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ORIGINAL ARTICLE

The impact of agricultural minimum wages on worker flows in South Africa

Marlies Piek[1,2] | Dieter von Fintel[3,4] | Johann Kirsten[5]

[1]United Nations University World Institute for Development Economics Research (UNU-WIDER), Helsinki, Finland

[2]Research on Socio-Economic Policy (RESEP), Department of Economics, Stellenbosch University, Stellenbosch, South Africa

[3]Institute of Labor Economics (IZA), Bonn, Germany

[4]Department of Economics, Stellenbosch Univeristy, Stellenbosch, South Africa

[5]Bureau for Economic Research (BER), Stellenbosch University, Stellenbosch, South Africa

Correspondence
Marlies Piek, United Nations University World Institute for Development Economics Research (UNU-WIDER), Helsinki, Finland.
Email: piek@wider.unu.edu and marliesr@sun.ac.za

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Abstract
This paper is the first to provide estimates of how minimum wages affect worker flows and employment growth rates in an employment scarce developing country context. We investigate the effects of a large, exogenous increase in agricultural minimum wages in South Africa. We find that changes occurred primarily among non-seasonal workers. Non-seasonal agricultural employment growth decreased in the initial periods after the minimum wage hike. This was mainly driven by slower rates of entry. The effect on the rate of entry decreases over time. While farms also responded by shedding non-seasonal workers at higher rates, this negative effect was limited to 1 year directly after the minimum wage hike. Employment growth recovers 4 years after the policy shock, indicating that firms adjusted relatively quickly despite the large legislated minimum wage increase. Seasonal employment growth and rates of entry and exit of seasonal workers were for the most part unaffected. Descriptive statistics, however, suggest a slight compositional change among seasonal workers: Farms replaced the worst paid seasonal workers with other low-income workers who were slightly better paid and presumably more productive.

KEYWORDS
agriculture, difference-in-differences, employment effects, hires, labour market dynamics, minimum wage, separations, worker flows

JEL CLASSIFICATION
C21, C23, J23, J38

1 | INTRODUCTION

Despite the study of minimum wages spanning many decades, the effects of wage legislation on labour market outcomes are not yet fully understood. Most existing studies focus on measuring changes in net employment,[1] while the impact on worker flows—separations and hires—is rarely investigated (Brochu & Green, 2013; Dube et al., 2016; Kabátek, 2021; Portugal & Cardoso, 2006). However, analysis of worker flows is central to understanding compositional changes in a sector. Which workers lose their jobs, by whom are they replaced and does this have distributional implications? To our knowledge, this paper is the first to quantify the effects of minimum wages on worker flows in a developing country context where, even in the absence of minimum wages, formal job opportunities are limited and the labour market is characterised by barriers to entering employment (Banerjee et al., 2008; Burger & Fourie, 2019; Kingdon & Knight, 2004). We quantify the effect of minimum wage legislation on agricultural worker flows and employment using rich anonymised administrative payroll tax data from South Africa. We focus on understanding how separations, hires and employment growth rates evolve for 4 years following changes in minimum wage legislation. Meer and West (2016) argued that employment losses may not occur discretely at the time when minimum wage legislation changes. Rather, the impacts manifest in the long run and work through permanently lower hiring and entry rates, which in turn contribute to reductions in future job growth. Our analysis therefore assesses how entry and exit rates respond after the minimum wage increase and how they contribute to changes in agricultural employment growth; assessing these effects a few years after a minimum wage shock puts into focus whether the impacts are temporary or permanent. Unlike Meer and West (2016), we conclude that the employment growth trajectory is only temporarily affected by large changes in minimum wages.

We use difference-in-difference models to estimate the effects on employment growth, rates of entry and exit resulting from the 50% in agricultural minimum wages in South Africa in 2013. We find that changes occurred primarily among non-seasonal workers. Non-seasonal agricultural employment growth decreased, and this was mainly driven by slower rates of entry. While farms also responded by shedding non-seasonal workers at higher rates, this change was limited to 1 year directly after the minimum wage hike, after which exit rates rapidly returned to their normal trajectory. Employment growth and the rate of entries recover (fully or partially, depending on the specification) 4 years after the policy shock, indicating that firms adjusted relatively quickly despite the large legislated minimum wage increase. Seasonal workers on the other hand experienced far fewer adjustments. Results on employment growth, rates of entry and exit are for the most part statistically insignificant.

This paper relates to five distinct literatures. First, only a handful of studies have investigated the effect of minimum wage legislation on worker flows, while none have focused on developing countries in this regard. This line of work has several implications for a richer understanding of the impacts of minimum wages. Even if employment levels do not change, worker flows may change the composition of the workforce. By inference, minimum wage hikes could create both winners and losers, two opposing changes that are masked by zero impacts on aggregate employment. For example, Portugal and Cardoso (2006) analysed the effect of increased minimum wages on youth hiring and separations in Portugal and found that both fell after the wage increase.[2,3] They found no effects of minimum wages on total employment. However, 'insiders' who retained their jobs received higher wages, were at lower risk of separation and held on to their jobs for longer. This, however, came at the expense of fewer new hires and greater exclusion of 'outsiders'. Separations and hires can, however, be co-determined: If separation rates decline, fewer hires will follow to replace workers. Findings from two further North American studies (Brochu & Green, 2013; Dube et al., 2016) corroborate those of Portugal and Cardoso (2006) in this

[1]See Belman and Wolfson (2014), Doucouliagos and Stanley (2009), Neumark et al. (2014a, 2014b) and Neumark (2018) for reviews of the mixed evidence on the effects of minimum wages on employment in developed countries. While there is also mixed evidence of the employment effects in developing countries, negative (and larger) employment effects are more evident when the following applies: The minimum wage is binding, minimum wage enforcement is stronger and the studies focus on vulnerable (lower wage) workers in the formal sector (Corella, 2019; Neumark & Corella, 2021). Research by Bhorat et al. (2017) corroborates the finding that noncompliance may be the reason for the relatively small employment effects found in studies focusing on developing countries.

[2]A note on separations and hires: Both can be influenced by workers and employers. Separations can be driven by voluntary quits or layoffs. Hires can be affected by firms' willingness to create new vacancies or by more/fewer individuals looking for employment. Unfortunately, most studies cannot distinguish between these with the available data. However, considering the specific context, it often becomes clear which mechanism was at play.

[3]The authors argue that the decrease in separations was driven by fewer voluntary quits, as employees wanted to keep their higher paying jobs following the minimum wage hike. According to the authors, this decrease in separations led to lower job hires.

regard. However, unlike Portugal and Cardoso (2006), Brochu and Green (2013) found that the lower separation rates were driven by a decrease in layoffs rather than quits. Lower separation rates are consistent with the predictions of the Mortensen–Pissarides model (Pissarides, 2000), which posits that firms' expected profits decrease if they lay off current employees and hire new ones as the minimum wage rises. In contrast to existing studies, we find that the rate of separations initially increased in response to minimum wage increases. Our findings therefore do not align with theoretical priors adopted by the developed country literature. We engage with a second strand of literature that considers context-specific reasons for the divergent findings. High barriers to labour market entry and unemployment rates in developing countries are important contextual factors that determine how minimum wages affect worker flows. In the rural, agricultural context we study, job opportunities are few, unemployment is high and the costs of firing and re-hiring unskilled workers are low. Firms are therefore in a position to respond to labour market shocks by laying off workers, with the knowledge that they could easily fill these vacancies at a later stage. Within this context, the increases in separations were plausibly firm led.

Third, this paper distinguishes differences by seasonal and non-seasonal workers. Findings from Matsuura et al. (2011) and Saha et al. (2013) have shown that stricter employment regulation incentivizes firms to hire fewer workers on secure, longer term contracts. Non-seasonal workers may be particularly affected by the increase in the legislated minimum wage in the context that we study. This is because non-seasonal (permanent) agricultural workers often reside on the farm and receive in-kind benefits (Visser & Ferrer, 2015). Losing a non-seasonal job therefore often implies losing accommodation and other in-kind benefits, which have significant negative impacts on their welfare. Our results show that non-seasonal workers were more affected compared to seasonal workers. The increased rate of job losses was, however, only evident in the year directly after the legislated minimum wage increase. Negative impacts on welfare (arising from job loss and loss of accommodation) were likely concentrated in 1 year and did not perpetuate in the medium and long run.

