 change.

In contrast to previous results, we find no evidence that the policy had an economically significant effect on the quantity of hiring by firms on average across industries. However, when we focus on firms in construction and wholesale trade, two industries that report high use of trial periods, we see statistically weak evidence that trial period policy could have increased new hiring in these industries by around 10 percent; in contrast, we see no such effect in education and training, two industries that report particularly low trial period use.

Both across industries and in high-use industries, we see no evidence that trial period policy altered the probability that an individual hired by a firm with at least 15 employees was young, a recent education leaver, a recent migrant, a recent beneficiary, a young Māori or Pasifika, or a person who had not worked in the preceding year. That is, these types of disadvantaged workers did not seem to disproportionately benefit from (or pay the employment costs of) the policy. Our analysis of very small firms is consistent with the policy increasing by about 1.2 percentage points the probability that a hire at a firm with fewer than 10 employees is aged under 25. However, this result is suggestive only, and could be driven by changing economic conditions rather than trial period policy.

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

We interpret our results as showing that any effect of trial period policy on firm hiring or dismissal behaviour has been economically insignificant at the economy level. Jobseekers overall were not more likely to find employment, long term or otherwise, because of the policy, though those with skills relevant to the construction or wholesale trade industries may have faced some increase in job opportunities.

It seems the primary effects of the policy were to reduce the cost to firms of continuing their pre-policy behaviour, 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. In some cases, high training 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 training 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.

Prior to trial periods being available, firms had at their disposal several alternative types of employment arrangements, such as fixed term contracts and contracts for casual work, which allowed employers to evaluate hires before committing to permanent employment relationships.[40] Trial periods may haves had a limited effect on hiring because in many cases they replaced these alternative types of arrangement.

Another possibility is that the policy change did not reduce dismissal costs as much as policymakers and firms believe. Finally, firm hiring decisions may be primarily based on their expected demand for labour as determined by demand for their output, and reducing the costs of dismissal may act purely as a reduction in costs for most firms.

[40] 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.

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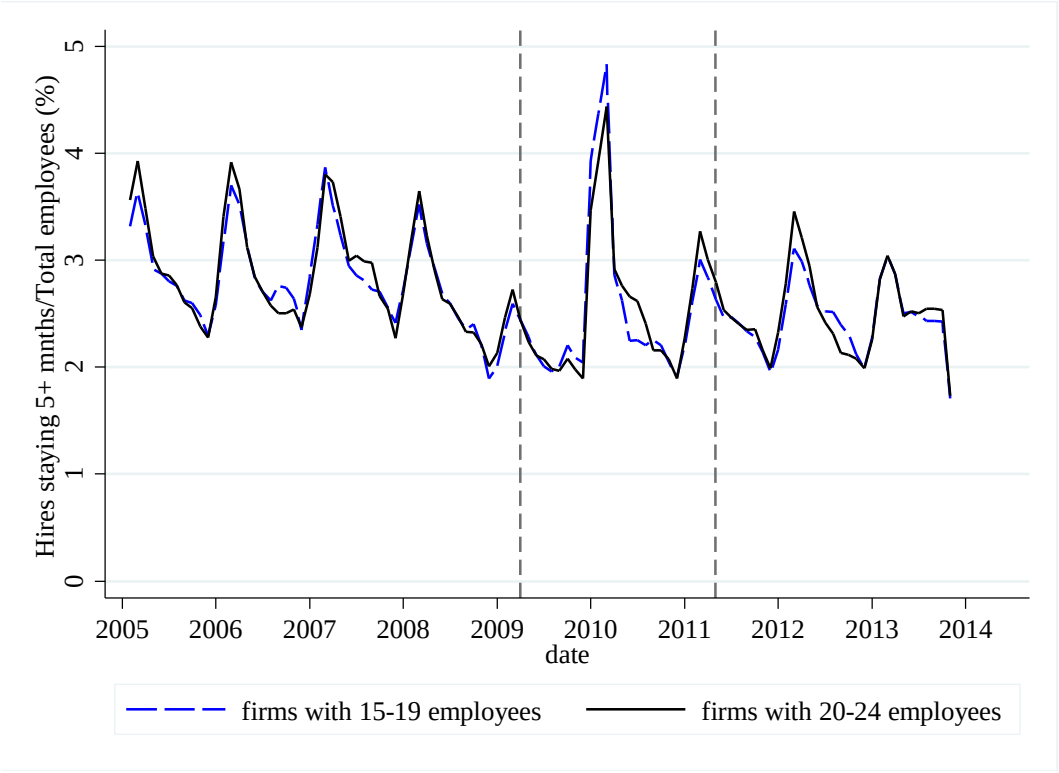
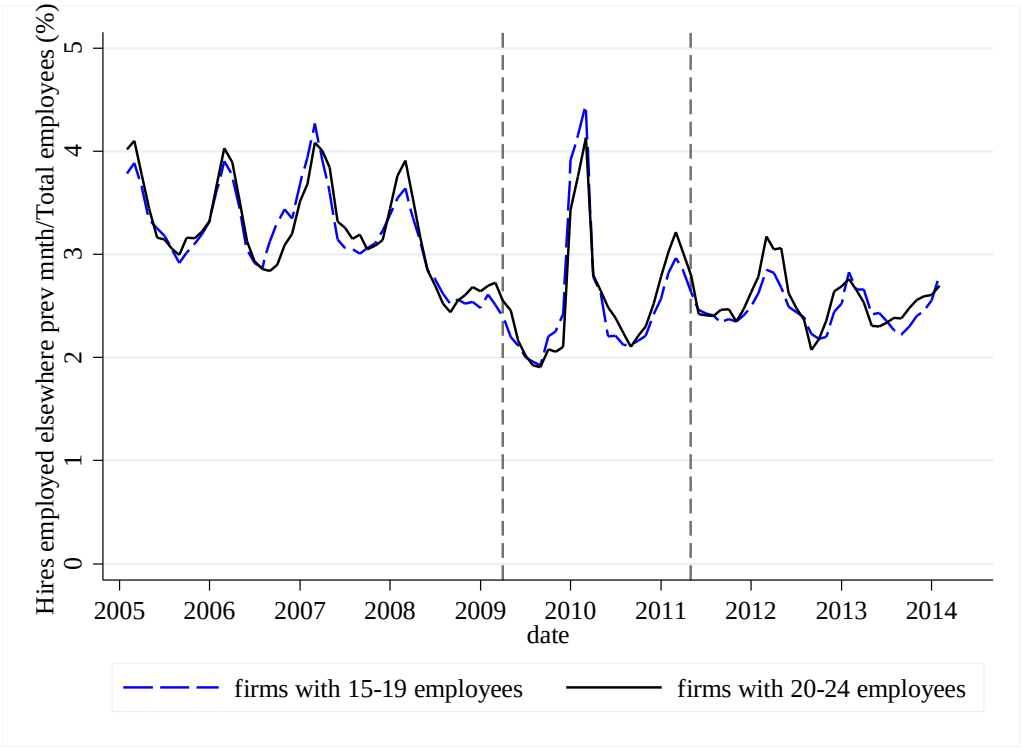
Appendix Figures

Appendix Figure 1: New long term hires as a percentage of total employees for treatment and control firms

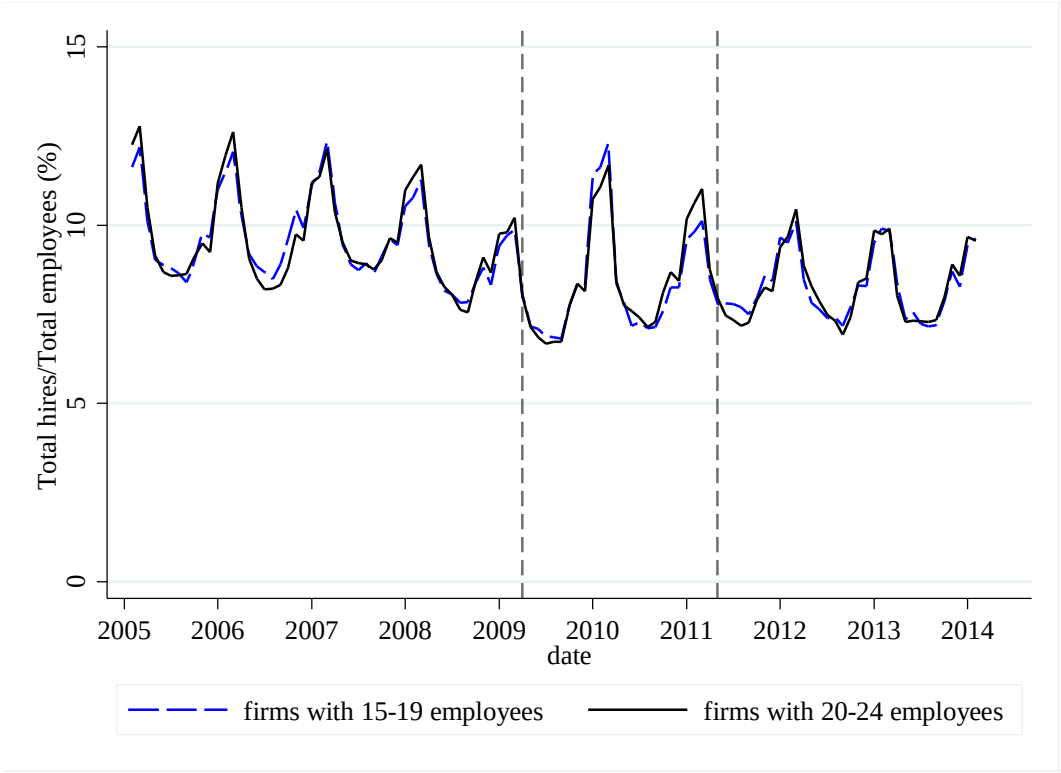


Notes: Long term hires are hires that last at least five months. Values are three-month moving averages. Vertical grey lines indicate the policy changes.

