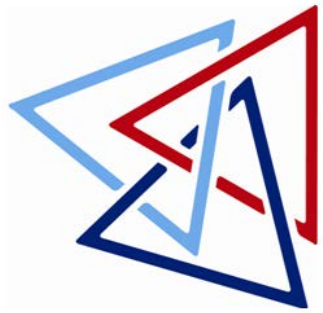

Estimating the wage and employment effects of a large increase in South Africa's agricultural minimum wage

Vimal Ranchhod and Ihsaan Bassier

Abstract

What were the effects of a 52% increase in the minimum wage in the agricultural sector in South Africa in 2013? We estimate the short run effects of this policy change on the employment and income of farmworkers, using both repeated cross-sectional data as well as individual level longitudinal data from the Quarterly Labour Force Surveys (QLFS). We find that the law had a substantial effect on the earnings of farmworkers who remained employed after the law came into effect, but that there was also a small and gradual decrease in agricultural employment. The descriptive evidence from the cross-sections indicates an increase in mean income per month of 17.9% about a year after the law came into effect. This coincided with a mean decrease in adult employment by this industry of about 8.2% over the same time period. Establishing causality empirically is challenging, though. Our difference in differences estimates indicate substantial increases in wages in this industry after the law, but this increase is not systematically related to an individual's wage rate prior to the law. There is also only very limited evidence that employment losses were statistically significant after the law. One explanation for the lack of a systematic relationship between pre-existing wages and subsequent job loss is that the wage gains following the law are observed to be more likely amongst workers who were earning relatively higher wages to begin with. Thus, endogenous compliance or partial compliance may make conventional estimators using a wage gap variable statistically invalid, and may also mitigate against unemployment effects. Overall, the most coherent interpretation of our results is that the law did cause significant increases in income for farmworkers, but did not cause substantial employment losses – although our regression models and data limitations make us cautious about these claims.

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Estimating the wage and employment effects of a large increase in South Africa's agricultural minimum wage

Vimal Ranchhod and Ihsaan Bassier[1]

(SALDRU, School of Economics, University of Cape Town)

1. Introduction

What were the effects of a substantial increase in the minimum wage in the agricultural sector in South Africa in 2013? Following worker protests primarily in the Western Cape province, the national minimum wage for farmworkers was unexpectedly raised by 52%, from R69 to R105 per day. We estimate the short run effects of this policy change on the employment and income of farmworkers, using repeated cross-sectional data as well as individual level longitudinal data from the Quarterly Labour Force Surveys (QLFS). These are large, nationally representative household surveys that are administered by StatsSA, the national statistical agency. We use variation in individual farmworkers' wages prior to the law for empirical identification. Our primary findings are that the period following the enactment of the law was one in which wages rose steeply while employment rates in agriculture fell gradually. Thus, the real aggregate wage bill for paid farmworkers in our sample was 11.5% higher one quarter after the law came into effect, but was approximately unchanged five quarters after the raise in the minimum. We argue that the wage increases were caused by the change in the policy, but that it is more difficult to attribute the changes in employment to the law.

Our paper adds to the already substantial body of evidence on the effects of minimum wages in three ways. First, it adds to the relatively small literature on the effects of minimum wages in developing countries in general, and South Africa in particular. Second, even within the larger global literature, there remains some debate about why there is ambiguity in the evidence on the employment effects of minimum wages. Is the observed ambiguity in empirical studies because there is true ambiguity in the effects, or is it the case that the papers that find no employment effects are finding these because the marginal increases in the levels of the minimum that are used for empirical identification are generally quite small? Third, we provide

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some evidence on the effects of minimum wages in contexts where many firms are non-compliant.

In addition, our paper speaks to an important labour issue that will probably become increasingly relevant in several other African countries with the passage of time. In South Africa, agriculture has historically accounted for a substantial amount of employment of relatively low skilled workers. This continues to be the case, but over the past three decades agriculture has become increasingly commercialized and mechanized, while agricultural output is increasingly being sold in highly competitive global markets. At least to some extent, these factors may explain why the long run trend in agricultural employment has been negative. An interesting and difficult policy question arises as to how a government might protect workers' well-being in such an environment, where agricultural workers have amongst the lowest mean wages in the economy. In particular, is it possible to maintain both wages and employment in agriculture as the sector evolves in response to global pressures?

The remainder of our paper is structured as follows: In Section 2, we briefly review some of the international and South African empirical literature on minimum wages. Section 3 provides some background and context in the sector, as well as a description of the law while Section 4 contains a summary of the data and related descriptive statistics. Our empirical methods are then presented in Sections 5. Section 6 contains our main regression results, Section 7 presents some caveats and robustness tests, and Section 8 concludes.

2. Literature review

The consensus on the impact of the minimum wage on employment and income has changed considerably over the past 20 years. The general view, which was widely held up to the 1980's, was that increases in minimum wages cause involuntary unemployment. By 1995, however, Card and Krueger wrote that "in the United States, the debate over the minimum wage has shifted from the question of whether minimum-wage increases cause small or large job losses to the question of whether minimum-wage increases cause any loss of jobs at all". The 'New Economics of the Minimum Wage' challenge was met by Neumark and Wascher in their review paper, claiming that "the oft-stated assertion that recent research fails to support the traditional view that the minimum wage reduces employment of low-wage workers is clearly incorrect" (2007:121). In this review, the majority of studies do suggest a negative effect,

though the distribution of measured effects is dominated by smaller and less significant estimates.

According to Brown (1988), economists' captivation with the subject is due to the strong theoretical predictions about the direction of the effect of the minimum wage on employment, combined with the convenient natural experiments to test these predictions: often, the laws can be classified as an exogenous shock and the labour turnover for the targeted population is high, leading to responsive adjustments. Beyond academic interest, minimum wage laws affect large numbers of workers who are often the most vulnerable. It is a popular policy tool presented as serving the interests of workers and around which there exists strong ideological alignments.

Because minimum wages potentially have important welfare implications, much attention is given to the econometric identification of their effects. The primary focus has been on employment effects, due to the possibility that unemployment might take some workers from a steady monthly income to zero income. This can be particularly harmful in contexts where there is limited unemployment support, either institutionally or through limited labour market alternatives which is the case in South Africa. Income is also examined as it indicates compliance with the policy, and the related aggregate wage bill is often used as a yardstick for its success.

2.1 International minimum wage literature

Although a large literature has developed, the disemployment effects of minimum wage laws remains contested. The range of estimates go from large and negative to insignificant effects, while a few studies even reveal positive effects. Table 1 lists the findings of selected studies.

Note that Table 1 only reports ranges of estimated employment elasticities. While the lower bound of disemployment effects is severe, the range masks the large number of studies reporting insignificant or small effects. Neumark and Wascher concede that "economic theory often fails to make an unambiguous prediction about the employment effects of minimum wage" (2007:4).