Fourth, firms re-evaluate their optimal input mix when faced with shocks. This paper discusses the possibility of substitution between different types of workers after minimum wages adjustments. Descriptive statistics on wages—a proxy for productivity—show that the gap between average wages of seasonal workers exiting and entering the agricultural sector increased after the minimum wage hike. This is consistent with the hypothesis that firms replaced less productive workers with slightly more productive low-income workers in response to the legislative change. The minimum wage hike therefore possibly enabled firms to be more efficient, but our evidence suggests that this could have occurred at the expense of the least productive seasonal workers. Within the context of high unemployment and few job opportunities in rural areas, the minimum wage increase may have priced this group of workers out of the market and increased their barriers to entering the job market. The minimum wage hike was beneficial to those who were employed at the higher wage, but it came at the expense of increasing barriers to entry into employment for the least productive seasonal workers.

Lastly, this paper distinguishes differences in the immediate and medium-term effects of minimum wages. Dube et al. (2016) showed that changes in separation and hiring rates occurred within three quarters after the minimum wage increase and remained at that level thereafter. In other words, minimum wage hikes introduced long-lasting effects beyond the short run. Harasztosi and Lindner (2019) found relatively small short-run disemployment effects in response to a large-scale minimum wage increase in Hungary and found that firms substituted labour with capital. Their firm-side analysis showed that altering the production function affected job creation over the long run. Similarly, Sorkin (2015) shows that even small, contemporaneous wage-employment elasticities can compound to large elasticities in the long run. In contrast, our results show that where firms adjusted employment, the effects were temporary and their magnitude became smaller over time.

The rest of the paper is structured as follows. Section 2 gives an overview of minimum wages in South Africa and the context surrounding the minimum wage hike and summarises the effects of agricultural minimum wages on employment levels in the country. Section 3 describes the dataset and methodological approach. Section 4 presents descriptive statistics on key variables. Section 5 presents the econometric results. Section 6 summarises the paper's findings and conclusions.

2 | BACKGROUND

Before a national minimum wage was implemented in South Africa in 2019, minimum wages were legislated for selected sectors only. Not all workers were covered by these provisions. This paper considers a substantial real increase of 50% in the agricultural minimum wage in March 2013, from R69 to R105 per day, as set out by sectoral determinations.[4] This significant increase followed widespread farmworker strikes at the end of 2012 that started in De Doorns, a farming town in the Western Cape. Farmworkers protested against poor working conditions and low pay and demanded a daily wage of R150.[5] Even though the final sectoral determination was 30% below workers' demands, the wage increase was unprecedented in the sector and therefore represents a significant shock to the agricultural labour market.

Even though no research has analysed worker flows following minimum wage changes in South Africa, literature on employment level effects is extensive. Agricultural minimum wages were introduced in 2003, leading to substantial job losses (Bhorat et al., 2014). The unpublished work of Garbers et al. (2015) is the only study that investigates labour–labour and labour–capital substitution resulting from minimum wages in South African agriculture. The authors showed that *skilled* farmworker employment and farming capital increased after the introduction of agricultural minimum wages in 2003. However, they did not investigate how the minimum wage affected the composition of the agricultural workforce as it relates to seasonal versus non-seasonal workers. Van der Zee (2017) and Ranchhod and Bassier (2017) analysed the effect of the 50% increase in legislated agricultural minimum wages in 2013. While van der Zee (2017) found negative employment effects, Ranchhod and Bassier (2017) were sceptical of findings that employment changed in response to the minimum wage increase. However, both studies investigated the employment relationship only in the very short run (up to seven and three quarters after the wage hike respectively) and did not consider the effects on non-seasonal and seasonal workers separately. They also used household survey data instead of worker- and firm-level administrative records.

3 | DATA AND METHODOLOGICAL APPROACH

3.1 | Data

We use rich anonymised administrative tax data from South Africa spanning from the 2010/2011 to the 2016/2017 tax years. In particular, the data come from employee IRP5 (or IT3a) tax certificates that employers submit to the South African Revenue Service (SARS) on behalf of each employee once a year (National Treasury & UNU-WIDER, 2019).[6] These certificates cover the entire population of formally employed individuals and can be used to track individuals longitudinally by means of an anonymised person identifier. It is possible to observe the same workers before and after the legislated agricultural minimum wage increase in March 2013 and to determine which sector they work in.[7] Workers who did not appear in the data in a particular year were unemployed or working in the informal sector.

The tax certificates provide information such as job duration, the amount of income received, the source of income, a firm identifier and demographic characteristics such as age and gender. The job duration information is key to this analysis for two reasons. First, this information is used to classify workers as seasonal or non-seasonal workers. We define seasonal workers as those employees who work up to 6 months for a given farmer in one tax year, while non-seasonal workers are defined as those who work

[4]The value of R105 per day is for a full day's work. This equates to R2,274 per month in 2013 prices.

[5]See Ledger (2016) for an in-depth overview of the farmworker strikes preceding the minimum wage increase.

[6]See Kerr (2018) and Pieterse et al. (2018) for a detailed description of the dataset.

[7]By law, firms must issue IRP5 or IT3a certificates for employees who earn more than R2,000 per tax year. IRP5 certificates are issued for employees for whom tax has been deducted, while IT3a certificates are issued for employees whose earnings fall below tax-paying threshold. Tax certificates are not issued by informal sector employers, so that these workers are excluded from our analysis. In South Africa, the tax year runs from 1 March until the end of February of the following year. The policy change we study therefore coincides with the start of the 2013/2014 tax year.

more than 6 months a year.[8] In the sample used in this paper, roughly half of the agricultural workers are classified as seasonal and the other half as non-seasonal. These proportions agree with the distribution of seasonal and non-seasonal farmworkers documented in the Quarterly Labour Force Survey contained within the Post-Apartheid Labour Market Series (PALMS) (Kerr et al., 2019).

Second, job duration is an important input for identifying a relevant sample of low income individuals. The relevant policy change affected farmworkers, who count among the lowest paid workers in the agricultural sector. While the data include industry codes that allow us to identify agricultural workers, occupation codes are not recorded on tax certificates. In other words, the tax certificates do not distinguish between workers with different functions and skill levels within the agricultural sector, and we cannot establish who is a 'farmworker'. In the absence of occupation data, we therefore limit our sample to low-income agricultural workers. We used the job duration variable to calculate workers' monthly earnings. We then consulted the Post-Apartheid Labour Market Series (PALMS) dataset (Kerr et al., 2019) to find an appropriate monthly earnings threshold below which workers can be classified as 'unskilled farmworkers'. This cut-off was set at R5,400 per month (in December 2016 prices), as discussed in detail in Appendix B. We restrict our sample to workers who earned below this threshold in all waves of the panel. This approach circumvents selective attrition, which could arise if some workers were to attrite from the sample in later waves only because their earnings grow above the threshold. These workers would incorrectly be classified as unemployed and retaining them in the analysis would overstate exit rates. Our sample therefore excludes workers who are upwardly mobile and our findings are therefore only representative of workers who are constrained to working in low-paid jobs.

Indicators of job entry and exit are constructed by rectangularising the dataset into a balanced panel of individuals. States of employment are imputed to each individual between observed job spells. This allows us to determine whether an individual entered or exited the agricultural sector. Entrants into agriculture were either unemployed in the previous year or were employed in a non-agricultural sector. A worker who exits, on the other hand, transitions from being employed in agriculture to being unemployed or finding employment in another sector.[9]

The data also include the unique identifier of the firm the individual is working for. This allows us to aggregate the individual-level data into a firm-level panel dataset that captures the number of employees, entrants and exits. The analysis is performed at the firm level, enabling the study of both entry and exit of workers. A firm-level treatment variable is used and is described in Section 3.2 below.

3.2 | Methodological approach

The effects of the minimum wage hike are estimated by comparing the evolution of the outcome variables—employment growth, entry rates and exit rates—in firms that employ a high proportion of workers affected by the minimum wage increase to firms with a low proportion of affected workers, similar to Harasztosi and Lindner (2019) and Bossler and Gerner (2020). An 'affected' worker is paid below the new minimum wage threshold before it is implemented. Because of non-compliance, not all affected workers are paid according to the legislated minimum wage after it is implemented. Our estimates are therefore 'intention-to-treat' estimates, an empirical reality that applies to most quantitative studies of the minimum wage. Our estimates are likely to be smaller than if we had used actual treatment.