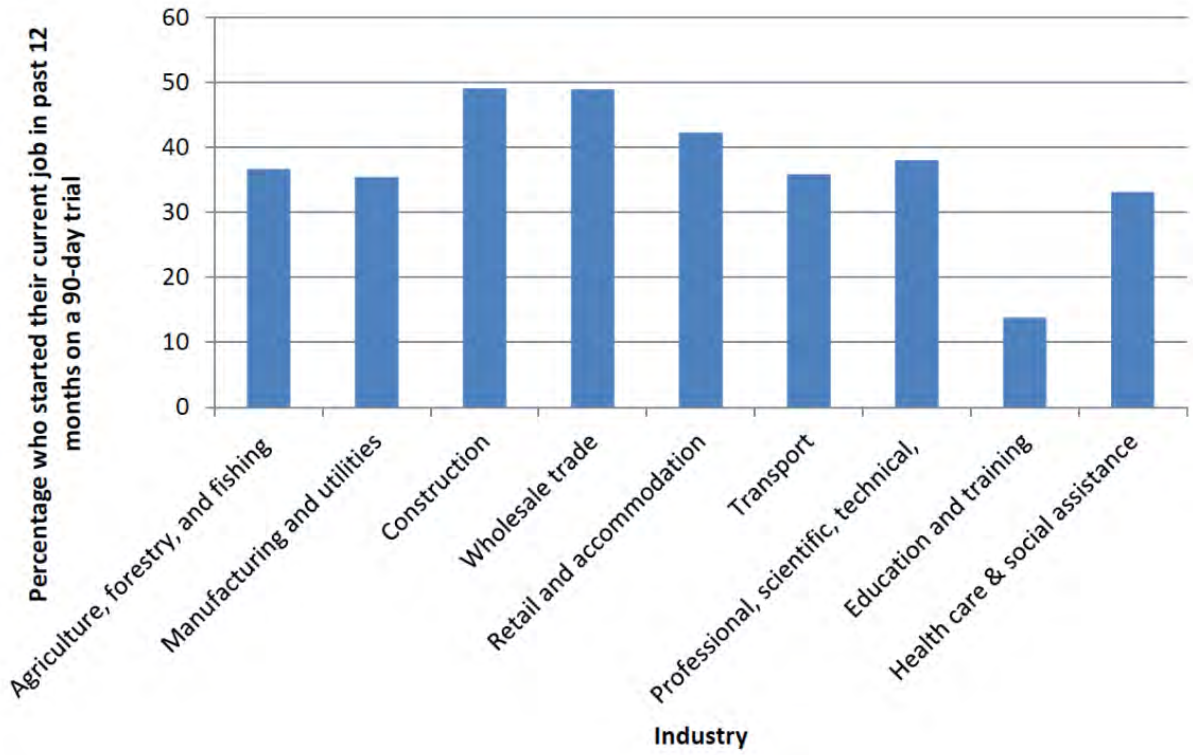
Appendix Figure 2: New hires moving from other jobs as a percentage of total employees for treatment and control firms



Appendix Figure 3: Total hires as a percentage of total employees for treatment and control firms



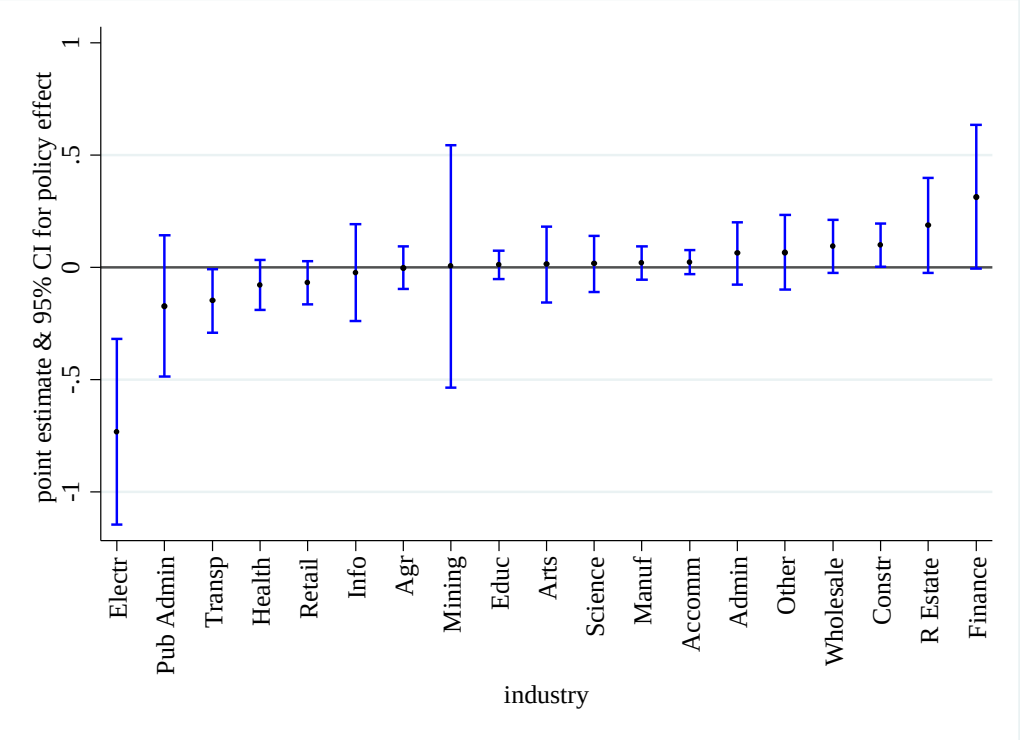
Appendix Figure 4: Trial period use by industry



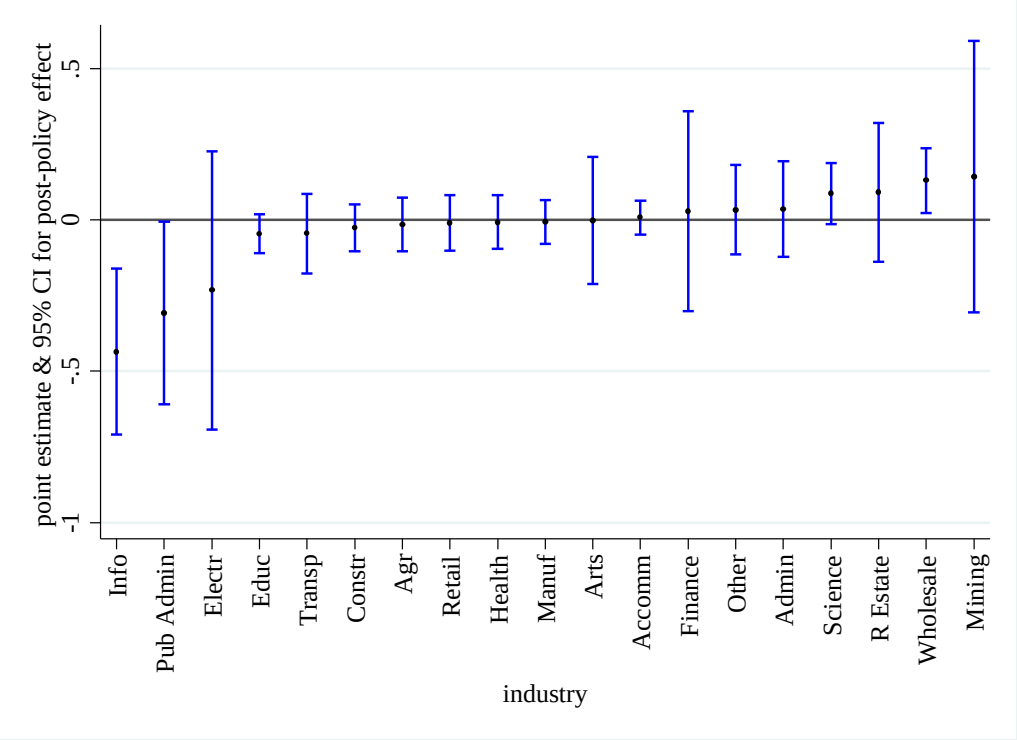
Notes: This figure a reproduction of Chart 1 from MBIE (2014). For employees who started their jobs in the previous 12 months, it shows the percentage that started their job on a trial period by industry.

Appendix Figure 5: Policy effect on the number of new hires by industry

Panel A: Coefficients on *between*small firm*



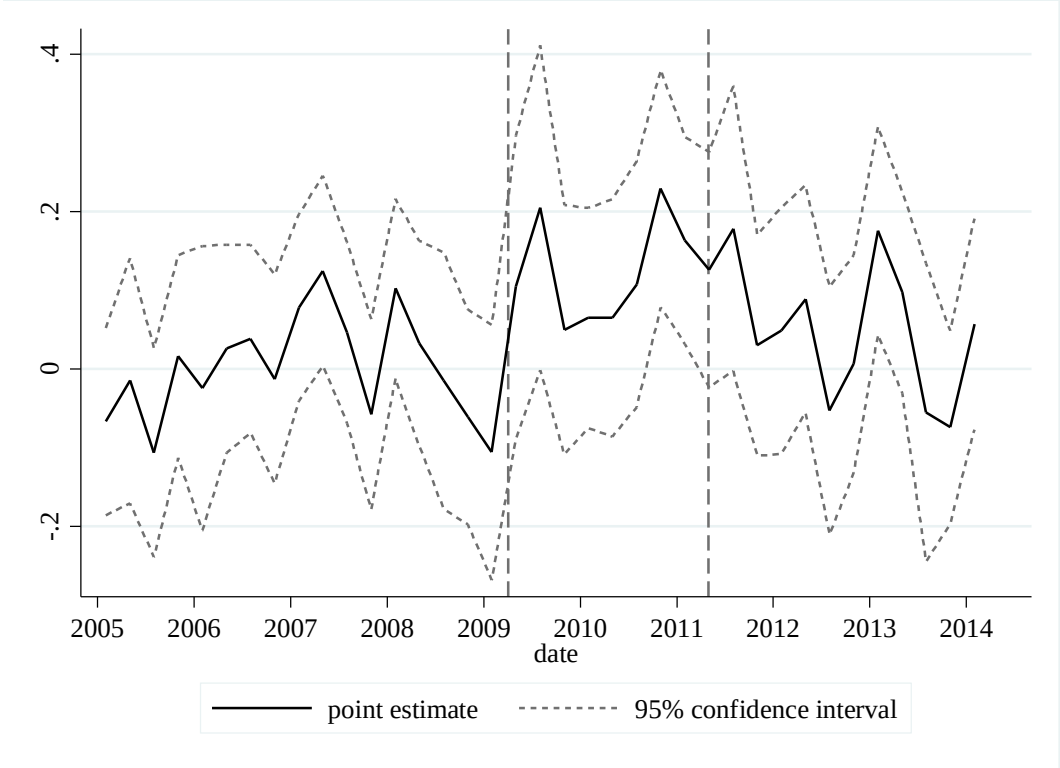
Panel B: Coefficients on *post*small firm*



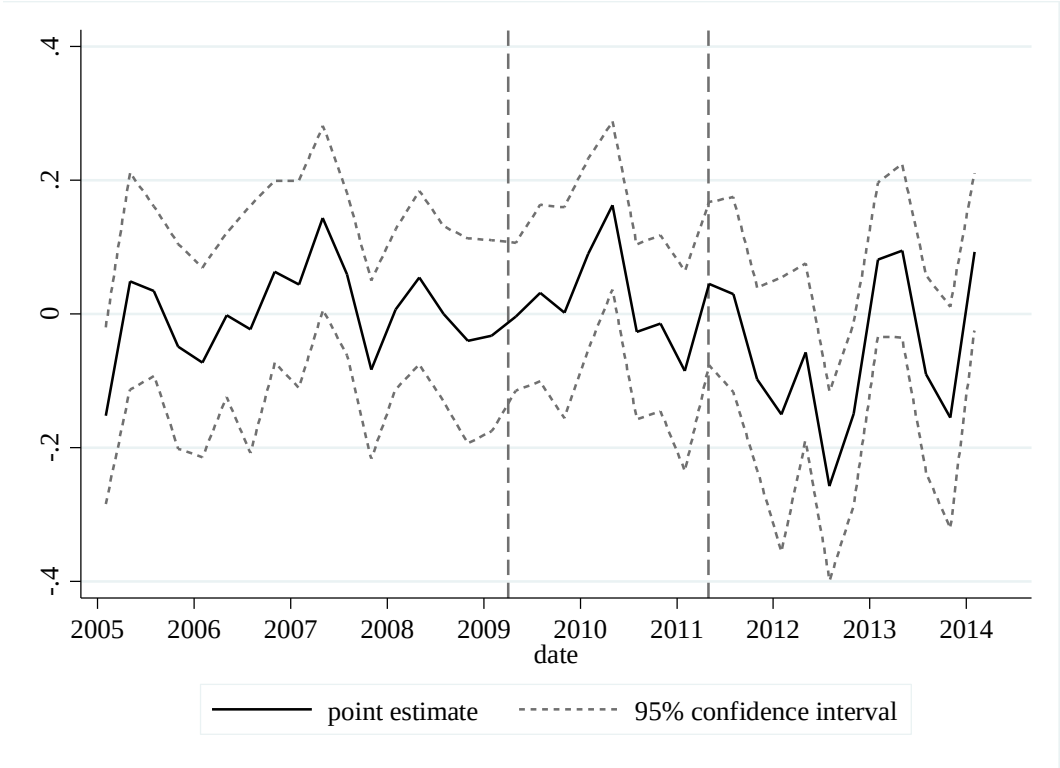
Notes: Coefficients and 95% confidence intervals are presented from specifications that replicate column (2) of Table 3, run separately by industry. Industry abbreviations are given in Appendix Table 1.