In the review of studies and results, a few patterns are observable. From the OECD, the UK and Sweden, there is evidence that disemployment effects may be mitigated, for example through employment protection, active labour market policies and bargained minimum wages. Second, studies focusing on less-skilled groups within affected sectors tend to find more

negative effects, suggesting a relative concentration of the harm. Third, Almeida and Terrell suggest in a review of minimum wages in developing countries that employment elasticities become more severe in proportion to the wage gap (2008) - although notable exceptions exist.

Various methodological concerns result in a large number of the above studies being unreliable, in the view of Neumark and Wascher. Common problems are the use of an unconvincing counterfactual, not accounting for the endogeneity of the minimum wage (timing often coincides with booms), and the erosion of the real wage. They further point out that longer term, lagged capital-labour substitution effects are an important, though often unexplored part of the story.

Table 1: Comparison of employment effects in minimum wage studies across regions. Studies vary between national and sectoral minimums.

Region	Studies reviewed	Employment elasticity	Comments
Industrialised countries	Studies: N=32, 10 considered reliable Countries: 10 Two additional international panel studies	Range: -0.54 to 0.07* (-1.6 to 1) Mostly negative, with many insignificant and positive effects for the UK, Greece and Sweden.	Negative effects diminish when subminimum exists or wage has been negotiated. Effects worsen for subsectors.
Latin America and Indonesia	Studies: N=15, 5 considered reliable Countries: 8	Range: -0.34 to 0.05 (-0.54 to 0.20) Again, many insignificant results. More negative effects observed for some women.	Estimates may be influenced by political variables; most minimums indicate sharp wage increases.
South Africa	BKM (2013) - 5 out of 11 sectors DR(2010) - Domestic workers	Insignificant employment effects, though some evidence of reduction in hours.	Only partial compliance observed.

* This is the range considered reliable by Neumark and Wascher, with the full range of reported results bracketed. Outlier subsector effects are omitted.

Source: Non-South African studies adapted from Neumark and Wascher (2007). BKM = Bhorat, Kanbur and Mayet; DR = Dinkelman and Ranchhod.

Another concern is that minimum wage studies based on the USA and other industrialised countries, which dominate the literature, may have limited generalizability to developing countries. Brown (1988) notes that most of those affected by the minimum wage in the former group are teenagers from relatively high income families. This is true for many of the studies

cited in Table 1, and is very different to the developing country context where a large proportion of the labour force earns at the minimum wage level. Other issues in comparisons include informal sector effects, compliance, anti-sweatshop campaigns creating excessive artificial upward pressure, and high inflation levels that have the potential to quickly erode real wage increases.

2.2 South African minimum wage literature

South Africa first introduced sectoral minimum wages in various sectors in 1999, a few years after the end of Apartheid. Table 1 shows the general prevalence of insignificant unemployment effects. One of the issues that has been raised is that the enforcement of minimum wage laws has been incomplete, although significant increases in hourly wages have been found. Some authors have suggested that perspectives of fairness, as in the case of the domestic worker sector, may have increased compliance, despite a lack of enforcement (Dinkelman and Ranchhod 2010).

While most employment effects may be insignificant, the costs of minimum wages may have been expressed in different ways. Adjusting on the intensive margin, hours, has been a key method used by employers to mitigate the effects of minimum wages. In the forestry and taxi industries, the reduction in hours was enough to offset the wage increase, leaving employees no better off in terms of earnings than they were prior to the increase in the minimum wage (Bhorat, Kanbur and Mayet 2013).

In addition, partial compliance characterises employers' responses (Bhorat, Kanbur and Mayet 2012). Generally, up to half of the employees can be observed as being paid below the minimum wage following the law. At the same time, a clear shift in wages coinciding with the law also presents itself. Part of this may be reflecting measurement error in the income data, and some of it could be explained by institutions of wage reductions in return for benefits.

Bhorat, Kanbur and Stanwix (2014) examine the effects of the first minimum wage in the agricultural sector which was introduced in 2003. They use two difference in differences estimators, one where the control group is made up of unskilled, non-unionised workers; and a second model which uses a wage gap variable to measure variations in treatment intensity.[2]

[2] Their GAP variable, which is defined by district, is the difference between the log of the minimum wage and the log of the prevailing median wage of agricultural farm workers within the relevant district.

Noting that the 2003 minimum wage was set at an extremely high level (the 70th percentile of the prevailing wage distribution), they find strong evidence of a negative employment effect together with a positive effect on total income. Hours are insignificantly affected, and an increase in non-wage compliance is observed. In addition to the possibility of decreases in non-monetary benefits, the authors also note the possibility of an increased employment of casual workers in place of permanent employees.

Other researchers have also investigated the effects of the 2003 agricultural minimum wage. In sugar farming (Murray and van Walbeek 2007) and the Western Cape grape farming (Conradie 2005) subsectors, both find negative employment effects that are not statistically significantly different from zero. Garbers (2015) uses an unbalanced panel of 53 District Councils across 19 waves to estimate effects on employment, wages, skills distribution and capital substitution, with variation coming from area-values of the minimum wage. His estimates are high, with employment elasticity of -1.3 and a tractor sales elasticity of 1.54.

Our paper hopefully contributes to the literature by econometrically estimating the impact of the 2013 agricultural minimum wage law on employment and income.

3. Background of the agricultural sector

3.1 Qualitative evidence on employment in the agricultural sector

The ILO recently commissioned a comprehensive report on farmworker living and working conditions (Ferrer and Visser 2015). There appears to be close to 600 000 households with over 2 million people living on farms. They observe a shift in labour practice, where "feudal relationships between farmers and farm workers are increasingly breaking down through movement off farms (for various reasons, including, but not only, evictions) and a shift away from the use of permanent workers towards the use of indirect labour and short-term employment contracts" (2015:1). There is a lack of rural infrastructure outside of farms, which is worsened due to incoming migrant labourers.[3]

This feeds into Hall's (2014) characterisation of the agricultural sector as a low wage industry that has been exhibiting strong negative trends over the past four decades: shedding jobs, casualising labour, consolidating from smallholder farms into large commercial farms, and a decline in the social wage. She adds high variability in employment to this, for example the

[3] Note that the above description excludes farms in former Apartheid Bantustans, which houses almost a third of South Africa's population and generally consists of small, labour-intensive subsistence farming.

shedding of 500 000 jobs in 2001 (two years before the first minimum wage) and the increase of 181 000 jobs in 2005-2006. Together with strong seasonal fluctuations, this makes causal analysis challenging.