Our firm-level employment and worker flow aggregates are count variables. We therefore estimate all specifications using count data regressions that account for the discrete nature of the dependent variable (Cameron & Trevedi, 2005). Count models specify the conditional mean of the outcome as

[8]While some workers who become unemployed for a spell may be incorrectly classified as seasonal workers, Table A1 shows that on average, seasonal workers hold more jobs in a given tax year than non-seasonal workers and that this number is close to two. Most of our seasonal workers are therefore likely rotating between jobs and not moving into unemployment. This provides us with confidence that, for the most part, our definition accurately distinguishes between seasonal and non-seasonal workers.

[9]This analysis therefore focuses on the potential of *agriculture* to create jobs and does not consider the aggregate effects of minimum wages on workers who leave agriculture for jobs created in other parts of the economy.

$E(y|x) = e^{x\beta}$. Choosing this functional form ensures that the conditional mean is non-negative and interpretation is comparable to a semi-log Ordinary Least Squares (OLS) specification ($log(y) = x\beta + u$). Coefficients from count models can be interpreted as semi-elasticities, in much the same way as they are using semi-log linear models. However, Mullahy and Norton (2022) recently argued that count models are preferred to semi-log OLS models. The benefit of using non-linear count models is that they naturally incorporate zero counts in the estimation. These zeroes are important in our sample. About one fifth of farms did not hire any new entrants, and a similar proportion of farms had no exiting workers. Unless alternatives to the log transformation are applied, these observations are omitted from semi-log OLS regressions. Researchers often add a 'small' value to zeroes before logging, but results are sensitive to this arbitrary approach (Bellemare & Wichman, 2020; Mullahy & Norton, 2022). Count models can be estimated without arbitrary data transformations or data loss and are used in this paper.

The two dominant count models are Poisson and negative binomial regressions. They assume that the variance depends on the mean so that $Var(y|x) = [1+\alpha]E(y|x)$. When the outcome distribution satisfies the assumption of equidispersion ($\alpha = 0$), a Poisson model is appropriate. Negative binomial models are more flexible and accommodate overdispersion ($\alpha > 0$), which commonly arises in count data with many zeroes. The Poisson is nested within the negative binomial model, and a likelihood ratio test of $H_0 : \alpha = 0$ can be used to assess, which is the more appropriate specification. We reject this hypothesis in nearly all our models, so that we only report negative binomial estimates.

Equation (1) shows the general difference-in-difference specification that we use:

$$y_{it} = e^{(x_{it}\beta + u_{it})} = exp(\alpha + \omega_t year_t + \theta\, FA_i + \delta_t\, year_t \times FA_i + \gamma' w_{it} + log(e_{it}) + u_{it}). \tag{1}$$

This is equivalent to

$$log(y_{it}) = x_{it}\beta + u_{it} = \alpha + \omega_t year_t + \theta\, FA_i + \delta_t\, year_t \times FA_i + \gamma' w_{it} + log(\mathrm{e}_{it}) + u_{it}, \tag{2}$$

where y_{it} is the outcome variable (employment, entry and exit) for firm i in period t. The *year* variable is a set of year dummy variables. The 2012/2013 tax year is used as the base category, allowing us to interpret impacts in relation to the period immediately before the policy was implemented. *FA* is the firm-level *fraction affected* variable, which measures the proportion of workers in firm i who earned less than the new minimum wage in the tax year before the legislative change.[10,11] w_{it} is a vector of control variables, including the province in which the farm is located, annual provincial rainfall, proportion of male workers on the farm and the proportion of workers on the farm that fall into various age categories.

Farms with initially higher total employment have the potential for larger absolute worker flows, even if they have similar proportional changes in employment compared to smaller farms. To take this into account, 'exposure' variables (e_{it})—farm-specific 'maxima' of the outcomes—with coefficients restricted to 1 are included in the specification. In effect, this transforms the model outcomes from counts to rates. Rearranging Equation (2) yields

$$log(y_{it}) - log\,(e_{it}) = log\left(\frac{y_{it}}{e_{it}}\right) = x_{it}\beta + u_{it}. \tag{3}$$

[10]This *fraction affected* is constant within a firm across time. Another typical measure of exposure is the 'wage gap' measure. See for instance Card and Krueger (1994) as well as Machin et al. (2003). We used the wage gap as a robustness check and obtained similar results to using the fraction affected.

[11]The data contain the number of reporting periods that workers were employed in a given tax year but provide no indication of the number of hours worked. It is therefore not possible to differentiate between full time and part time workers or to calculate hourly wages. This does not affect the measurement of our outcome variables but does introduce measurement error into our treatment variable. We are likely to overstate our treatment variable, the fraction of workers affected, because part-time workers may be paid below the monthly threshold. This is because they work few hours and not because they are paid a low hourly wage rate. Our estimates are therefore likely to be understated.

In our models of employment, e_{it} is farm i's total employment in the prior year. In effect, we model year-to-year employment growth. We also use lagged farm-level employment as the exposure for exit, so that we model the proportion of the prior year's workforce that no longer works on the farm in the current period. For entry, we use current employment as the exposure. We therefore model the proportion of the currently employed that are new to the firm. Using lagged employment as an exposure for exit and current employment for entry means that their effects are not perfectly additive in explaining changes in employment.

Coefficients on the difference-in-difference terms, δ_t, measure percentage changes in the outcome variables for fully affected firms (in which all workers were initially paid below the new minimum wage) relative to completely unaffected firms (in which all workers were already paid above the new legislated minimum wage before its implementation). The difference-in-difference models therefore measure the deceleration and acceleration of the outcomes relative to the base period. This approach allows us to discern fine impacts of the minimum wage legislation as we can assess whether job growth slows down significantly relative to the base period.

A separate difference-in-difference term is estimated for every tax year, allowing us to trace the impact dynamically after implementation. We also estimate the difference-in-difference terms for years prior to the implementation of the policy. Finding insignificant estimates in the pre-policy period supports the validity of the common trends assumption, which is a necessary condition for using the differences-in-differences specification. Standard errors were clustered on the firm level. In addition to unweighted estimates, we also show results weighted by firm size. Unweighted results do not distinguish by firm size. Weighting acknowledges that firm-level entry and exit rates in larger firms make a larger contribution to aggregate entry and exit rates. Differences between the two sets of results give insights into the role of firm size in driving the observed changes. The weighted regressions are our preferred results as they indicate the average effect across all firms.

By construction, the analysis includes only firms that had at least one agricultural employee in the 2012/2013 tax year. We therefore exclude farms that started operating after the policy was implemented. Firms that shut down after the policy shock, however, are included in the firm-level analysis until the firm dies. Our firm-level results are therefore limited to understanding the impacts of the policy on hiring and firing decisions on pre-existing farms but only until their death. It is not possible to study job creation from the birth of new farming operations, because none of their workers were affected by the policy change. The share of employment in new farms is small, and our sample captures most agricultural workers. Tables A2 and A3 provide statistics on number and proportion of farms entering and exiting and the number of employed on these farms by year.

4 | DESCRIPTIVE STATISTICS

Figure 1 traces dynamic changes to (log) employment, entry and exit levels of seasonal agricultural and non-seasonal agricultural workers between 2011/2012 and 2016/2017. We use the regression sample, but trends are similar in the full sample and are not shown. We start with a comparison of employment changes. Before the minimum wage increase, seasonal and non-seasonal employment followed an upward trajectory. Seasonal employment continued to grow over the post-implementation period, albeit at a lower annual rate than in the pre-implementation period. Non-seasonal employment, on the other hand, declined by roughly six percentage points between 2012/2013 and 2016/2017.

We now turn to changes in worker flows. The number of seasonal and non-seasonal entrants decreased significantly in 2013/2014 (the first tax year following the wage hike) and remained at these low levels throughout the sample period. This decrease is particularly large for non-seasonal workers. Unlike entry, the impact on exit was a once-off event. Exit spiked in 2013/2014 but stabilised to pre-policy levels in the following tax year.