Appendix Figure 6: Time-varying estimate of the policy effect on number of new hires, high-use and low-use industries

Panel A: High-use industries construction and wholesale trade



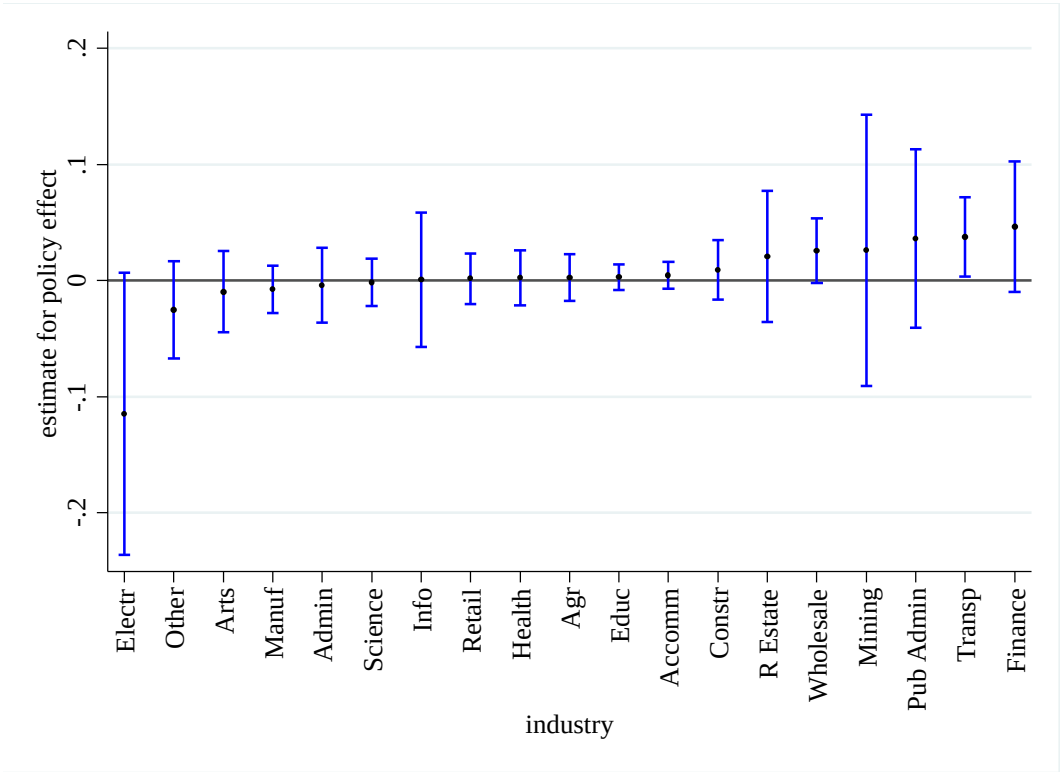
Panel B: Low-use industries education and training



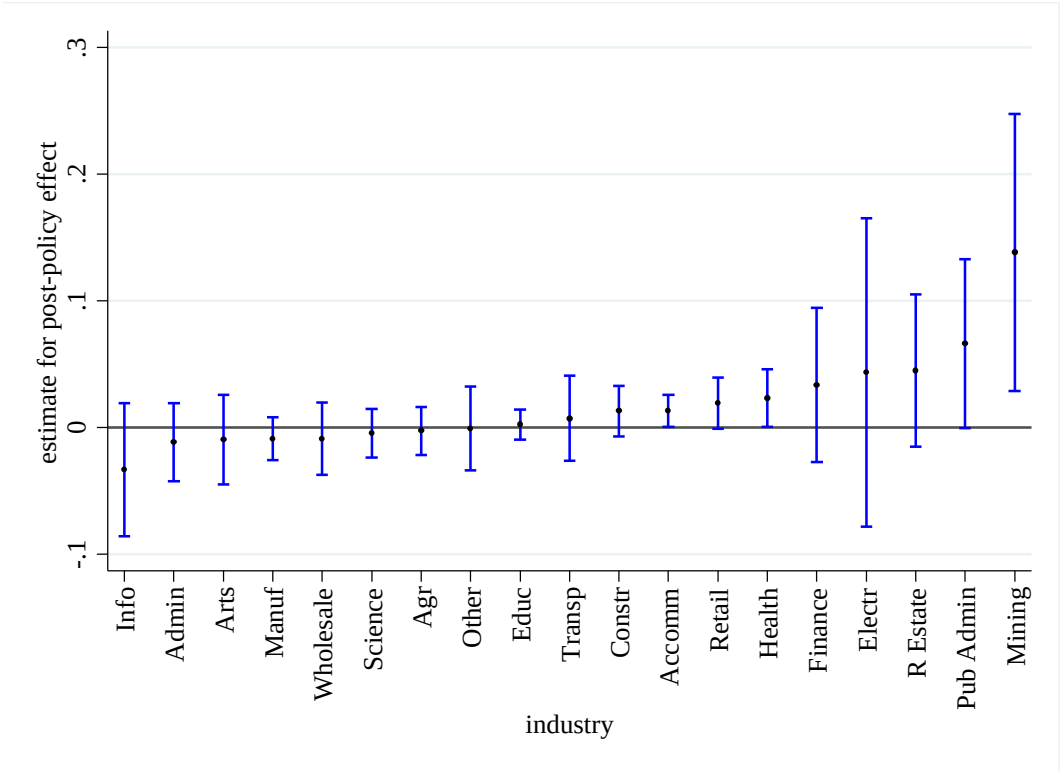
Notes: Replications of Figure 6, run separately for high-use and low-use industries.

Appendix Figure 7: Policy effect on the probability a new hire was a disadvantaged jobseeker by industry

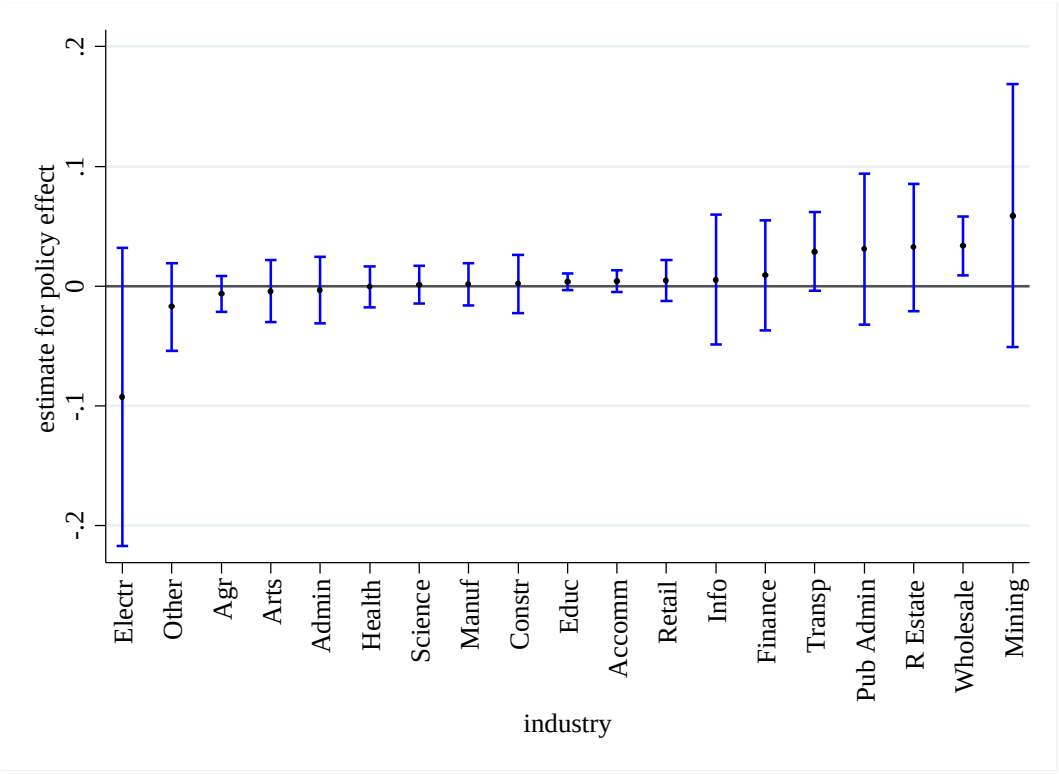
Panel A1: Beneficiary, coefficient on *between*small*



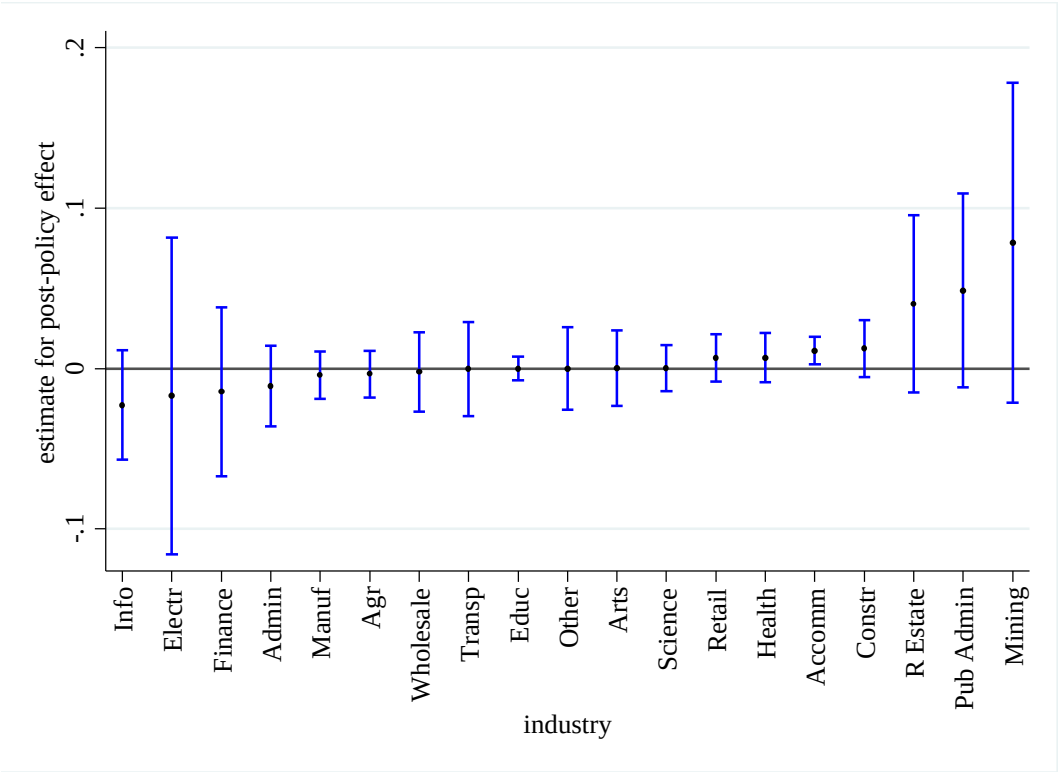
Panel A2: Beneficiary, coefficient on *post*small*