Both the Hall and the ILO reports highlight market deregulation and globalisation as root causes of these long-term trends. Following South Africa's transition to a full democracy in 1994, state subsidisation and agricultural tariffs were reduced considerably. This integration into global food chains resulted in South African farms becoming price takers to global commodity indices, and South African farms have experienced a trend towards farm consolidation in line with global trends. Other possible explanations of the trends include the rising cost of intermediate goods and the increase in perceived labour costs.

3.2 The 2013 minimum wage

Coming to the focus of this paper, the 2013 minimum wage was introduced with effect from the 1st of March 2013. It was to some extent a negotiated wage, the middle point between the R69 minimum wage and the R150 demand, taking into consideration affordability for farm owners. R105 was settled on, constituting a 52% increase in the previous minimum.

One important aspect of the change in the law is that it came about due to sudden, sustained and occasionally violent worker protests that occurred in November 2012 in the Western Cape, with worker demands relating to the need to be paid a living wage. According to Hall (2014), being able to buy food giving 61% of recommended nutrition is only achievable by having both parents in a four-person household earning R150 a day, which was more than double the 2012 minimum wage of R69 a day. It was this nutritional figure of R150 a day that was eventually forwarded as the benchmark from the farmworkers for the 2013 minimum wage law.

Moreover, prior to 2013, the agricultural minimum wage from 2003 had generally been adjusted annually in response to inflation, with occasional small increases of a few percentage points above inflation. By all accounts, this 52% increase would have come as a shock, and a large one at that, for most farm owners.

A few studies have attempted to describe the impact of the minimum wage. The same ILO report analysed data from a small number of farms, finding that of the farms that reduced employment in 2013, most reduced seasonal labour, which affected females in particular. Of the primary strategies in adjusting to the law, they highlight casualisation, reducing hours,

shortening of contracts, increasing productivity targets and cutting benefits. Additionally, a case study of a few suppliers of farming machinery indicated an increase in mechanisation. However, these are all individual cases and cannot be generalised to the sector.

Another ILO report, in partnership with the Labour Research Service and headed by Cottle, conducted a brief general descriptive analysis of wages in this sector (2015). They find a decrease of 55 000 farmworker jobs leading up to 2014, though they accompany this with the caution that attribution is challenging and that much larger fluctuations in employment unrelated to labour costs has not been uncommon. Overall, the wage bill increased by R1.5 billion in 2013 and a further R1.6 billion in 2014. Interestingly, the wage bill in 2013, post the new law, still only accounted for just over 10% of total farming costs (Cottle 2015: 28).

Separating the minimum wage effects from other confounding factors is our primary objective, and we attempt to do so with descriptive statistics and econometric analyses in the next sections.

4. Data and descriptive statistics

4.1 Data: Quarterly Labour Force Surveys

We make use of twenty waves of StatsSA's Quarterly Labour Force Surveys for our study, using all quarters or waves between 2010 and 2014. Out of these 20 waves, just over 12 precede the minimum wage, which was implemented two-thirds of the way through the 13th wave. These are nationally representative datasets which are collected four times per year. The surveys are administered to each household member above the age of 15, and questions are asked about labour force participation, industry and occupation if employed, earnings, hours of employment, and some other characteristics such as whether the person has a written contract or not. Since wage data is only available for each year through the Labour Market Dynamics in South Africa (LMDSA) release, we use the LMDSA for all our descriptive statistics.[4] The wage data was converted to a monthly value and adjusted for inflation, using the quarterly CPI benchmarked on December 2012.

Moving to definitions, we classified a farmworker as follows: Any employed adult aged 15 to 64 who self-identifies as (a) having an occupation other than a manager, professional or clerk,

[4] The LMDSA is a dataset released by StasSA which effectively combines information from each year's four QLFS releases.

(b) working in the crop or animal farming industries, (c) with a real income less than R5000 per month (or missing), (d) who are employed for the purposes of receiving remuneration and (e) who reports their race as being either African or Coloured.

The age restriction restricts our analyses to working aged adults, while the occupational restrictions excludes relatively high earners who would probably be unaffected by the minimum wage. The R5000 income per month limit is also used to exclude a small number of high earners, and excludes about 1.5% of the remaining sample. Our restriction relating to paid remuneration excludes primarily subsistence farmers or people who work on family owned properties, as these workers are unlikely to be affected by the change in the law. Finally, the racial restriction is also due to our wanting to isolate a sample of workers who will likely be affected by the law. This rule excludes about 1% of our sample, almost all of whom are white, and who typically earn well above the minimum wage. A breakdown of the sample sizes and how they are affected by some of these rules are presented in Table 2. In total, we have just over 18000 farmworkers who meet these criteria, which averages approximately 900 observations per cross-section.

To provide some sense of our farmworkers' other characteristics, we calculated some summary statistics from our sample using the waves of data prior to the law. Farmworkers are not highly educated, with over 90% not having completed secondary school. Approximately two-thirds are male, and about 75% are African. Their mean age is 37 with an inter-quartile range of 28 to 46. About 54% and 39% of male and female farmworkers are either married or living with a partner. About 54% live in rural formal areas, 20% live in urban formal and another 20% live in tribal areas.[5]

[5] The remainder live in urban informal areas.

Table 2: Sample sizes from LMD surveys 2010-2014

	I	II	III	IV	V	VI
Year	Quarter	Wave	African/Coloured Adults 15-64	Employed	Farmworkers	Farmworkers with income<R5000/m or missing
2010	1	1	49,643	17,811	993	983
2010	2	2	49,236	17,724	919	905
2010	3	3	48,098	16,991	896	877
2010	4	4	46,828	16,710	886	870
2011	1	5	46,099	16,303	850	842
2011	2	6	45,802	16,236	839	833
2011	3	7	47,304	17,145	864	853
2011	4	8	47,517	17,535	880	861
2012	1	9	47,462	17,332	918	901
2012	2	10	47,632	17,224	846	836
2012	3	11	48,243	17,814	890	877
2012	4	12	48,149	17,776	920	909
2013	1	13	47,860	17,635	907	894
2013	2	14	48,942	18,204	924	916
2013	3	15	49,126	18,562	958	948
2013	4	16	49,448	18,759	957	943
2014	1	17	49,009	18,352	987	970
2014	2	18	47,482	17,847	911	893
2014	3	19	47,648	17,974	956	943
2014	4	20	47,122	17,901	970	951
Total			958,650	351,835	18,271	18,005

Notes:

1. There are 266 monthly income values for farmworkers that exceeds R5000 per month, which represents about 1.5% of farmworkers with non-missing income values.
2. A total of 187 White and Indian farmworkers were excluded by restricting the study to African/Coloured farmworkers. This represents about 1% of all farmworkers who are non-managers.
3. Sample sizes are unweighted.
4. We restricted farmworkers on occupation to exclude managers, professionals and clerks.