The descriptive evidence suggests that the lower employment levels and slower employment growth on existing farms were mainly driven by declining entry of especially non-seasonal workers, rather than by a continued increase in worker exit. Garbers et al. (2015) show that farms decreased low-wage employment in response to the introduction of agricultural minimum wages in South Africa in 2003, but they acquired more capital and hired better-skilled workers. While our paper cannot investigate

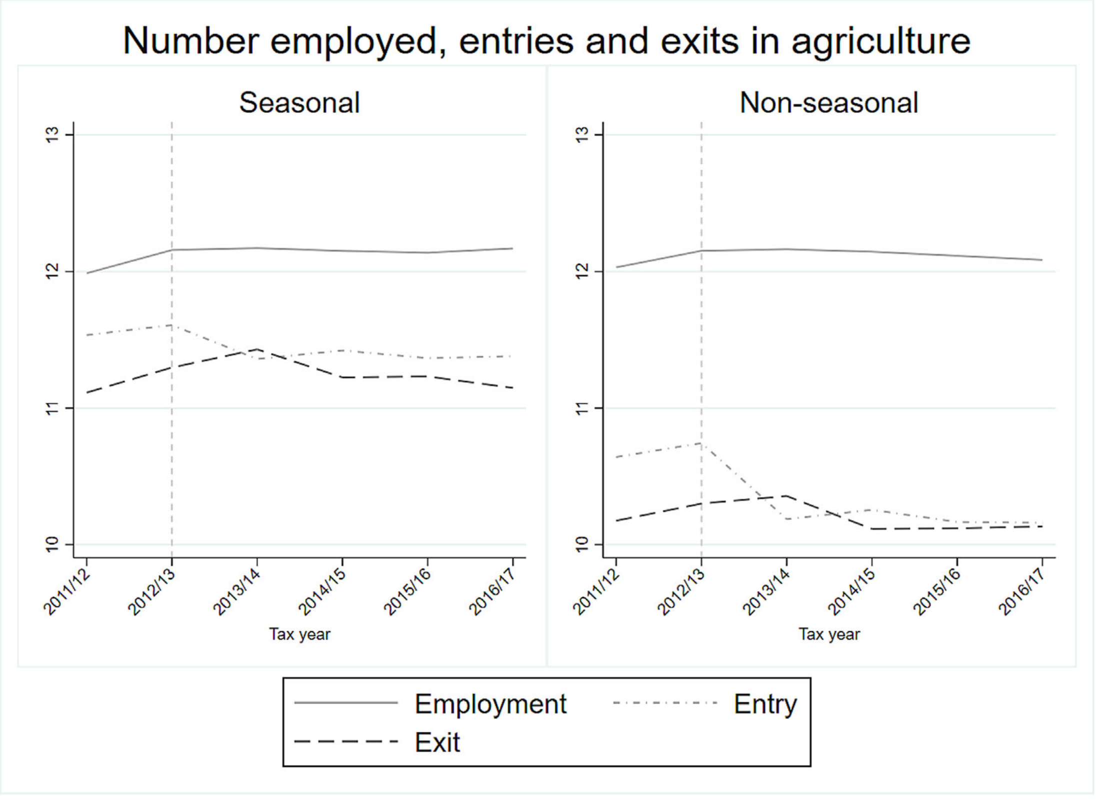

FIGURE 1 The number of employed, entries and exits in agriculture. *Note*: Seasonal workers are defined as those who work less than 6 months a year on a given farm, while non-seasonal workers work more than 6 months a year. Entrants are defined as individuals who worked in a sector they did not work in during the previous tax year. Exiting workers are defined as individuals who no longer worked in the same sector or who became unemployed in the following tax year. The sample is restricted to low-income individuals, defined as individuals who, throughout the period of analysis, consistently earned less than R5,400 per month in real terms. December 2016 is used as the base period to convert values into real terms. These figures are based on the econometric sample. Similar patterns are observed in the full sample (not shown). The vertical line indicates the period/tax year preceding the relevant minimum wage hike on 1 March 2013. Source: Own illustration using Version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019)

changes in capital, we briefly investigate whether farms responded by hiring better-skilled workers. Although our sample is restricted to low-income workers, we can nevertheless investigate whether farms hired slightly more productive workers to replace the worst paid workers.

Figure 2 plots real monthly wages—a proxy for worker productivity—of incumbents, entrants and those who exited the sector for low wage seasonal and non-seasonal workers.[12,13,14] Both seasonal and

[12]Wages of seasonal workers (incumbents, entrants and those who exit) are higher than those of the non-seasonal workers. This is likely driven by two factors: (a) non-seasonal workers possibly receive in-kind benefits (e.g. accommodation on the farm) that are not captured under reported wages, and (b) farmers often cover transportation costs for workers who do not reside on the farm. This practice is more common among seasonal workers (Visser & Ferrer, 2015).

[13]Although not the focus of this paper, Figure 2 shows that wage levels increased substantially for all types of workers (incumbents, entrants and those who exited) following the hike. This implies that the higher legislated minimum wage was effective in raising the average wage for low-wage agricultural workers even if compliance was imperfect (as seen by the average wages of entrants and exiters that are below the legislated minimum wage).

[14]While wages can stand proxy for worker productivity, minimum wage legislation can weaken the link between pay and productivity. This possibility is clearly observed when *all* groups' wages increase sharply after the minimum wage was adjusted. However, we do not infer that worker productivity increased sharply and by a similar magnitude at that time. The figures we present therefore do not accurately track absolute productivity levels within particular groups over time. However, they do provide indications of the evolution of *relative* cross-sectional productivity differences across groups of workers, each of which was affected by the same minimum wage adjustment. We limit our interpretation to the analysis of level differences of average wages between types of workers at specific points in time.

non-seasonal incumbents' real mean monthly wages exceeded the legislated minimum. This suggests that, on average, the minimum wage was not binding for the most productive, low wage workers and hence was likely retained after the minimum wage increased. For the most part, entrants' average wages were higher than those who exited. This was the case before and after the minimum wage increased. However, among seasonal workers, the difference between average wages of entrants and those who exited increased in the period after the minimum wage hike. It appears that among seasonal workers, farms replaced the worst paid workers with other low-income workers who were slightly better paid and presumably more productive, resulting in a slight compositional change within this segment of the agricultural sector. The minimum wage hike may have outpriced the least skilled seasonal workers. The same trends are not evident among non-seasonal workers.

5 | ECONOMETRIC RESULTS

Figures 3 and 4 plot the difference-in-difference coefficients from Equation (3) and 95% confidence intervals for three outcome variables: (a) employment growth, (b) entry rates and (c) exit rates for the