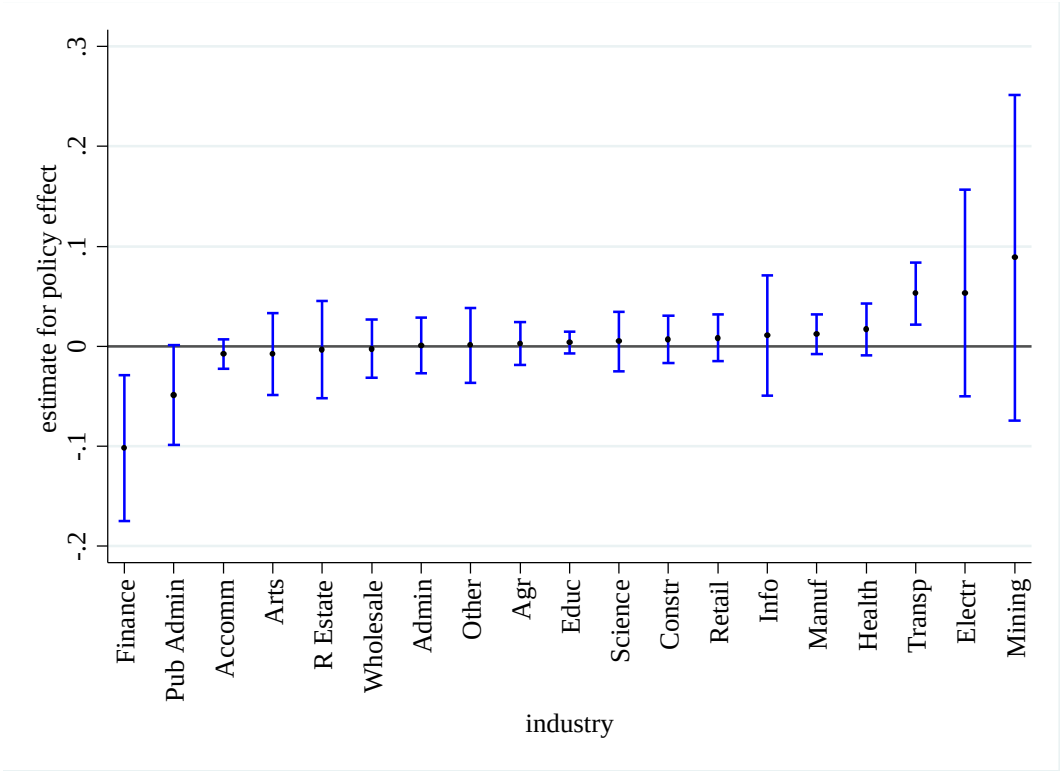
Panel B1: Jobseeker beneficiary, coefficient on *between*small*



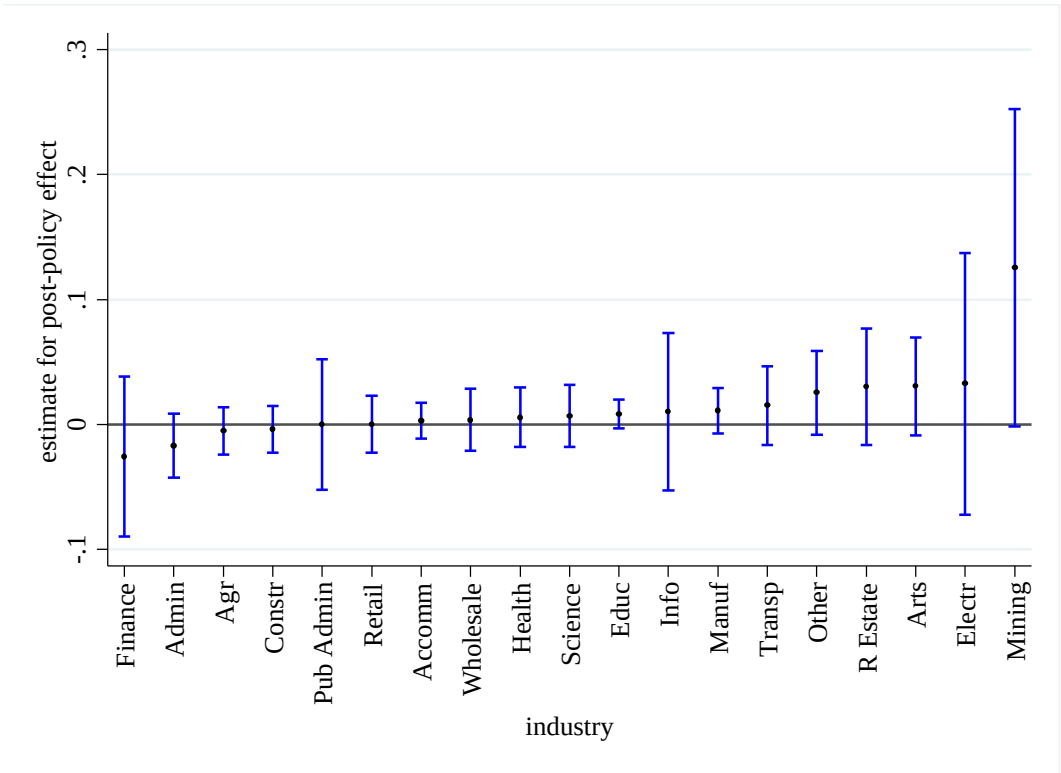
Panel B2: Jobseeker beneficiary, coefficient on *post*small*



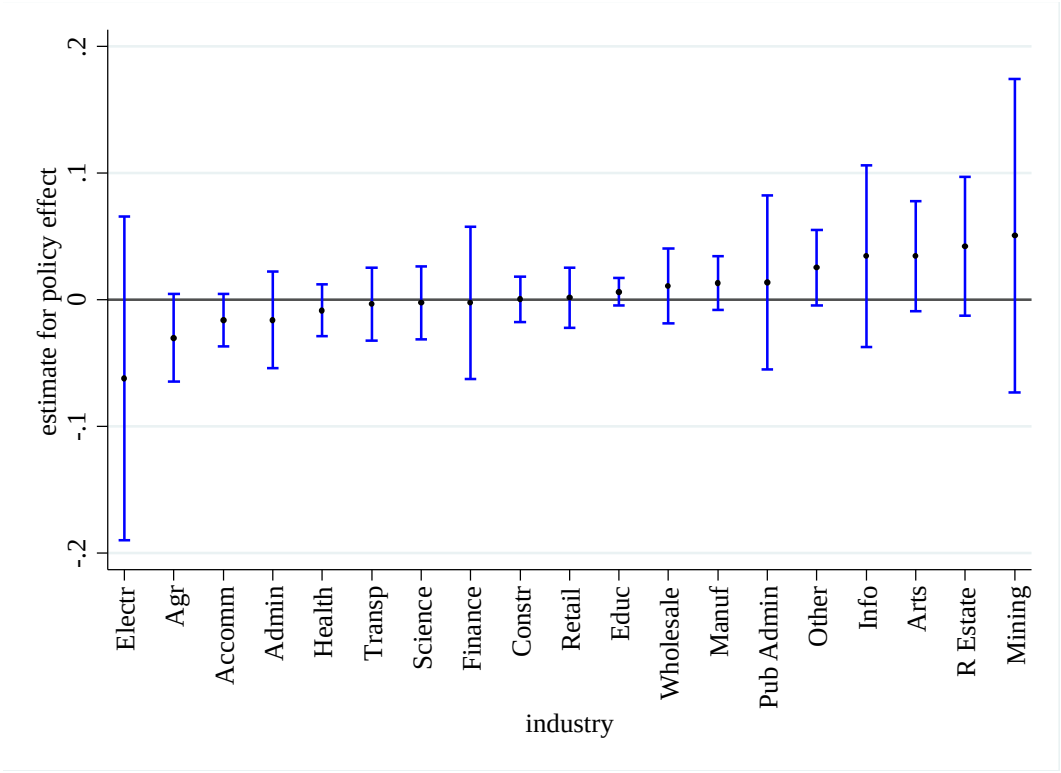
Panel C1: Non-worker, coefficient on *between*small*



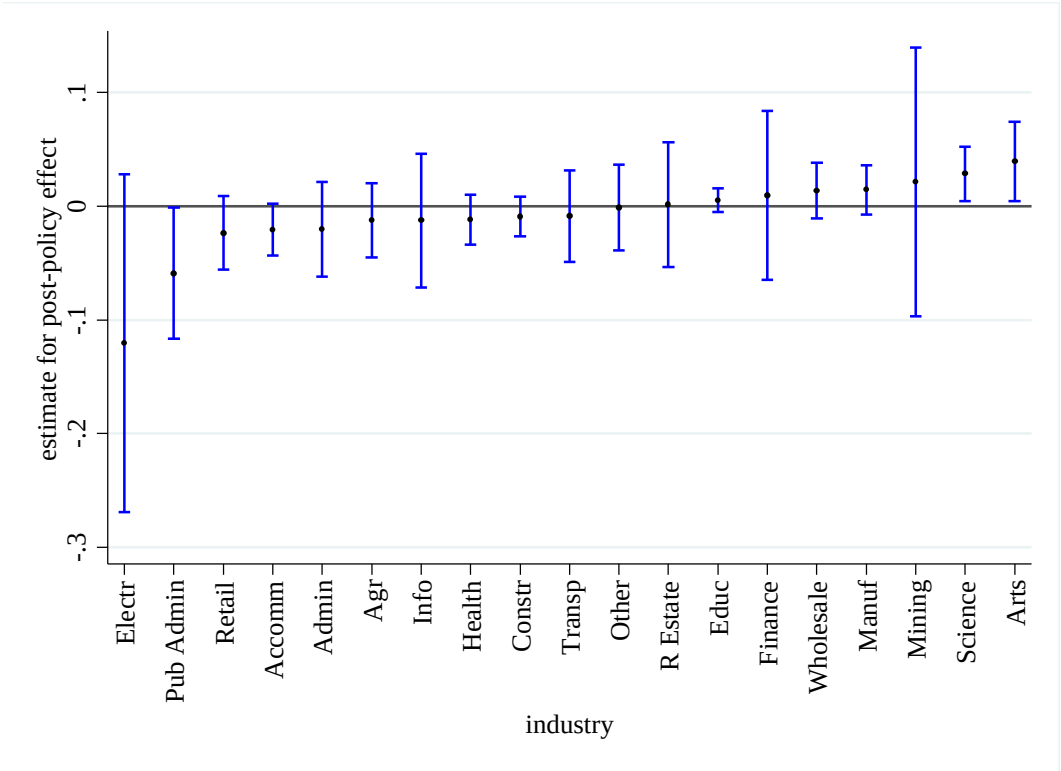
Panel C2: Non-worker, coefficient on *post*small*