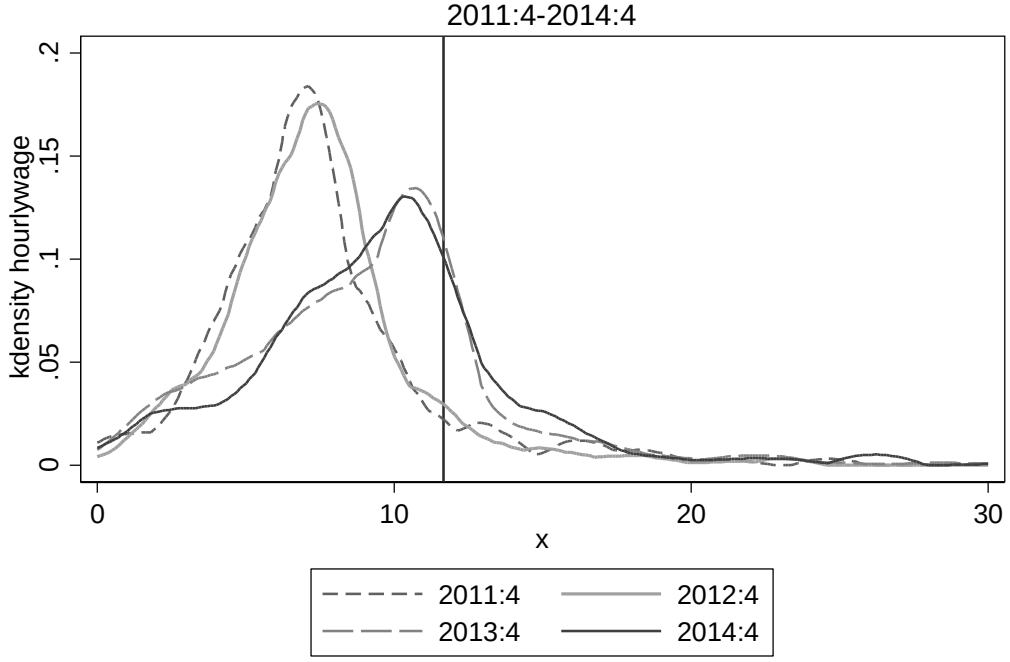
For our regression analyses, we make use of a subset of the QLFS. Over the 20 waves that we are using, the QLFS has a rotating panel component at the dwelling level, with a 25% out-rotation rate. By working with StatsSA, we matched individuals who had maintained residency in the same dwelling over time, using data from their names, surnames, age, race and gender. We thus have a sub-sample of individuals who are observed in multiple consecutive waves, up to a maximum of four waves. A summary of the overall sample size for the rotating panel, by entering cohort, is displayed in Table A1 in the appendix. We see that we have just over 250

000 repeated observations, and over 135 000 observations that we observe for the maximum of 4 consecutive quarters.

4.2 Descriptive statistics

The graph below shows kernel density estimates of the real monthly wage distribution of farmworkers, for each year's 4th quarter data.[6] What we can see is that the real wage distribution shifts sharply to the right in the period just after the law came into being, and that the mode of the distribution moves almost to the point that one would have anticipated given the level that the minimum was set at.[7] In addition, it seems that most of the adjustment occurred within one year after the law, as the difference between the distributions one year after the law and two years after the law does not appear to be that pronounced.

Figure 1: Farmworkers hourly wage by quarter

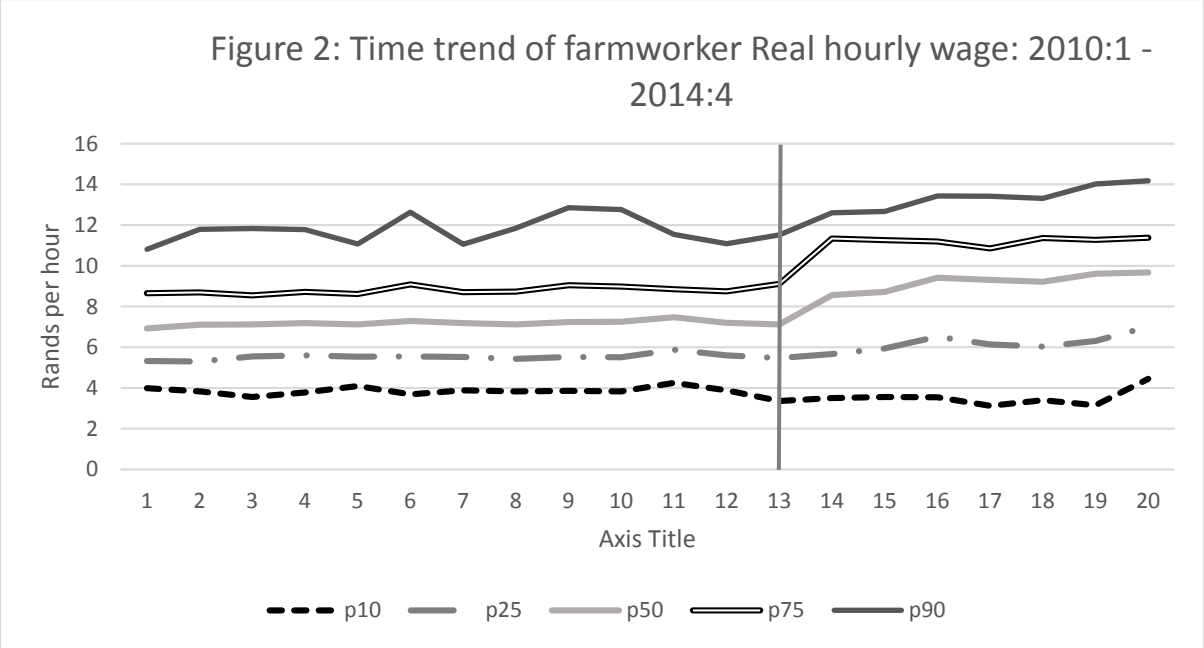


A different way to visually inspect how the wage distribution shifted in conjunction with the increase in the minimum is to plot a time trend of various percentiles of the farmworker wage distribution. We do this in Figure 2 below. This provides us with a few interesting observations.

[6] We chose to include only a select number of waves as the graph becomes overwhelming with all of the data, and it becomes more difficult to see how the wage distribution shifted as time passed.

[7] It may well be that the mode is in fact at the minimum. To calculate the hourly wage we divided monthly earnings by (weekly hours*4.3). If instead we had divided by (weekly hours*4) then the mode shifts exactly to the minimum wage line.