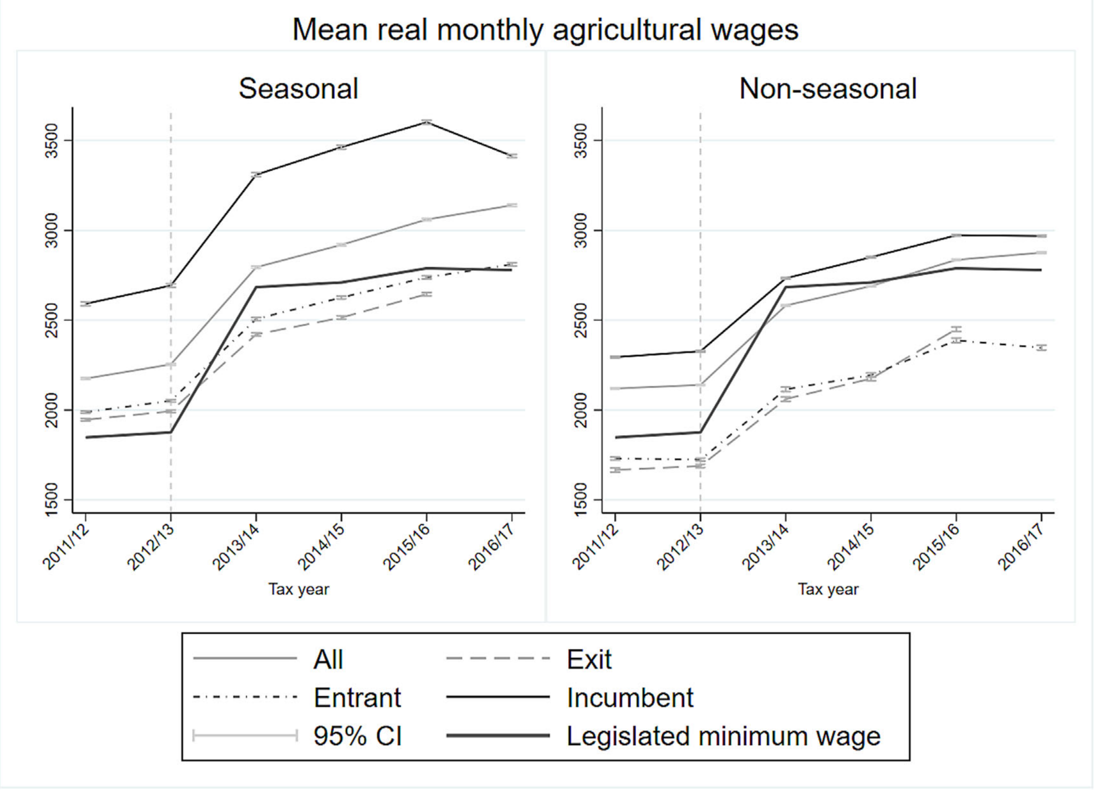

FIGURE 2 Mean real monthly agricultural wages. *Note*: Seasonal workers are defined as those who work less than 6 months a year, while non-seasonal workers work more than 6 months a year. Entrants are defined as individuals who worked in a sector they did not work in during the previous tax year. Exiting workers are defined as individuals who no longer worked in the same sector or who became unemployed in the following tax year. Incumbents are defined as individuals who did not enter and exit in the same year. The sample is restricted to low-income individuals, defined as individuals who, throughout the period of analysis, consistently earned less than R5,400 per month in real terms. December 2016 is used as the base period to convert values into real terms. To match the econometric sample, this sample consists of agricultural firms that had employees in the 2012/2013 tax year. The vertical line indicates the period/tax year preceding the relevant minimum wage hike on 1 March 2013. Source: Own illustration using Version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019)

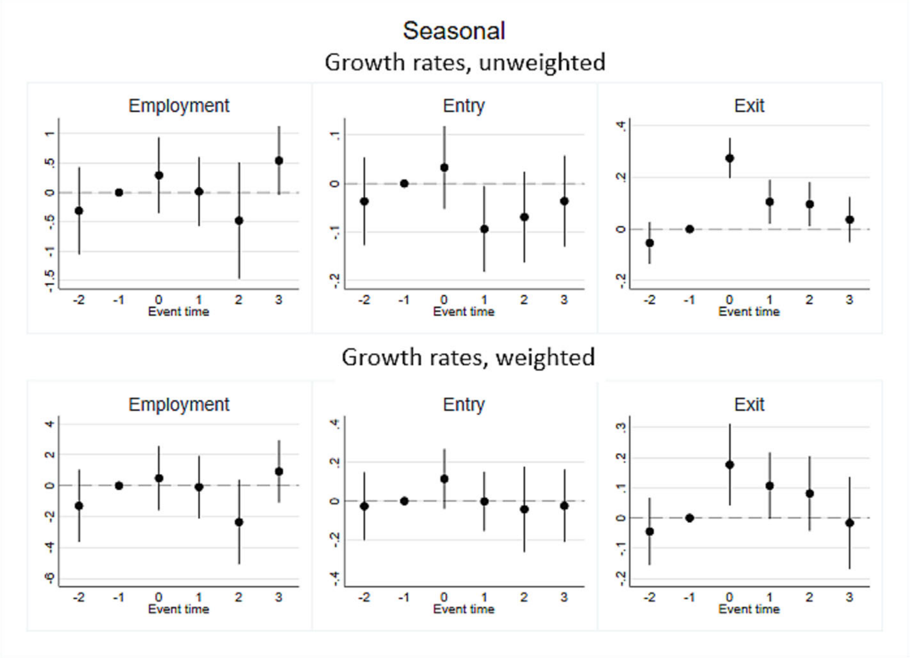

FIGURE 3 Difference-in-difference coefficient plots for seasonal workers. *Note*: Only seasonal workers are included in this sample. The figure plots the difference-in-difference coefficients (δ_t) based on Equation (3) using a negative binomial regression. The spikes indicate 95% confidence intervals. The analysis is on the firm level and was constructed by aggregating the IRP5 data to the firm-level. The dependent variables are the number of employed workers, entrants and exiting workers, respectively—the exposure variable for the employment and exit regressions is the firm size in the prior year, while it is the firm size in the current year for the entry regressions. The top and bottom panels show unweighted and weighted results, respectively. Event time $t = -1$ indicates the year before the minimum wage hike (the 2012/2013 tax year). Controls include the province where the firm is located, annual provincial rainfall, proportion of male workers and the proportion of workers in different age categories. Standard errors were clustered on the firm level. Only agricultural firms who had employees in the 2012/2013 tax year are included in the sample. The sample is restricted to low-income individuals, who consistently earned below ZAR 5,400 per month in December 2016 prices. Source: Own illustration using Version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019)

seasonal and non-seasonal workers, respectively. The top and bottom panel results are based on unweighted and weighted regressions, respectively. The regression tables can be found in Tables A4 and A5. The weighted regressions are our preferred results, as they indicate the average effect across all firms and are used to interpret the results below.

We start with Figure 3, the results for seasonal workers. We observe insignificant impacts for the period before the policy was announced, lending support for the common trends assumption and motivating the causal interpretation of our intention to treat difference-in-difference estimates. Both the unweighted and weighted results show that the growth rate of employment did not change significantly for seasonal workers. This finding is in line with the descriptive evidence shown in Figure 1. While we observe statistically significant effects on the rate of entry (decrease) and exit (increase) on the unweighted results, these largely disappear when the regressions are weighted. When firm size is taken into account, the overall net effects for the rate of employment, entry and exit for seasonal workers are zero apart from the rate of exit that spiked 1 year after the hike. The significant unweighted results suggest that mainly small farms experienced temporary changes in entry and exit rates.

Figure 4 shows the results for non-seasonal workers. In all specifications, there are no significant pre-policy effects, which again gives support for the common trends assumption. Irrespective of weighting, the growth rate of non-seasonal employment decreased following the legislative change, which was also evident in the descriptive statistics in Figure 1. However, 3 years after the change, employment growth