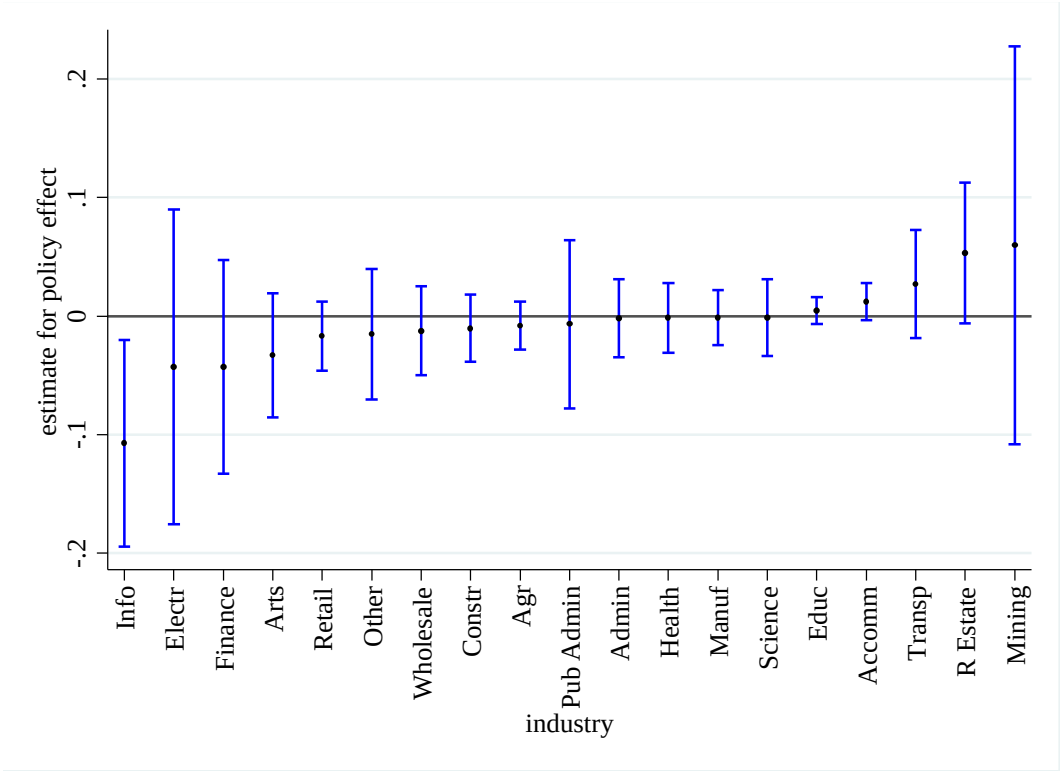
Panel D1: Migrant, coefficient on *between*small*



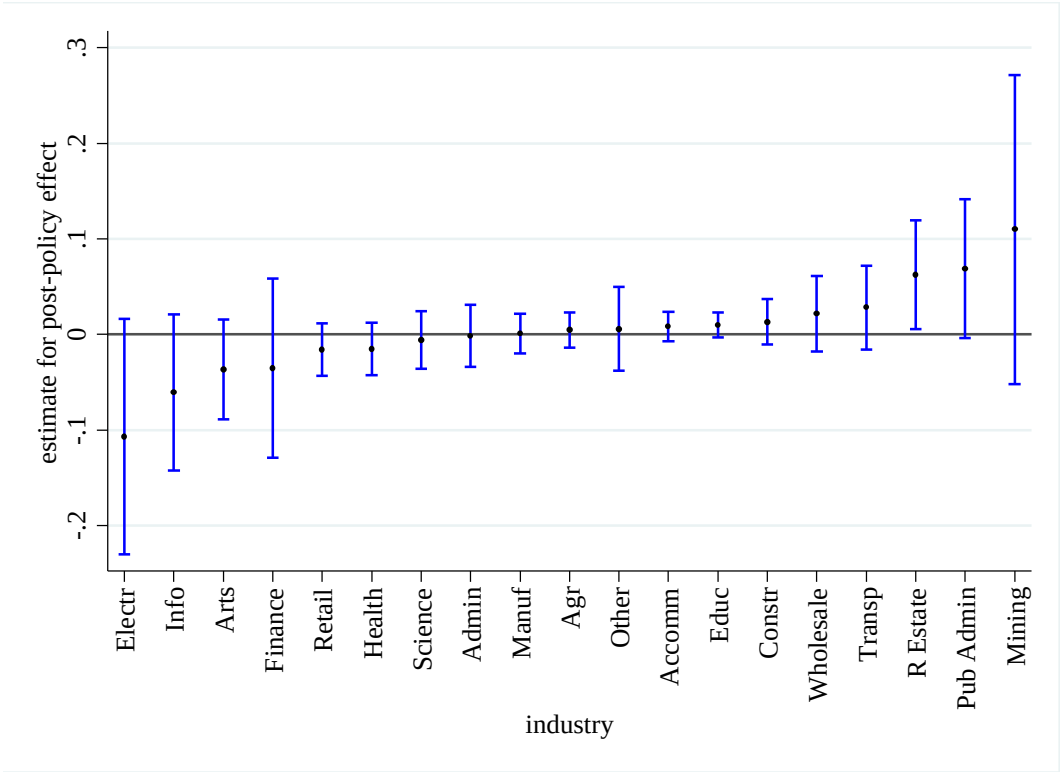
Panel D2: Migrant, coefficient on *post*small*



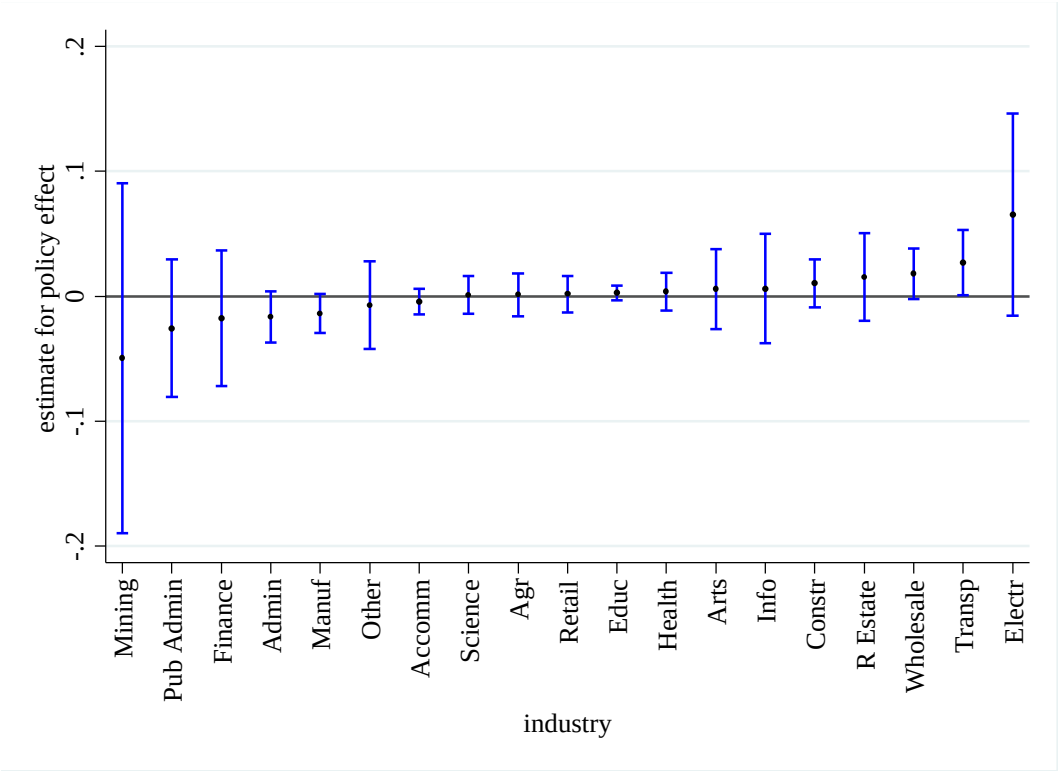
Panel E1: Under 25 years, coefficient on *between*small*



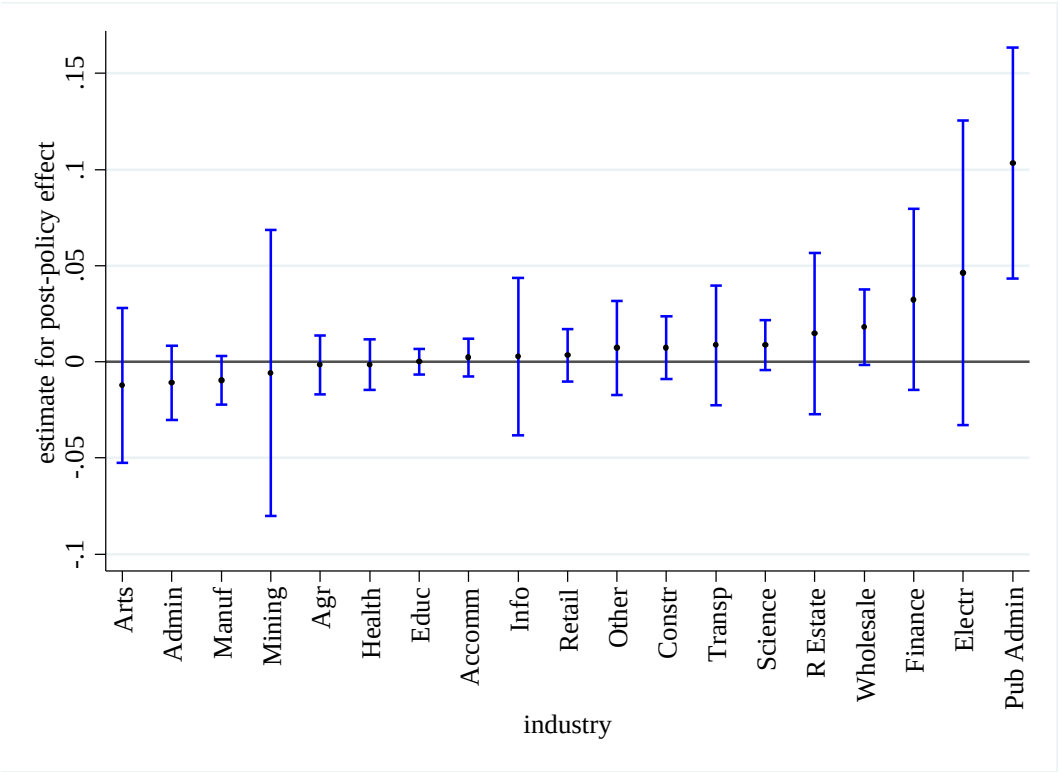
Panel E2: Under 25 years, coefficient on *post*small*