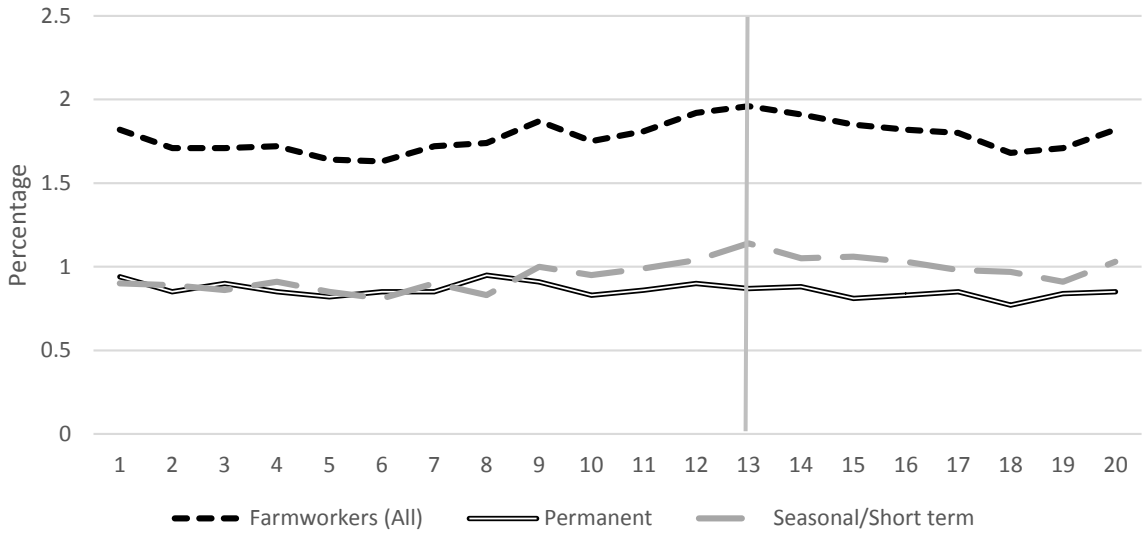
Firstly, with the exception of the 90th percentile, the wage distribution was remarkably stable from the start of 2010 until the end of 2012. Second, the new minimum wage was set at a level exceeding the 90th percentile of the prevailing wage distribution. Third, after the law came into effect, we see significant increases in the hourly wage rate, but only for those who were earning at the median wage level and above. We thus see that despite the increase in wages, most farmworkers are not earning the minimum even by wave 20, which is almost two years after the law came into effect. Indeed, even the median wage earner in wave 20 is earning a wage that is non-compliant, and it is unlikely that measurement error can account for such a large discrepancy. Our summary evidence then is that wages did increase, although there was a substantial amount of heterogeneity in terms of responses. In addition, the timing of the law coincides very neatly with the observed wage increases, and this suggests that the increase probably was due to the new minimum wage.



The next graph that we present shows how the labour absorption rate changed over our 20 waves of data. We present the aggregate percent of adults employed as farmworkers, and further separate this group into permanent and seasonal agricultural workers. What we observe is that, after the law, there is a gradual but sustained downward trend in each of these sub-groups of agricultural workers, although the decline is stronger amongst seasonal workers. The aggregate of these two groups combined thus shows an even stronger decline in the number of farmworkers of either type, which suggests that the law coincided with a substantial decline in agricultural employment. Further evidence of a negative employment effect is observed as

there is a change in the trend in employment levels, which was increasing prior to the law and then started decreasing afterwards. On the other hand, it is difficult to make a compelling argument either way, as the trend seems to display a number of turning points in the years preceding the minimum wage as well.

Figure 3: Time trend in percentage employed as farmworkers: 2010:1-2014:4



In the cross sections, we observe that the mean real income increased from R1457 per month at the end of 2012, to R1773 per month a year later and finally to R1871 per month by the end of 2014. This represents a 28.5% increase in mean real monthly income in a two year span. At the same time, the percent of African or Coloured adults employed as farmworkers decreased from 1.92% to 1.82%, a decrease of 5.2%. The question is whether these changes can be attributed to the change in the minimum wage, or are due to some other confounding factors that affect both the wage distribution and the employment rates simultaneously.

5. Empirical methods

To estimate the causal effects of the law, a number of empirical approaches may be employed. The existing literature provides several illustrative examples that we considered. One of the most popular approaches is the difference-in-differences method (DD) which was used in the paper by Card and Kruger (1994). This method involves identifying a group of similar workers who would have experienced the same trends in employment and wages absent the law, but where some workers are subject to the law and some are not. This provides for a natural experiment to estimate the difference between the treated and untreated groups.

In our case, however, it is difficult to imagine who the comparison group is that would experience the same trends and shocks as agricultural workers, but who are unaffected by the law. First, unlike Card and Kruger, we do not have geographic variation as the law was a national law for the entire agricultural sector. Second, we might consider workers in other sectors but the shocks to agriculture, such as weather and pestilence, can be highly idiosyncratic and would be unlikely to affect workers in other industries in a similar manner. This would apply regardless of whether we considered a single other industry or a composite group of workers from multiple other industries.

Ultimately, we decided to use individual level variation in wages in Wave 13 to measure variation in treatment intensity, and to test whether this variation is systematically related to the likelihood of wage gains or job loss amongst farmworkers, in a way that is different to any relation that may have been present in earlier waves. We restrict the estimation sample to members of the panel who were farmworkers in either waves 1, 5, 9 or 13. Wave 13, being the wave that contains the 1st of March 2013, is our treatment wave, while farmworkers from time periods before the law capture our pre-treatment period.[8]

Our treatment intensity variable is a *percentage GAP* variable, which is defined as the inflation adjusted difference between a farmworker's wages and the 2013 minimum wage. Farmworkers who earn more than the 2013 minimum (in real terms) are assigned a value of zero for the *percentage GAP* variable. Our dependent variables are *pctwagegain* (percent wage gain), *hours change* and *job loss*, which are measured one, two and three quarters after the *percentage GAP* variable is calculated (where possible). We thus have three regressions for each dependent variable, which allows us to trace out a time path of adjustment to some extent.[9] All of our regression models were fitted using the conventional sampling weights from the cross-sections.[10]

[8] We used only these pre-treatment waves for two reasons. First, this implicitly allows for a comparison as any seasonal variations in agricultural employment will be naturally accounted for. Second, since no individual could be in the panel for more than four quarters, there is no chance of overlap between respondents in these different cohorts.

[9] Note that we can go at most three periods forward due to the 25% out-rotation design.

[10] We also estimated the results using an adjusted *inverse probability weight*, which corrects for non-random attrition that is related to observable characteristics (see Wooldridge 2002, pp 587-590). The two different sets of weights do not change the results in a substantive way.