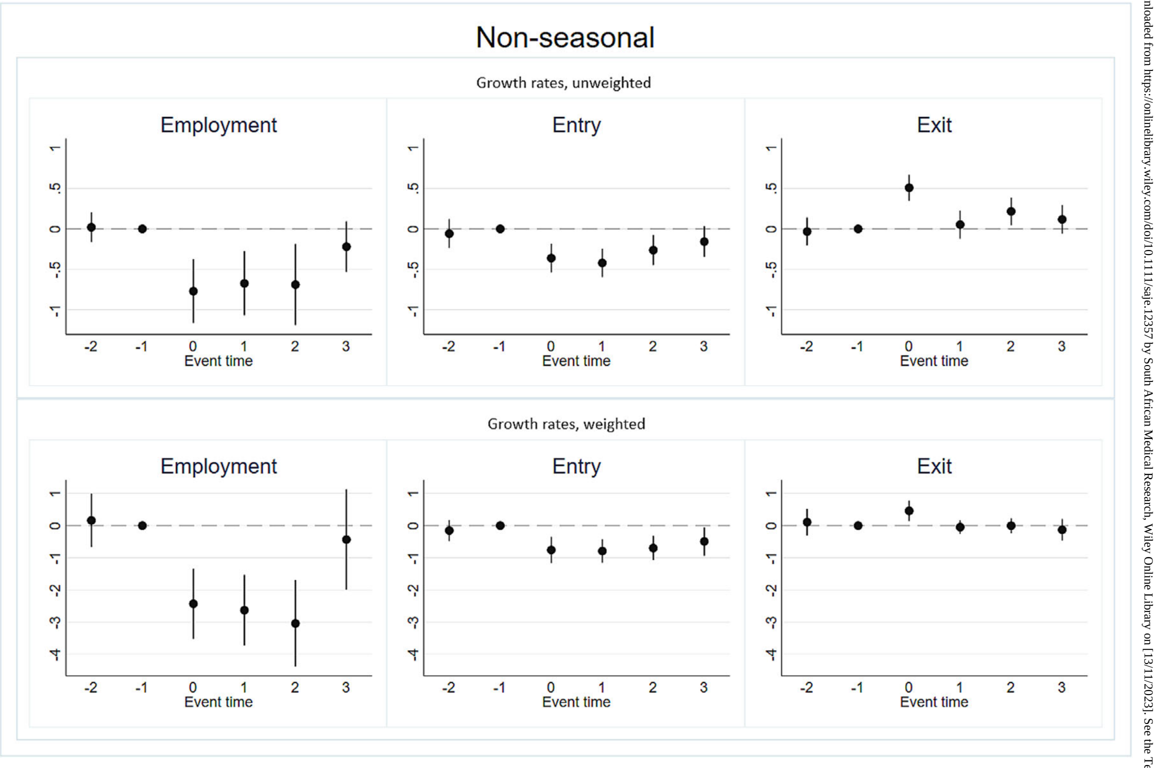

FIGURE 4 Difference-in-difference coefficient plots for non-seasonal workers. *Note*: Only non-seasonal workers are included in this sample. The figure plots the difference-in-difference coefficients (δ_t) based on Equation (3) using a negative binomial regression. The spikes indicate 95% confidence intervals. The analysis is on the firm level and was constructed by aggregating the IRP5 data to the firm level. The dependent variables are the number of employed workers, entrants and exiting workers, respectively—the exposure variable for the employment and exit regressions is the firm size in the prior year, while it is the firm size in the current year for the entry regressions. The top and bottom panels show unweighted and weighted results, respectively. Event time $t = -1$ indicates the year before the minimum wage hike (the 2012/2013 tax year). Controls include the province where the firm is located, annual provincial rainfall, proportion of male workers and the proportion of workers in different age categories. Standard errors were clustered on the firm level. Only agricultural firms who had employees in the 2012/2013 tax year are included in the sample. The sample is restricted to low-income individuals, who consistently earned below ZAR 5,400 per month in December 2016 prices. Source: Own illustration using Version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019)

rates returned to their pre-policy levels. The effects are much larger for the weighted regressions compared to the unweighted regressions, indicating that most changes occurred among larger firms. Unweighted results imply that if the fraction of affected workers in a firm were to increase from 0% to 100%, employment growth rates would decrease by between 76% 1 year after the policy change and 22% 4 years after the policy change. With average fraction affected at 0.71, effects for the mean firm peak at 54% in the first year. This implies a wage-employment growth rate elasticity that is unit elastic. Weighted regressions produce elasticities that are about three to four times higher. While we do not over-interpret this finding, it emphasises that large farm employment growth was most affected by the minimum wage. The rate of entry declined significantly after the minimum wage hike, but the effect decreases over time. If the fraction of affected workers in a firm were to increase from 0% to 100%, entry rates would decrease by 79% in the most affected year and by 49% 4 years after the minimum wage hike. With average fraction affected at 0.71, effects for the mean firm range between 35% and 56%. This implies a wage-entry growth rate elasticity ranging from 0.70 to unit elasticity in the most affected years. Non-seasonal exit rates spiked for 1 year directly after the minimum wage shock, but they rapidly returned to pre-policy rates in subsequent years. If the fraction affected was to increase from 0% to 100%, exit rates would increase by 46% for non-seasonal workers. Reading the findings together, the main channel through which the growth of non-seasonal employment was affected was through multi-period reductions in entry, a result that is also evident in the descriptive statistics. Four years after the minimum wage increase, the effects on the employment growth rate were statistically insignificant.

6 | DISCUSSION AND CONCLUSIONS

Narrowly focusing on the effects of minimum wages on employment *levels* ignores important changes in job separations and hires that affect the composition of the workforce. This study contributes to the small literature that analyses the impact of minimum wages on worker flows (Brochu & Green, 2013; Dube et al., 2016; Portugal & Cardoso, 2006). Even when net employment levels are unaffected by changes in the minimum wage, worker flows can adjust in meaningful ways. Changes in worker flows indicate whether minimum wage adjustments introduce higher separations or lower entries or both. This knowledge is essential for evaluating whether minimum wage legislation is meeting its intended goal of increasing the welfare of low-wage workers. If a legislated wage change results in higher wages and does not result in job loss and new entrants have the same access to jobs in the sector as before, minimum wage legislation is meeting its objective. However, if the legislation increases barriers to entry, it can have long-run consequences for worker welfare. This consideration is particularly important in developing countries, where formal job opportunities are already scarce and barriers to entry into formal employment high (Banerjee et al., 2008).

We used rich anonymised administrative payroll tax data to investigate the effects of a large, exogenous increase in agricultural minimum wages in South Africa on employment levels and worker flows. Our results show that the rates of employment growth, entry and exit were largely unaffected for seasonal workers. However, the descriptive statistics in Figure 2 suggest a slight compositional change among seasonal workers: Farms replaced the worst paid workers with other low-income workers who were slightly better paid and presumably more productive. Future work will further investigate whether the minimum wage hike outpriced the least skilled seasonal workers.

On the other hand, there were large decreases in the non-seasonal employment growth rate, primarily driven by a decrease in entry rates. The negative effect on entry rates disappeared over time, and their effect on employment growth was therefore temporary. Four years after the policy shock, the effect on the rate of employment dissipates. During the adjustment period, barriers to entry were increased among non-seasonal workers due to the decrease in entry rates.

Taken together, wage levels increased substantially for seasonal and non-seasonal low-wage farmworkers following the legislated wage hike and it came at the expense of short-run decreases in entry rates and employment rates among non-seasonal workers. Barriers to entry in finding formal non-seasonal employment were exacerbated only in the short term, but the minimum wage hike brought about substantial wage increases. The medium-run effects of the minimum wage increase on employment growth were limited, even if farms responded to the shock in the short run. While this paper did not analyse *how* farmers adapted over time, the results are consistent with Bhorat et al.'s (2017) findings that minimum wages have relatively small and temporary effects on employment in developing countries—likely the result of noncompliance with the legislation. While this paper highlighted that the effects of minimum wage policies vary over time, future research is required to uncover the reasons behind the dynamics of worker flows.

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ORCID

Marlies Piek https://orcid.org/0000-0002-0276-1749
Dieter von Fintel https://orcid.org/0000-0001-6771-1315
Johann Kirsten https://orcid.org/0000-0003-0770-9648

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APPENDIX A

TABLE A1 Average number of jobs held by seasonal and non-seasonal agricultural workers per year

Average number of jobs in agriculture held by low wage worker		
Tax year	**Seasonal workers**	**Non-seasonal workers**
2011	1.71	1.36
2012	1.77	1.35
2013	1.78	1.34
2014	1.81	1.34
2015	1.85	1.37
2016	1.89	1.37
2017	1.88	1.37

Source: Own illustration using version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019).

TABLE A2 Number and proportion of farms entering and exiting

	Number and proportion of farms entering and exiting				
Tax year	Entering	Entering (%)	Exiting	Exiting (%)	Total
2011	-	-	353	6%	5,940
2012	974[a]	15%[a]	358	6%	6,322
2013	719	11%	415	6%	6,659
2014	728	10%	457	7%	6,949
2015	723	10%	499	7%	7,253
2016	693	9%	791[a]	11%[a]	7,468
2017	757	10%	-	-	7,661

Source: Own illustration using version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019).
[a]Values may be overstated because of how entry and exit are defined. A longer time range (pre-2011 and post-2017) will confirm whether these are overstated or not.

TABLE A3 Number and fraction of employed in entering and exiting farms

Number and fraction of employed in entering and exiting farms					
	Entering farms	Entering farms (%)	Exiting farms	Exiting farms (%)	Total employment
2012	35,664[a]	8%[a]	16,268	4%	432,244
2013	26,793	6%	14,681	3%	462,145
2014	18,097	4%	14,031	3%	451,146
2015	20,447	4%	14,245	3%	480,628
2016	16,764	3%	27,103[a]	5%[a]	511,392
2017	19,202	4%	-	-	502,672

Source: Own illustration using version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019).
[a]Values may be overstated because of how entry and exit are defined. A longer time range (pre-2011 and post-2017) will confirm whether these are overstated or not.