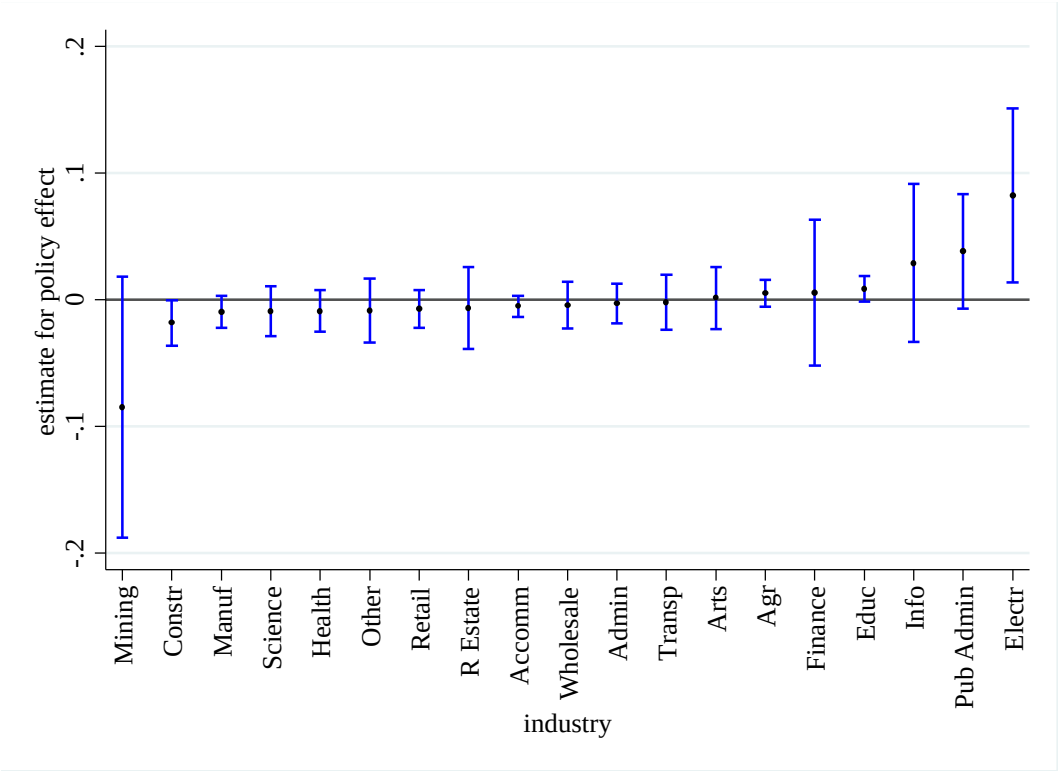
Panel F1: Māori or Pasifika under 25 years, coefficient on *between*small*



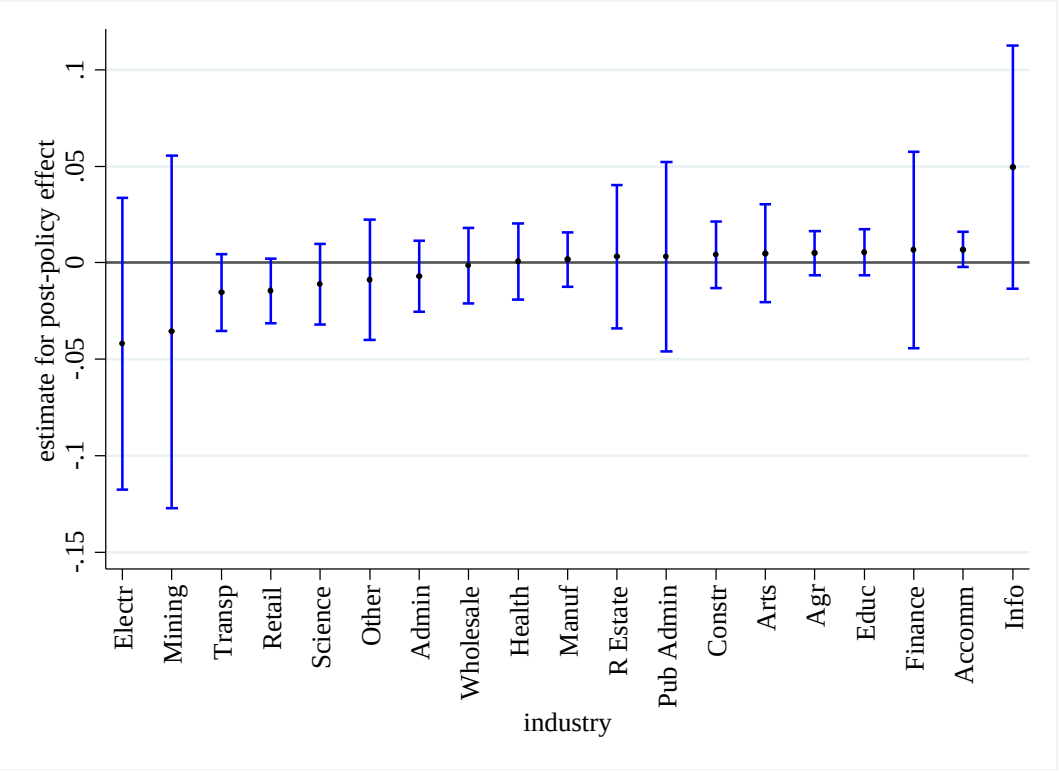
Panel F2: Māori or Pasifika under 25 years, coefficient on *post*small*



Panel G1: Education leaver, coefficient on *between*small*



Panel G2: Education leaver, coefficient on *post*small*



Notes: Panels present results of specifications that replicate columns of Table 6, run separately by industry. Coefficients and 95% confidence intervals are shown. Industries are given in Appendix Table 1.

Appendix Tables

Appendix Table 1: ANZSIC codes

Code	Industry description	Abbreviation
A	Agriculture, Forestry and Fishing	Agr
B	Mining	Mining
C	Manufacturing	Manuf
D	Electricity, Gas, Water and Waste Services	Electr
E	Construction	Constr
F	Wholesale Trade	Wholesale
G	Retail Trade	Retail
H	Accommodation and Food Services	Accomm
I	Transport, Postal and Warehousing	Transp
J	Information Media and Telecommunications	Info
K	Financial and Insurance Services	Finance
L	Rental, Hiring and Real Estate Services	R Estate
M	Professional, Scientific and Technical Services	Science
N	Administrative and Support Services	Admin
O	Public Administration and Safety	Pub Admin
P	Education and Training	Educ
Q	Health Care and Social Assistance	Health
R	Arts and Recreation Services	Arts
S	Other Services	Other

Notes: Codes and industry descriptions come from Statistics NZ. Abbreviations are the authors' own.

Appendix Table 2: 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	OLS regression	OLS regression
Dependent variable:	Number of new hires	Number of new hires	Number of new hires	Dummy for any new hires	Log(new hires+1)
Between * Small firm	**0.018**	**0.012**	**0.005**	**0.006**	**0.019*** **
	(0.015)	**(0.012)**	**(0.012)**	**(0.004)**	**(0.005)**
Post * Small firm	0.006	0.001	0.002	0.004	0.013***
	(0.014)	(0.011)	(0.012)	(0.004)	(0.005)
Small firm	-0.021*	0.001	-0.008	-0.002	-0.011***
	(0.012)	(0.009)	(0.010)	(0.003)	(0.004)
Firm size (ln)	0.952***	1.023***	0.941***	0.258***	0.359***
	(0.036)	(0.028)	(0.030)	(0.009)	(0.011)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	*781,725*	*827,883*	*810,189*	*835,362*	*835,362*
% of non-zero hires	*45.8%*	*46.1%*	*61.6%*	*46.6%*	*46.6%*

Notes: The first three 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 five years. Columns (4) and (5) change the estimation method to OLS, and change the dependent variable to a dummy variable for the firm hiring anyone in the month and the log of the number of new hires. The regressions replicate those in column (2) of Table 3 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 3: Policy effect on the total number of hires including rehires

Firm sample:	Sizes 15-24	Sizes 15-24	Sizes 15-24	Sizes 18-21	Sizes 10-50	Sizes 15-24, excl 19, 20	Single-plant, sizes 15-24	Sizes 15-24, plant-level
Dependent variable: Number of hires of any kind	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Between * Small firm	**-0.011**	**-0.003**	**-0.003**	**-0.004**	**0.003**	**0.007**	**-0.009**	**0.003**
	(0.016)	**(0.013)**	**(0.012)**	**(0.016)**	**(0.008)**	**(0.014)**	**(0.013)**	**(0.016)**
Post * Small firm	0.004	0.001	0.001	-0.006	0.008	0.006	-0.006	-0.003
	(0.015)	(0.012)	(0.012)	(0.015)	(0.009)	(0.014)	(0.013)	(0.015)
Small firm	-0.011	-0.010	-0.010	-0.014	-0.015*	-0.014	-0.005	-0.001
	(0.013)	(0.010)	(0.010)	(0.014)	(0.009)	(0.015)	(0.011)	(0.012)
Firm size (ln)	0.969***	0.930***	0.930***	0.781***	0.936***	0.930***	0.938***	0.711***
	(0.041)	(0.032)	(0.032)	(0.112)	(0.010)	(0.041)	(0.034)	(0.040)
Plant size (ln)								0.009***
								-0.001
Between	-0.167***							
	(0.013)							
Post	-0.151***							
	(0.013)							
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
Regional Council employment shares			Yes					
Observations	*835,362*	*835,362*	*835,362*	*314,637*	*2,495,838*	*678,978*	*709,782*	*1,049,721*
% of non-zero hires	*60.2%*	*60.2%*	*60.2%*	*61.5%*	*58.0%*	*59.8%*	*60.4%*	*50.8%*
Alpha	1.399***	0.896***	0.892***	0.874***	0.910***	0.892***	0.872***	1.769***
	(0.016)	(0.012)	(0.012)	(0.014)	(0.011)	(0.012)	(0.012)	(0.019)

Notes: This table presents the coefficients from negative binomial regressions at the firm-month level (or plant-month level in column (8)) where the dependent variable is the firm's total number of hires, including hires of employees who had previously worked for the firm. Otherwise, the regressions replicate those in Table 3. Standard errors, in parentheses, are robust and clustered at the firm level (columns (1) to (7)) or plant level (column 8). Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 4: Policy effect on the number of new hires in each quartile of the earnings distribution

Dependent variable: New hires that lasted 3+ months in:	Lowest quartile of the earnings distribution	Second quartile of the earnings distribution	Third quartile of the earnings distribution	Top quartile of the earnings distribution
Between * Small firm	**-0.005**	**0.002**	**0.011**	**0.017**
	(0.023)	**(0.023)**	**(0.021)**	**(0.024)**
Post * Small firm	0.034	-0.019	0.007	0.008
	(0.021)	(0.020)	(0.020)	(0.022)
Small firm	-0.030*	0.011	-0.010	-0.021
	(0.018)	(0.017)	(0.017)	(0.019)
Firm size (ln)	0.776***	0.950***	1.042***	1.109***
	(0.055)	(0.052)	(0.051)	(0.061)
Month-in-year fixed effects	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry FE	Yes	Yes	Yes	Yes
Observations	*835,362*	*835,362*	*835,362*	*835,362*
% of non-zero hires of given type	*11.5%*	*12.6%*	*13.1%*	*11.3%*