Our regressions were estimated with additional covariates which are a quadratic in age, a gender dummy and a race dummy. Formally, for t=1, 2 and 3, and wave =1 or 5 or 9 or 13, we estimate:

$$Y_{i,t} = \beta_0 + \beta_1 \, Post + \beta_2 \, pctwagegap_{i,0} + \beta_3 \, Post*pctwagegap_{i,0} + \beta_4 \, X_{i,0} + \varepsilon_{i,t}$$

Where:

$pctwagegap_{i,0} = 100*(2275- monthly\_wage_{i,0})/2275$

Post=0 if wave=1, 5 or 9, and Post=1 if wave=13

$X_{i,0}$ are age, age squared, Coloured and Female dummies

Y_i are Δ hours worked, job loss (i.e. employed as a farmworker), and pctwagegain (from t_0).

6. Regression results

Our regression results are presented in Table 3 below. For each outcome variable, we present the regression results that were estimated using the sampling weights. The estimates on the interaction terms represent the changes in the dependent variables for wave 14, 15 and 16 respectively, that are systematically related (in a linear way) to the *percentage GAP* variable, over and above any linear relationship that *percentage GAP* had to these outcome variables in previous waves.

Note that the sample sizes drop quite sharply as we measure the outcome variable in each subsequent quarter, as there is a greater element of attrition for both 'natural' reasons as well as due to the out-rotation that is part of the design of the sampling framework. We also trimmed the estimation sample for the wage growth regressions by excluding observations that had a growth rate greater than 200% across the relevant waves.[11]

Let us start by focussing on the wage gain regressions. The units here are in percentage points, and so in column 1 of Table 3, we observe that in wave 13, farmworkers who maintained employment as farmworkers for one period (i.e. up to wave 14) experienced a 10.98 percentage point increase in their wages on average, relative to similar farmworkers in waves 1, 5 or 9.[12] Proceeding similarly, we measure that the real wage increase on average was 8.35 and 20.77 percentage points after 2 and 3 periods respectively.

[11] We did this as there are a small number of exceptional growth rates which substantially affect our regression results. We believe that these are most likely to be measurement error, or else people changing jobs, as an increase of greater than 200% in less than a calendar year is an extremely large increase. This excludes 18, 11 and 3 observations from our 1, 2 and 3 period growth regressions respectively.

[12] As measured in wave 2, 6 and 10 respectively.

At the same time, the coefficient on the *percent GAP* variable has a large, positive and clearly statistically significant coefficient for all waves, including those prior to the law. The most likely explanation is that this reflects a substantial amount of measurement error in the wage data. To the extent that this is mean reverting over time we will observe a spurious positive correlation between the *percentage GAP* and *pctwagegain* variables.

The coefficient on our interaction term, which we argue represents the effect of the law in the difference in differences model, is negative and significant at -0.147 after 1 period, and negative and marginally significant after 3 periods at -0.325. If we interpret the *percent GAP* and *post* percent GAP* coefficients together, they would suggest that having wages further from the minimum are associated with positive wage gains in both pre-law and post-law time periods, but that this effect is attenuated in the post law period.

Turning next to the *jobloss* and *hours worked* regression results we see that in general there is little different between wave 13 and the earlier comparison waves. Most of the coefficients are small and not statistically significant, with the one exception being the regression on job loss after 1 period. In this regression, however, the coefficient on the interaction term is -0.00219 and statistically significant, which would mean that having wages farther below the minimum wage would lead to a decrease in the likelihood of a job loss. Moreover, this would need to be interpreted in conjunction with the *percentage GAP* coefficient, which is very similar in magnitude and of the opposite sign. Together they would imply that *percentage GAP* has almost no linear relationship with the probability of losing a job within one quarter after the law was passed, in contrast to what we observed in previous waves.

Overall, the regression results do not provide support for the hypothesis that the law caused significant employment losses on either the intensive or extensive margins, although this needs to be considered in conjunction with any attenuation bias arising due to measurement error in the *percentage GAP* variable. Measurement error becomes even more problematic in the *pctwagegain* regressions, where mean reversion leads to spurious positive coefficients.

Table 3: Regression results									
	% wage gain	% wage gain	% wage gain	job loss	job loss	job loss	Hours	Hours	Hours
	1 period	2 period	3 period	1 period	2 period	3 period	1 period	2 period	3 period
post	10.98***	8.352**	20.77***	0.0589*	-0.0679	-0.0570	-0.266	1.117	0.541
	(3.086)	(3.483)	(6.811)	(0.0344)	(0.0465)	(0.0838)	(0.694)	(1.014)	(1.613)
pctgap	0.455***	0.293***	0.444***	0.00188***	0.000842	0.000525	-0.00298	0.0184	0.0528***
	(0.0441)	(0.0508)	(0.0875)	(0.000459)	(0.000614)	(0.00100)	(0.00960)	(0.0145)	(0.0202)
post_pctgap	-0.147**	-0.0606	-0.325*	-0.00219***	0.00122	0.000772	-0.0106	-0.0342	-0.0302
	(0.0734)	(0.0834)	(0.166)	(0.000794)	(0.00106)	(0.00196)	(0.0163)	(0.0239)	(0.0384)
age	0.225	-1.187**	-1.957*	-0.00465	-0.00292	-0.0190	0.212**	0.188	0.164
	(0.473)	(0.547)	(1.094)	(0.00525)	(0.00720)	(0.0130)	(0.107)	(0.160)	(0.262)
agesq	-0.00450	0.0152**	0.0232*	2.93e-05	-6.07e-06	0.000224	-0.00290**	-0.00160	-0.00175
	(0.00602)	(0.00685)	(0.0138)	(6.73e-05)	(9.11e-05)	(0.000165)	(0.00135)	(0.00200)	(0.00330)
_Igender_2	-0.847	2.330	-0.156	0.119***	0.175***	0.149***	0.533	0.411	-0.317
	(1.610)	(1.935)	(3.257)	(0.0176)	(0.0246)	(0.0389)	(0.364)	(0.565)	(0.776)
_Irace_2	8.103***	2.839	4.700	0.00351	0.00681	0.0372	0.777*	0.296	0.496
	(1.818)	(2.101)	(3.612)	(0.0204)	(0.0277)	(0.0439)	(0.409)	(0.614)	(0.858)
Constant	-16.12*	10.47	22.16	0.202**	0.234*	0.542**	-4.061**	-5.936*	-5.594
	(9.034)	(10.52)	(20.87)	(0.0995)	(0.137)	(0.247)	(2.037)	(3.072)	(4.999)
Observations	1,734	959	389	2,161	1,211	525	1,756	969	394
R-squared	0.082	0.063	0.086	0.038	0.059	0.037	0.010	0.012	0.023
1. Standard errors in parentheses									
2. *** p<0.01, ** p<0.05, * p<0.1									
3. Weighted by cross-sectional weights									
4. The wage gain estimation samples were trimmed to exclude observations with more than 200% growth.									
This excludes 18, 11 and 3 observations from the 1, 2 and 3 period growth regressions respectively.									