TABLE A4 Unweighted regression results for employment, entries and exits

	Seasonal			Non-seasonal		
	Employment	Entry	Exit	Employment	Entry	Exit
Fraction affected (FA)	−0.218	0.318***	0.292***	0.130**	0.261***	0.025
	(−0.28)	(−0.036)	(−0.036)	(−0.058)	(−0.066)	(−0.068)
Tax year (2011/2012)	0.166	0.098**	0.05	0.001	0.148**	0.051
	(−0.334)	(−0.04)	(−0.035)	(−0.056)	(−0.073)	(−0.071)
Tax year (2013/2014)	−0.483*	−0.153***	−0.166***	0.548***	−0.291***	−0.363***
	(−0.28)	(−0.038)	(−0.034)	(−0.166)	(−0.073)	(−0.067)
Tax year (2014/2015)	−0.27	0.009	−0.091**	0.436***	−0.129*	−0.133*
	(−0.261)	(−0.039)	(−0.037)	(−0.166)	(−0.073)	(−0.071)
Tax year (2015/2016)	0.133	−0.059	−0.101***	0.445**	−0.308***	−0.259***
	(−0.46)	(−0.041)	(−0.037)	(−0.208)	(−0.076)	(−0.071)
Tax year (2016/2017)	−0.721***	−0.061	−0.088**	0.208*	−0.323***	−0.178**
	(−0.25)	(−0.041)	(−0.038)	(−0.125)	(−0.077)	(−0.072)
Tax year (2011/2012) x FA	−0.311	−0.037	−0.054	0.02	−0.058	−0.032
	(−0.379)	(−0.046)	(−0.042)	(−0.094)	(−0.091)	(−0.088)

(Continues)

TABLE A4 (Continued)

	Seasonal			Non-seasonal		
	Employment	Entry	Exit	Employment	Entry	Exit
Tax year (2013/2014) x FA	0.295	0.033	0.275***	−0.767***	−0.360***	0.507***
	(−0.329)	(−0.044)	(−0.04)	(−0.2)	(−0.09)	(−0.081)
Tax year (2014/2015) x FA	0.017	−0.094**	0.106**	−0.670***	−0.419***	0.053
	(−0.3)	(−0.045)	(−0.044)	(−0.202)	(−0.09)	(−0.088)
Tax year (2015/2016) x FA	−0.477	−0.069	0.097**	−0.686***	−0.261***	0.215**
	(−0.506)	(−0.048)	(−0.044)	(−0.255)	(−0.095)	(−0.086)
Tax year (2016/2017) x FA	0.544*	−0.036	0.037	−0.219	−0.157	0.117
	(−0.3)	(−0.048)	(−0.045)	(−0.158)	(−0.096)	(−0.089)
Proportion male	−0.599***	0.067***	0.087***	−0.344***	−0.086**	−0.130***
	(−0.081)	(−0.019)	(−0.02)	(−0.061)	(−0.04)	(−0.04)
Proportion in age Q1	0.415***	1.155***	0.155***	1.200***	2.281***	1.385***
	(−0.132)	(−0.04)	(−0.041)	(−0.125)	(−0.087)	(−0.093)
Proportion in age Q2	−0.097	0.757***	0.208***	0.685***	1.362***	0.838***
	(−0.09)	(−0.04)	(−0.038)	(−0.075)	(−0.071)	(−0.078)
Proportion in age Q3	−0.106	0.532***	0.179***	0.338***	0.783***	0.550***
	(−0.082)	(−0.043)	(−0.042)	(−0.05)	(−0.072)	(−0.078)
Proportion in age Q4	−0.058	0.474***	0.099**	0.149***	0.323***	0.333***
	(−0.08)	(−0.043)	(−0.042)	(−0.036)	(−0.072)	(−0.076)
Free State	0.04	0.169***	0.079**	−0.079	0.129***	0.061
	(−0.138)	(−0.029)	(−0.034)	(−0.066)	(−0.043)	(−0.041)
Gauteng	−0.313***	0.229***	0.230***	−0.215***	−0.002	0.111**
	(−0.108)	(−0.028)	(−0.032)	(−0.061)	(−0.047)	(−0.045)
KwaZulu-Natal	−0.257**	0.072***	0.140***	−0.108**	−0.054	0.032
	(−0.105)	(−0.023)	(−0.024)	(−0.063)	(−0.038)	(−0.034)
Limpopo	0.072	0.191***	0.228***	0.023	0.121**	0.081*
	(−0.217)	(−0.028)	(−0.029)	(−0.1)	(−0.051)	(−0.048)
Mpumalanga	−0.270**	0.118***	0.165***	−0.086	0.098**	0.033
	(−0.106)	(−0.026)	(−0.03)	(−0.062)	(−0.043)	(−0.04)
North West	−0.086	0.163***	0.189***	−0.091	0.150***	0.090*
	(−0.156)	(−0.031)	(−0.031)	(−0.071)	(−0.055)	(−0.052)
Northern Cape	0.139	0.022	0.089***	−0.103	0.135**	0.125**
	(−0.196)	(−0.032)	(−0.033)	(−0.097)	(−0.056)	(−0.05)
Western Cape	−0.105	−0.140***	−0.108***	−0.157***	−0.223***	−0.187***
	(−0.109)	(−0.024)	(−0.026)	(−0.061)	(−0.036)	(−0.035)
Rainfall	0.000	0.000	0.000	0.000	0.000	0.000
	(0)	(0)	(0)	(0)	(0)	(0)
Constant	1.368***	−1.526***	−1.233***	0.272**	−2.218***	−2.258***
	−0.309	−0.055	−0.054	−0.137	−0.094	−0.088
ln (alpha)	−0.028	−1.971***	−2.089***	−1.109***	0.009	−0.669***

TABLE A4 (Continued)

	Seasonal			Non-seasonal		
	Employment	Entry	Exit	Employment	Entry	Exit
	−0.05	−0.035	−0.037	−0.101	−0.017	−0.023
Pseudo R-squared	0.009	0.029	0.023	0.014	0.023	0.011
N	16,193	19,204	16,193	27,912	29,971	27,912

Note: The level of analysis is on the firm-level and was constructed by aggregating the IRP5 data to the firm-level. 'FA' indicates the fraction affected and is a firm-level treatment intensity variable, as discussed in Section 3.1. The regressions were run on two segments: seasonal and non-seasonal. A negative binomial regression was used. The dependent variables are the number of employed workers, entrants and exits—The exposure variable for the employment and exit regressions is the firm size in the prior year, while it is the firm size in the current year for the entry regressions. Standard errors were clustered on the firm level and are shown in parentheses. Only agricultural firms who had employees in the 2012/2013 tax year are included in the sample. The sample is restricted to low-income individuals, who consistently earned below ZAR 5,400 per month in December 2016 prices.
Source: Own calculations using version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019).
$^*p < 0.1$.$^{**}p < 0.05$.$^{***}p < 0.01$.

TABLE A5 Weighted regression results for employment, entries and exits

	Seasonal			Non-seasonal		
	Employment	Entry	Exit	Employment	Entry	Exit
Fraction affected (FA)	−0.529	0.198***	0.270***	0.34	0.613***	0.233
	(−0.998)	(−0.074)	(−0.057)	(−0.247)	(−0.14)	(−0.173)
Tax year (2011/12)	0.446	0.106	0.049	−0.071	0.186	−0.089
	(−0.997)	(−0.076)	(−0.047)	(−0.234)	(−0.136)	(−0.188)
Tax year (2013/14)	−1.289	−0.204***	−0.094	1.948***	0.187	−0.383***
	(−0.865)	(−0.067)	(−0.058)	(−0.407)	(−0.171)	(−0.144)
Tax year (2014/15)	−0.956	−0.05	−0.140***	1.891***	0.304**	−0.161*
	(−0.853)	(−0.066)	(−0.045)	(−0.461)	(−0.151)	(−0.085)
Tax year (2015/16)	1.356	−0.035	−0.104**	2.178***	0.202	−0.163
	(−1.18)	(−0.093)	(−0.05)	(−0.561)	(−0.149)	(−0.103)
Tax year (2016/17)	−1.766**	−0.063	−0.078	0.980**	0.034	−0.015
	(−0.83)	(−0.08)	(−0.064)	(−0.498)	(−0.182)	(−0.123)
Tax year (2011/12) x FA	−1.31	−0.026	−0.045	0.162	−0.153	0.106
	(−1.196)	(−0.089)	(−0.057)	(−0.423)	(−0.166)	(−0.21)
Tax year (2013/14) x FA	0.482	0.115	0.177**	−2.430***	−0.758***	0.460***
	(−1.057)	(−0.079)	(−0.069)	(−0.555)	(−0.208)	(−0.162)
Tax year (2014/15) x FA	−0.092	−0.002	0.107*	−2.631***	−0.790***	−0.046
	(−1.031)	(−0.078)	(−0.056)	(−0.562)	(−0.186)	(−0.106)
Tax year (2015/16) x FA	−2.357*	−0.042	0.081	−3.038***	−0.696***	−0.004
	(−1.395)	(−0.113)	(−0.063)	(−0.687)	(−0.191)	(−0.117)
Tax year (2016/17) x FA	0.923	−0.024	−0.016	−0.433	−0.494**	−0.129
	(−1.029)	(−0.096)	(−0.078)	(−0.797)	(−0.225)	(−0.169)