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 as described in each column header. New hires are those who had not worked for the firm in the previous five years. The sample period is January 2005 to March 2014, and firms included are those sizes 15 to 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 5: Policy effect on firm growth

Firm sample:	Sizes 15-24	Sizes 15-24	Sizes 15-24	Sizes 18-21	Sizes 10-50	Sizes 15-24, excl 19, 20	Single-plant, sizes 15-24
Dependent variable: % change in number of employees over the month	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Between * Small firm	**-0.599*** **	**-0.500*** **	**-0.500*** **	**-0.528*** **	**0.063***	**-0.408*** **	**-0.477*** **
	(0.053)	**(0.052)**	**(0.052)**	**(0.056)**	**(0.038)**	**(0.055)**	**(0.057)**
Post * Small firm	-0.449***	-0.445***	-0.444***	-0.473***	-0.258***	-0.325***	-0.458***
	(0.052)	(0.051)	(0.051)	(0.053)	(0.037)	(0.053)	(0.056)
Small firm	0.160**	0.479***	0.479***	-0.280***	-0.941***	-5.971***	0.502***
	(0.066)	(0.055)	(0.055)	(0.050)	(0.047)	(0.135)	(0.061)
Between	0.249***						
	(0.038)						
Post	0.303***						
	(0.037)						
5th-order polynomial in firm size	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-in-year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Regional Council employment shares			Yes				
Observations	713,139	713,139	713,139	222,096	2,325,210	502,050	601,782
% with some change in employment over the month	*56.8%*	*56.8%*	*56.8%*	*49.2%*	*58.4%*	*49.8%*	*57.1%*
R squared	*0.077*	*0.144*	*0.144*	*0.226*	*0.188*	*0.181*	*0.153*

Notes: This table presents the coefficients from OLS regressions at the firm-month level where the dependent variable is the firm's percentage change in number of employees over the month. The sample period is January 2005 to March 2014. 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 6: Policy effect on the probability a new hire was a disadvantaged jobseeker, probit regressions

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	Māori or Pasifika under 25	Education leaver
Between * Small firm	**0.003**	**0.003**	**0.003**	**-0.002**	**-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	0.004	0.001	0.002
	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
Small firm	-0.001	-0.000	-0.002	-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	0.008	0.005	-0.005	0.002
	(0.007)	(0.006)	(0.008)	(0.010)	(0.010)	(0.006)	(0.005)
Plant size (ln)	-0.002	0.000	0.004*	0.002	0.014***	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 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*892,362*	*891,414*	*892,890*	*891,186*	*890,013*	*886,557*	*769,368*
% hires of given type	*18.2%*	*11.5%*	*24.9%*	*18.2%*	*41.7%*	*9.7%*	*12.1%*
Pseudo R squared	*0.031*	*0.041*	*0.035*	*0.096*	*0.096*	*0.042*	*0.013*

Notes: This table presents the marginal effects from probit 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 table replicates Table 3, but uses probit instead of OLS. See the notes to Table 3 for further details. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 7: 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	Māori 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, excl. 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)
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 10-30, excl. 17-22							
Between * Small firm	**0.001**	**0.002**	**-0.004**	**0.002**	**-0.000**	**0.002**	**0.001**
	(0.003)	**(0.002)**	**(0.003)**	**(0.004)**	**(0.003)**	**(0.002)**	**(0.002)**
Post * Small firm	0.004	0.003	0.002	0.001	0.003	0.001	0.003
	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.002)	(0.002)
Small firm	-0.003	-0.001	-0.003	-0.001	0.005	-0.002	-0.001
	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)	(0.002)	(0.002)
Observations	*1,439,826*	*1,439,826*	*1,439,826*	*1,439,826*	*1,434,627*	*1,434,627*	*1,242,864*

Continued on following page

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	Māori or Pasifika under 25	Education leaver
Continued from previous page *Single plant firms sized 15-24*							
Between * Small firm	**0.005**	**0.003**	**0.002**	**-0.004**	**-0.002**	**0.000**	**-0.001**
	(0.003)	**(0.003)**	**(0.003)**	**(0.004)**	**(0.004)**	**(0.003)**	**(0.002)**
Post * Small firm	0.003	0.002	0.004	-0.002	0.004	-0.000	0.003
	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)	(0.002)	(0.002)
Small firm	-0.001	0.001	-0.003	-0.003	0.001	-0.002	0.000
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	*774,756*	*774,756*	*774,756*	*774,756*	*772,200*	*772,200*	*666,930*

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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 8: Policy effect on the probability any hire was a new disadvantaged jobseeker

Dependent variable: Indicator for hire being new to firm and of type:	Beneficiary in past year	Jobseeker beneficiary in past year	Not worked in past year	Recent migrant	Under 25 years old	Māori or Pasifika under 25	Education leaver
Between * Small firm	**0.003**	**0.003***	**0.004**	**-0.001**	**0.001**	**0.001**	**0.000**
	(0.002)	**(0.002)**	**(0.002)**	**(0.003)**	**(0.003)**	**(0.002)**	**(0.001)**
Post * Small firm	0.002	0.002	0.002	-0.002	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Small firm	-0.001	-0.000	-0.002	-0.002	0.002	-0.002	0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Firm size (ln)	-0.002	0.001	-0.001	0.006	0.003	-0.002	0.001
	(0.005)	(0.004)	(0.006)	(0.007)	(0.008)	(0.004)	(0.004)
Plant size (ln)	0.002	0.002	0.006***	0.002	0.012***	0.007***	-0.001
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Month-in-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month * 3-digit industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*1,390,431*	*1,390,431*	*1,390,431*	*1,390,431*	*1,387,491*	*1,387,491*	*1,267,557*
% hires of given type	*11.7%*	*7.4%*	*16.0%*	*11.7%*	*26.7%*	*6.2%*	*7.4%*
R squared	*0.021*	*0.021*	*0.025*	*0.062*	*0.081*	*0.018*	*0.013*

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 new to the firm and a particular type of disadvantaged jobseeker, as described in the column header. The specifications replicate Table 6, except non-new hires are also included as observations in the regressions, with dependent varable equal to 0. See the notes to Table 6 for further details. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 9: Policy effect on the probability a new hire was a disadvantaged jobseeker, by earnings quartile

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	Māori or Pasifika under 25	Education leaver
Lowest earnings quartile							
Between * Small firm	**0.004**	**0.001**	**0.016****	**-0.001**	**-0.001**	**-0.006**	**0.003**
	(0.006)	**(0.004)**	**(0.008)**	**(0.006)**	**(0.008)**	**(0.005)**	**(0.004)**
Post * Small firm	-0.001	-0.002	0.004	0.000	-0.001	0.002	0.008*
	(0.006)	(0.004)	(0.007)	(0.006)	(0.007)	(0.005)	(0.004)
Small firm	-0.007	-0.003	-0.009	0.002	0.006	0.000	-0.003
	(0.005)	(0.003)	(0.006)	(0.005)	(0.006)	(0.004)	(0.004)
Observations	*130,572*	*130,572*	*130,572*	*130,572*	*130,248*	*130,248*	*115,446*
Second earnings quartile							
Between * Small firm	**0.007**	**0.005**	**0.005**	**-0.002**	**0.003**	**0.002**	**-0.001**
	(0.007)	**(0.005)**	**(0.007)**	**(0.007)**	**(0.007)**	**(0.005)**	**(0.005)**
Post * Small firm	0.019***	0.012***	0.013*	0.000	-0.009	0.001	0.003
	(0.006)	(0.005)	(0.007)	(0.008)	(0.007)	(0.005)	(0.006)
Small firm	-0.007	-0.004	-0.004	-0.009	0.009	0.003	-0.003
	(0.005)	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Observations	*142,587*	*142,587*	*142,587*	*142,587*	*142,368*	*142,368*	*124,986*
Third earnings quartile							
Between * Small firm	**0.003**	**0.007**	**0.001**	**0.004**	**-0.003**	**0.002**	**-0.007**
	(0.006)	**(0.005)**	**(0.006)**	**(0.007)**	**(0.007)**	**(0.004)**	**(0.005)**
Post * Small firm	-0.001	-0.001	0.003	0.006	-0.006	0.001	-0.008
	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)	(0.004)	(0.006)
Small firm	0.007	0.002	0.010*	-0.009	0.002	-0.008**	0.007
	(0.005)	(0.004)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)
Observations	*142,560*	*142,560*	*142,560*	*142,560*	*142,407*	*142,407*	*124,722*
Highest earnings quartile							
Between * Small firm	**0.003**	**0.002**	**0.002**	**-0.002**	**-0.009***	**-0.001**	**-0.006**
	(0.004)	**(0.004)**	**(0.006)**	**(0.005)**	**(0.005)**	**(0.002)**	**(0.005)**
Post * Small firm	0.001	0.000	0.000	-0.002	-0.004	0.003	-0.002
	(0.004)	(0.003)	(0.006)	(0.005)	(0.005)	(0.002)	(0.005)
Small firm	0.004	0.006*	-0.008	0.001	0.002	-0.001	0.005
	(0.004)	(0.003)	(0.005)	(0.005)	(0.004)	(0.002)	(0.004)
Observations	*130,710*	*130,710*	*130,710*	*130,710*	*130,485*	*130,485*	*115,662*

Notes: The panels of this table present the main coefficients of interest 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 sample of hires is restricted to those that lasted at least 3 months, and that fell in the earnings quartile described in the panel header. The regressions are otherwise the same as those in Table 6. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 10: Policy effect on the probability a new hire in a high- and low-use industries 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	Māori or Pasifika under 25	Education leaver
High-use industries							
Between * Small firm	**0.015**	**0.015***	**0.002**	**0.005**	**-0.012**	**0.014***	**-0.012***
	(0.010)	**(0.009)**	**(0.010)**	**(0.008)**	**(0.012)**	**(0.007)**	**(0.007)**
Post * Small firm	0.004	0.007	-0.001	-0.000	0.016	0.012*	0.003
	(0.009)	(0.008)	(0.008)	(0.007)	(0.011)	(0.007)	(0.007)
Small firm	0.012*	0.011*	-0.005	-0.005	-0.011	-0.015**	-0.001
	(0.007)	(0.006)	(0.007)	(0.006)	(0.009)	(0.006)	(0.006)
Observations	*92,067*	*92,067*	*92,067*	*92,067*	*91,689*	*91,689*	*78,291*
Low-use industries							
Between * Small firm	**0.003**	**0.004**	**0.004**	**0.006**	**0.005**	**0.003**	**0.009***
	(0.006)	**(0.004)**	**(0.006)**	**(0.005)**	**(0.006)**	**(0.003)**	**(0.005)**
Post * Small firm	0.002	-0.000	0.008	0.005	0.010	-0.000	0.005
	(0.006)	(0.004)	(0.006)	(0.005)	(0.007)	(0.003)	(0.006)
Small firm	0.005	0.005	-0.002	-0.007*	-0.004	0.002	-0.009*
	(0.005)	(0.003)	(0.005)	(0.004)	(0.006)	(0.003)	(0.005)
Observations	*113,457*	*113,457*	*113,457*	*113,457*	*113,262*	*113,262*	*100,953*