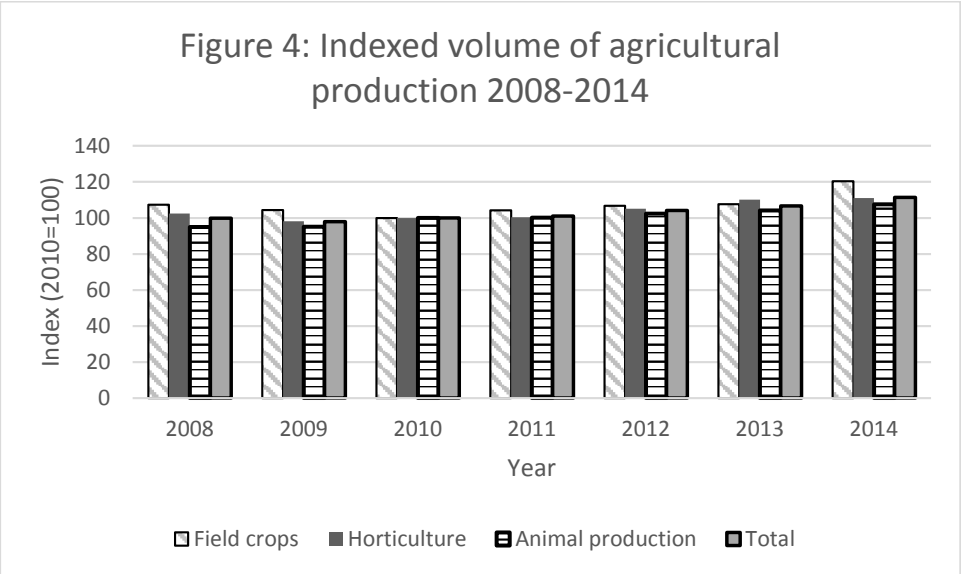
In addition, since the most striking coefficients in the wage gain regressions relate to the *post* variable, it seems that the law either had no effect on the evolution of wages, or had an effect for farmworkers that was not linearly related to the distance between their wave 13 wage and the minimum wage. Our analysis is thus further complicated by a setting where there is endogenous non-compliance or partial compliance with the law.

7. Caveats and robustness tests

There are a few potential threats to our findings stated above. First, there may be confounding factors that are driving both the increase in wages and the change in employment that are not due to the minimum. There are also econometric issues, such as mean reverting measurement error and non-random attrition that could be driving the panel related results. In this section, we try to address these issues where possible.

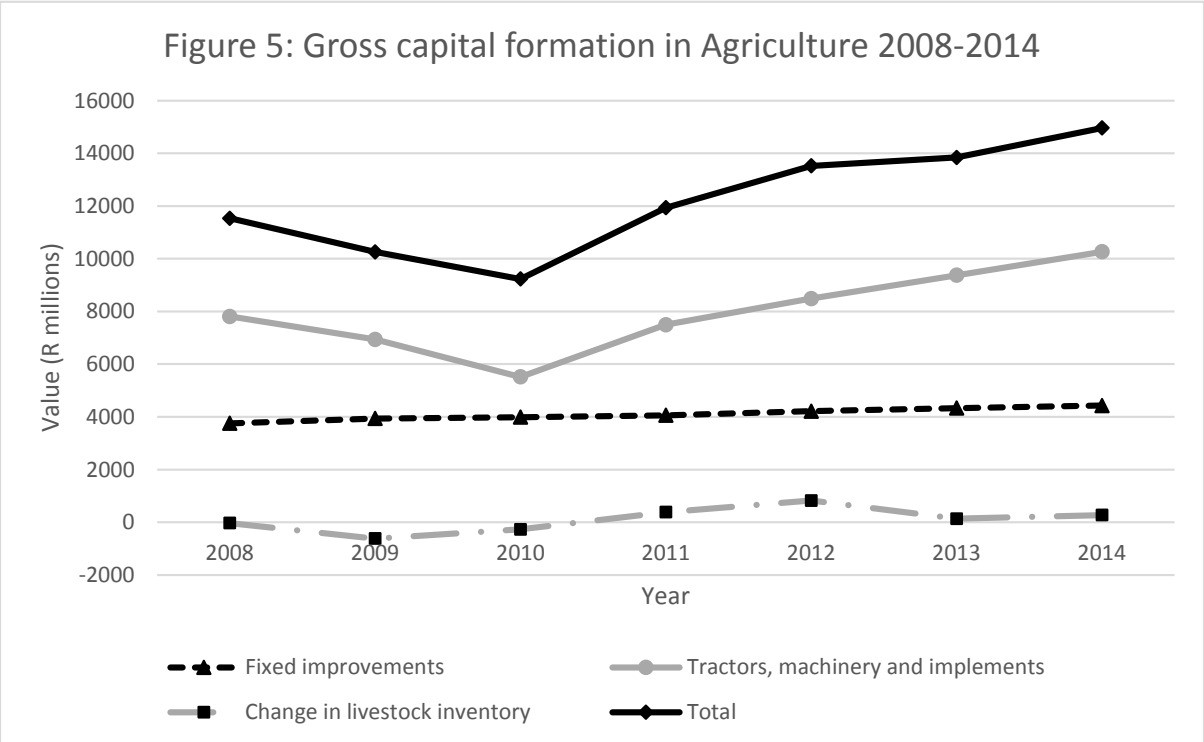
7.1 Agricultural output

In the Figure 4, we plot agricultural output per annum, as obtained from the *Department of Agriculture, Forestry and Fisheries Annual Report 2015*. We see that output was increasing in a stable fashion over this period, which might explain wage increases but probably not the sharp increase in mean wages or the shift in the distribution that we observed in 2013.