(Continues)

TABLE A5 (Continued)

	Seasonal			Non-seasonal		
	Employment	**Entry**	**Exit**	**Employment**	**Entry**	**Exit**
Proportion male	−1.126***	−0.222***	−0.184**	−0.665***	−0.378***	−0.401***
	(−0.281)	(−0.081)	(−0.079)	(−0.258)	(−0.1)	(−0.088)
Proportion in age Q1	1.166**	2.082***	1.131***	1.334*	4.121***	2.817***
	(−0.587)	(−0.169)	(−0.176)	(−0.743)	(−0.289)	(−0.35)
Proportion in age Q2	−1.435**	0.867***	0.539***	0.843*	1.352***	1.509***
	(−0.712)	(−0.191)	(−0.194)	(−0.478)	(−0.206)	(−0.181)
Proportion in age Q3	−1.001	0.871***	0.564**	0.031	0.686***	1.093***
	(−0.61)	(−0.24)	(−0.234)	(−0.428)	(−0.205)	(−0.202)
Proportion in age Q4	1.003	1.094***	0.655**	0.302	0.007	0.402**
	(−0.764)	(−0.33)	(−0.263)	(−0.307)	(−0.198)	(−0.178)
Free State	0.65	0.156	0.008	−0.853**	0.228***	0.140*
	(−0.464)	(−0.105)	(−0.111)	(−0.375)	(−0.085)	(−0.079)
Gauteng	−0.267	0.357***	0.258***	−1.328***	0.056	0.194
	(−0.369)	(−0.074)	(−0.084)	(−0.343)	(−0.096)	(−0.131)
KwaZulu-Natal	−0.316	0.217***	0.225**	−0.573	−0.067	0.111*
	(−0.349)	(−0.081)	(−0.088)	(−0.349)	(−0.065)	(−0.059)
Limpopo	0.576	0.340***	0.314***	−0.776**	0.247***	0.182**
	(−0.552)	(−0.078)	(−0.091)	(−0.353)	(−0.079)	(−0.074)
Mpumalanga	−0.602**	0.151**	0.172**	−1.059***	−0.048	0.083
	(−0.29)	(−0.075)	(−0.087)	(−0.331)	(−0.092)	(−0.067)
North West	−0.27	0.331***	0.307***	−1.310***	−0.029	0.038
	(−0.322)	(−0.08)	(−0.077)	(−0.428)	(−0.139)	(−0.183)
Northern Cape	−0.226	0.02	0.103	−1.000**	0.033	−0.01
	(−0.353)	(−0.074)	(−0.078)	(−0.459)	(−0.089)	(−0.081)
Western Cape	−0.257	−0.084	−0.044	−0.973***	−0.409***	−0.284***
	(−0.293)	(−0.072)	(−0.08)	(−0.344)	(−0.083)	(−0.077)
Rainfall	0.000	0.000	0.000*	−0.001	0.000	0.000
	(−0.001)	(0.000)	(0.000)	(−0.001)	(0.000)	(0.000)
Constant	2.911***	−1.879***	−1.728***	1.569**	−2.648***	−2.605***
	(−0.954)	(−0.202)	(−0.201)	(−0.641)	(−0.199)	(−0.221)
ln (alpha)	0.292***	−2.246***	−2.422***	−0.091	−0.364***	−1.071***
	(−0.084)	(−0.06)	(−0.066)	(−0.114)	(−0.044)	(−0.044)
N	16,193	19,204	16,193	27,912	29,971	27,912

Note: The level of analysis is on the firm level and was constructed by aggregating the IRP5 data to the firm-level. 'FA' indicates the fraction affected and is a firm-level treatment intensity variable, as discussed in Section 3.1. The regressions were run on two segments: seasonal and non-seasonal. A negative binomial regression was used. The regressions were weighted by firm size. The dependent variables are the number of employed workers, entrants and exits—The exposure variable for the employment and exit regressions is the firm size in the prior year, while it is the firm size in the current year for the entry regressions. Standard errors were clustered on the firm level and are shown in parentheses. Only agricultural firms who had employees in the 2012/2013 tax year are included in the sample. The sample is restricted to low-income individuals, who consistently earned below ZAR 5,400 per month in December 2016 prices.

Source: Own calculations using version 0.6 of the IRP5 data (National Treasury & UNU-WIDER, 2019).

*p < 0.1.**p < 0.05.***p < 0.01.

TABLE B1 Main variables used in the IRP5 data

Variable name	Variable description	Restrictions
taxyear	Time period identifier	Data from 2010/2011 to 2016/2017 was used
amt3601	Main source code for labour market earnings	The sample was restricted to low-income individuals earning less than R64,800 per year
dateofbirth	Date of birth	Only individuals in the working-age population were included
idno	Personal identifier	Observations for whom this was missing were dropped
taxrefno	Firm identifier	Observations for whom this was missing were dropped
mainincomesourcecode	The industry code is defined on the firm level, based on the main income source code and is available on the three-digit level	The mode of the industry across tax years was used
natureofperson	Specifies whether the certificate was issued for an individual, a trust, an association, etc.	All observations that were not issued for an individual were dropped
totalperiodsinyearofassessment	Indicates how many periods there are in the year of assessment	N/A
totalperiodsworked	Indicates how many periods the employee worked in the year of assessment	N/A

Source: Own compilation.

APPENDIX B: INFORMATION ON THE IRP5 DATASET

This paper uses the IRP5 dataset (Version 0.6) for the tax years from 2010/2011 to 2016/2017, available at the National Treasury in South Africa. The panel is created from administrative tax data submitted by employers for their employees.

Several considerations were essential for preparing the data. First, IRP5 certificates are issued to individuals and non-individual entities. Since this research focuses on employment, observations that are not linked to individuals were dropped. Moreover, the data are available on a job-contract level, which means that an individual worker can have multiple IRP5 certificates in a tax year, either from the same employer or from multiple employers. We limit our focus to the main jobs of individuals in each tax year.[15] This allowed us to follow individuals between states of employment and unemployment across time. Second, IRP5 certificates report on payments such as salaries, bonuses, travel allowances and other non-wage benefits, which can be identified by SARS source codes. Since the main interest here is in paid work, observations that have missing or zero salaries have been dropped.[16] This ensured that, for example, only certificates related to labour income (not pension income) were included. In addition, the sample has been restricted to the working-age population, and to observations for which anonymised personal identifiers are available. Without these numbers, it would not have been possible to track individuals across time.

We used industry codes to identify agricultural workers. Occupation codes would also have been useful to identify farmworkers specifically, but these are not recorded on tax certificates. Alternatively, we identify low-paid agricultural workers with earnings cut-offs. The thresholds should correspond to the wages of elementary farmworkers. This cut-off is obtained from the Quarterly Labour Force Survey

[15]The 'main job' was identified by taking the job with the highest earnings.

[16]The source code for salaries is 3601.

contained within the PALMS dataset. This dataset contains occupation codes, industry codes and earnings (Kerr et al., 2019). In 2017, the 95th earnings percentile for 'farmworkers' was R4,500 per month (which equates to R54,000 per year). However, since underreporting is common, we added a 20% premium to the threshold (Wittenberg, 2017). This is equivalent to earnings of R5,400 per month, or R64,800 per year. We ran the analysis on a sample of workers who earned less than R5,400 per month in 2016 prices. To circumvent the possibility of workers 'earning themselves out of the sample', we restrict our sample to workers who earned below this threshold in all waves of the panel. Since IRP5 data are filed on an annual basis, the reported number of periods worked in a tax year was used to create monthly earnings. See Table B1 for a description of the main variables used in the analysis.