Notes: The panels of this table present the main coefficients of interest 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 table replicates Table 6, except the sample is limited to hires in the high-use industries construction and wholesale trade in the first panel, and to hires in the low-use industries education and training in the second panel. Standard errors, in parentheses, are robust and clustered at the firm level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 11: Policy effect on the distribution of employment duration of re-hires

Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
Between * Small firm	**0.003**	**0.002**	**0.000**	**-0.002**	**-0.001**	**-0.001**	**-0.002**	**-0.002**
	(0.002)	**(0.002)**	**(0.003)**	**(0.003)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**
Post * Small firm	0.007**	0.006**	0.001	0.000	-0.003	-0.003	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Small firm	-0.001	-0.003	-0.003	-0.003	0.000	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Firm size (ln)	0.026***	0.014**	0.032***	0.029***	0.026***	0.025***	0.014***	0.015***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)	(0.005)	(0.005)
Plant size (ln)	-0.015***	-0.013***	-0.022***	-0.019***	-0.011***	-0.009***	-0.007***	-0.005***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age		0.013***		0.018***		0.015***		0.012***
		(0.000)		(0.000)		(0.000)		(0.000)
Age squared (/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)
Māori		-0.012***		-0.028***		-0.027***		-0.020***
		(0.001)		(0.002)		(0.001)		(0.001)

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Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
Coefficients continued from previous page								
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	Yes	Yes	Yes
Calendar month * 3-digit industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	*399,915*	*390,963*	*399,915*	*390,963*	*399,915*	*390,963*	*399,915*	*390,963*
% hires of given type	*61.8%*	*61.9%*	*34.9%*	*35.1%*	*15.1%*	*15.3%*	*8.2%*	*8.4%*
R squared	*0.103*	*0.121*	*0.155*	*0.181*	*0.056*	*0.077*	*0.044*	*0.061*

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 size 15 to 24 and the employee had not worked for the firm in the previous five years. The table replicates Table 7, 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 12: Policy effect on the distribution of employment duration in high- and low-use industries

Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
High-use industries								
Between * Small firm	**-0.002**	**-0.003**	**-0.017***	**-0.018***	**-0.011**	**-0.010**	**-0.014***	**-0.013**
	(0.007)	**(0.007)**	**(0.009)**	**(0.009)**	**(0.009)**	**(0.009)**	**(0.008)**	**(0.008)**
Post * Small firm	-0.033***	-0.026***	-0.048***	-0.040***	-0.044***	-0.036***	-0.033***	-0.027***
	(0.008)	(0.008)	(0.011)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
Small firm	-0.003	-0.005	0.001	-0.004	0.016**	0.011	0.006	0.003
	(0.006)	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)
Observations	*72,489*	*70,449*	*72,489*	*70,449*	*72,489*	*70,449*	*72,489*	*70,449*
Low-use industries								
Between * Small firm	**0.004**	**0.004**	**0.004**	**0.004**	**-0.004**	**-0.004**	**-0.011****	**-0.011****
	(0.006)	**(0.006)**	**(0.006)**	**(0.006)**	**(0.006)**	**(0.005)**	**(0.005)**	**(0.005)**
Post * Small firm	0.013*	0.012	-0.003	-0.003	-0.014*	-0.015**	-0.010	-0.010
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)
Small firm	-0.008	-0.008	-0.016***	-0.015**	-0.010*	-0.007	-0.005	-0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Observations	*96,330*	*95,067*	*96,330*	*95,067*	*96,330*	*95,067*	*96,330*	*95,067*

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. The table replicates Table 7, but includes hires at firms sized 15-24 in the high-use industries construction and wholesale trade only in the top panel, and in the low-use industries education and training only in the bottom panel. The second column in each pair contains additional controls, as in Table 7. Standard errors, in parentheses, are robust and clustered at the individual level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 13: Policy effect on the distribution of employment duration of different hire types

Panel A: Beneficiaries, jobseekers, non-workers and migrants								
Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
Recent-beneficiary hires								
Between * Small firm	**-0.002**	**-0.002**	**-0.002**	**-0.002**	**-0.001**	**-0.001**	**-0.001**	**-0.001**
	(0.005)	**(0.005)**	**(0.006)**	**(0.006)**	**(0.005)**	**(0.005)**	**(0.004)**	**(0.004)**
Post * Small firm	0.006	0.006	-0.009	-0.008	-0.013*	-0.012*	-0.006	-0.005
	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.005)	(0.005)
Small firm	-0.002	-0.002	0.004	0.004	0.006	0.005	0.002	0.002
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.003)	(0.003)
Observations	*129,540*	*129,507*	*129,540*	*129,507*	*129,540*	*129,507*	*129,540*	*129,507*
Recent-jobseeker hires								
Between * Small firm	**-0.001**	**-0.001**	**-0.003**	**-0.004**	**0.004**	**0.004**	**0.003**	**0.003**
	(0.007)	**(0.007)**	**(0.008)**	**(0.008)**	**(0.006)**	**(0.006)**	**(0.005)**	**(0.005)**
Post * Small firm	0.002	0.002	-0.014	-0.013	-0.011	-0.010	-0.008	-0.008
	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)	(0.006)	(0.006)
Small firm	0.006	0.005	0.009	0.009	0.006	0.006	0.007	0.007
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)
Observations	*80,904*	*80,904*	*80,904*	*80,904*	*80,904*	*80,904*	*80,904*	*80,904*
Recent non-working hires								
Between * Small firm	**0.006**	**-0.000**	**0.006**	**-0.003**	**-0.001**	**-0.002**	**-0.000**	**-0.002**
	(0.005)	**(0.005)**	**(0.005)**	**(0.006)**	**(0.005)**	**(0.005)**	**(0.004)**	**(0.004)**
Post * Small firm	0.010*	0.010	0.008	0.001	-0.004	-0.009	-0.002	-0.005
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.005)	(0.005)
Small firm	0.004	-0.002	-0.008*	-0.006	0.004	0.003	-0.000	0.000
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)
Observations	*174,486*	*147,909*	*174,486*	*147,909*	*174,486*	*147,909*	*174,486*	*147,909*
Recent-migrant hires								
Between * Small firm	**0.005**	**0.004**	**0.007**	**0.003**	**-0.006**	**-0.007**	**-0.002**	**-0.005**
	(0.006)	**(0.006)**	**(0.006)**	**(0.008)**	**(0.005)**	**(0.006)**	**(0.003)**	**(0.005)**
Post * Small firm	0.004	-0.002	-0.000	-0.015	-0.011*	-0.018**	-0.007	-0.013**
	(0.007)	(0.008)	(0.007)	(0.010)	(0.006)	(0.008)	(0.004)	(0.007)
Small firm	0.014***	0.002	0.004	0.008	0.011**	0.012**	0.007**	0.009**
	(0.005)	(0.006)	(0.005)	(0.007)	(0.004)	(0.006)	(0.003)	(0.005)
Observations	*126,027*	*84,780*	*126,027*	*84,780*	*126,027*	*84,780*	*126,027*	*84,780*

Panel B: Beneficiaries, jobseekers, non-workers and migrants

Dependent variable: Indicator for employment lasting at least:	2 months	2 months	5 months	5 months	12 months	12 months	24 months	24 months
Young (under 25 years) hires								
Between * Small firm	**0.007***	**0.007***	**-0.000**	**-0.001**	**-0.000**	**0.001**	**-0.002**	**-0.002**
	(0.004)	**(0.004)**	**(0.004)**	**(0.004)**	**(0.003)**	**(0.004)**	**(0.002)**	**(0.003)**
Post * Small firm	0.003	0.005	-0.005	-0.003	-0.005	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.003)	(0.004)
Small firm	-0.002	-0.005	-0.000	-0.001	-0.002	-0.003	0.001	0.000
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	*298,614*	*274,461*	*298,614*	*274,461*	*298,614*	*274,461*	*298,614*	*274,461*
Young Māori & Pasifika hires								
Between * Small firm	**0.007**	**0.007**	**-0.002**	**-0.003**	**0.000**	**-0.000**	**0.004**	**0.004**
	(0.008)	**(0.008)**	**(0.009)**	**(0.009)**	**(0.007)**	**(0.007)**	**(0.005)**	**(0.005)**
Post * Small firm	-0.005	-0.005	-0.015	-0.014	-0.010	-0.009	-0.009	-0.009
	(0.010)	(0.010)	(0.012)	(0.012)	(0.010)	(0.009)	(0.007)	(0.007)
Small firm	0.004	0.004	0.003	0.003	-0.006	-0.006	0.000	0.001
	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.006)	(0.004)	(0.004)
Observations	*70,128*	*70,128*	*70,128*	*70,128*	*70,128*	*70,128*	*70,128*	*70,128*
Recent education-leaver hires								
Between * Small firm	**0.003**	**0.004**	**-0.005**	**-0.004**	**0.003**	**0.004**	**-0.001**	**-0.001**
	(0.006)	**(0.006)**	**(0.008)**	**(0.008)**	**(0.007)**	**(0.007)**	**(0.006)**	**(0.006)**
Post * Small firm	0.002	0.003	-0.013	-0.013	-0.013	-0.013	-0.001	-0.001
	(0.008)	(0.008)	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)	(0.007)
Small firm	-0.007	-0.008	0.007	0.006	0.005	0.004	0.000	-0.001
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
Observations	*87,876*	*87,813*	*87,876*	*87,813*	*87,876*	*87,813*	*87,876*	*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. Even-numbered columns contain more extensive controls. The table replicates Table 7, but 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. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.10.

Appendix Table 14: Survey of Working Life (2012) trial period use by 1-digit industry

Code	Industry description	% of employees starting on trial period	Respondents in survey
A+B	Agriculture, Forestry, Fishing and Mining	36.7%	705
C+D	Manufacturing, Electricity, Gas, Water and Waste Services	35.4%	1,371
E	Construction	49.1%	1,047
F	Wholesale Trade	48.9%	537
G	Retail Trade	44.2%	1,932
H	Accommodation and Food Services	40.0%	1,575
I	Transport, Postal and Warehousing	35.9%	567
J	Information Media and Telecommunications	28.1%	222
K	Financial and Insurance Services	31.1%	405
L	Rental, Hiring and Real Estate Services	43.1%	201
M	Professional, Scientific and Technical Services	38.5%	1,140
N	Administrative and Support Services	37.0%	528
O	Public Administration and Safety	10.7%	693
P	Education and Training	13.8%	1,104
Q	Health Care and Social Assistance	33.1%	1,395
R	Arts and Recreation Services	30.6%	384
S	Other Services	41.6%	489
	Total	35.8%	14,295

Notes: Codes and industry descriptions come from Statistics NZ. The survey gives data on the total number of respondents, and the estimated number of employees in each industry in the whole economy. Figures in the 'Respondents in survey' column are constructed from these numbers, by the authors, and have been randomly rounded to base 3.