7.2 Capital–labour substitution

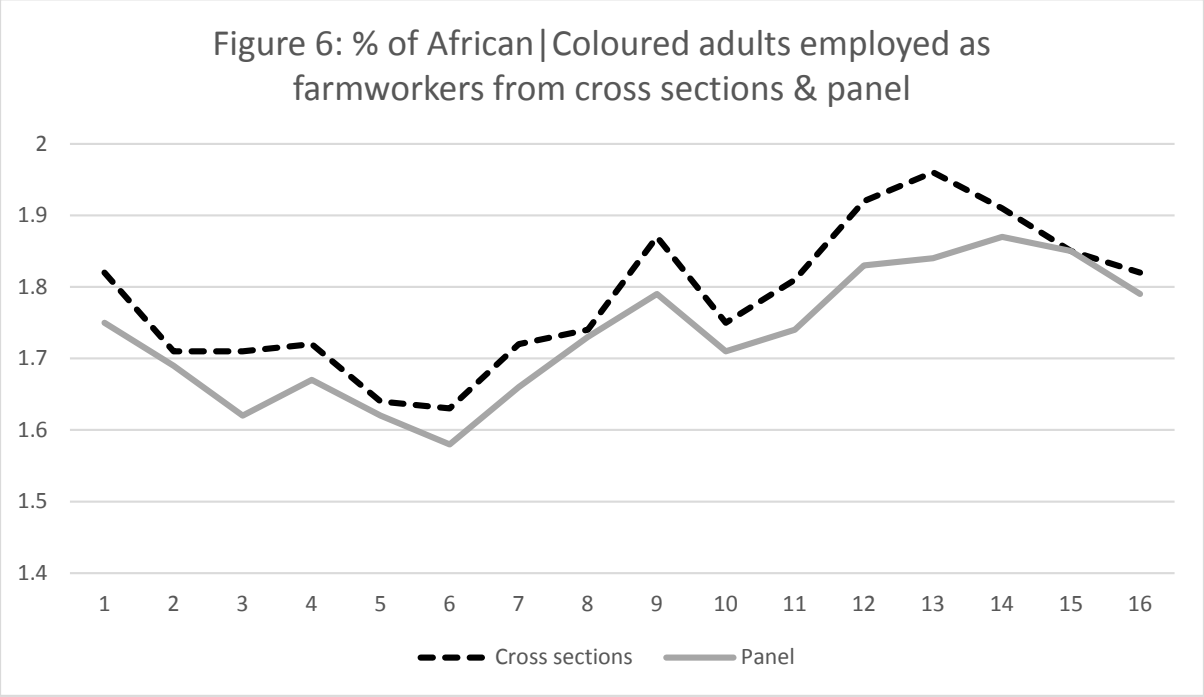
One possibility to reconcile aggregate employment decreases with increases in agricultural output would be to observe substantial increases in capital investment. Note that this could be an outcome of the law though, and would be one way in which farmers could respond to the increase in labour costs. We next plot gross capital formation from 2008-2014, where the data was obtained from the same report. Our observation is that investment in capital was indeed increasing over this time period, although the upward trend started by 2011 and this was simply maintained in 2013 and 2014. There is thus insufficient evidence on the capital formation side to explain a reduction in agricultural employment due to investments in capital in 2013 alone, which also were not observed prior to 2013.



7.3 Non-random attrition in panel

A different concern may be that the panel results are driven by sample selection rather than 'true' effects. In addition to using the inverse probability weights in our regressions as a robustness check, we can also ask how well our aggregate trends from the panel sub-sample tracks that of the full cross sections. This is presented in the Figure 6 below.

While the two samples have slightly different levels of employment, their trends do match each other reasonably well. Since our estimates are all being driven by changes in employment, income or wages, the similarity in the trends suggests that selection into the panel is probably not an important driver of our obtained results.



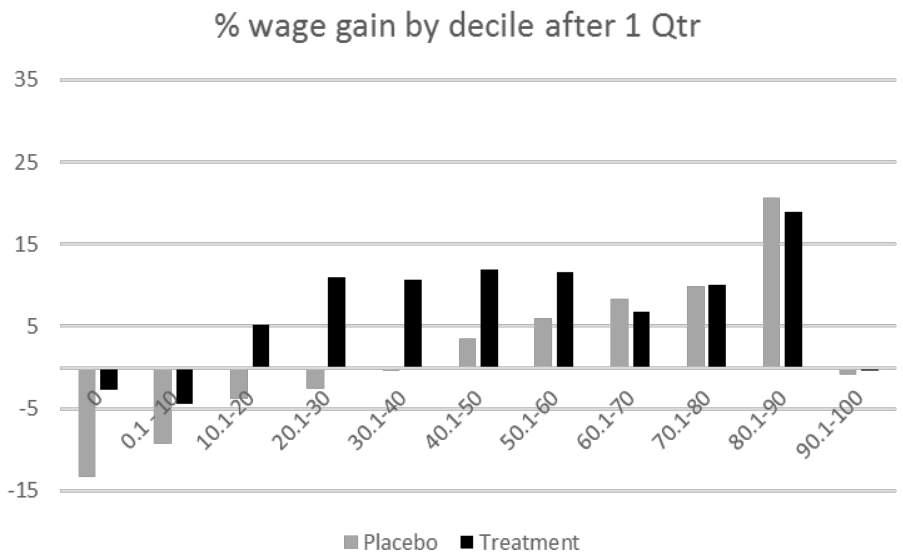
7.4 Panel evidence on mean reverting measurement error and non-linear responses to the law

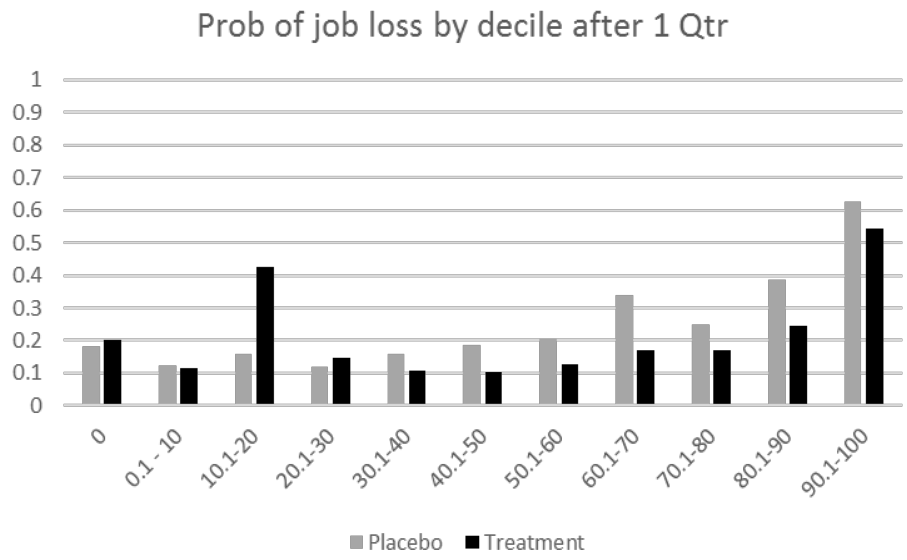
We have already discussed the challenges to estimation caused by measurement error in the wage variable. In Figure 7, we plot the wage growth and probability of job loss for different intervals of the percent GAP variable, for both the `placebo' (i.e. Waves 1, 5 and 9) and `treated' (i.e. Wave 13) observations.[13]

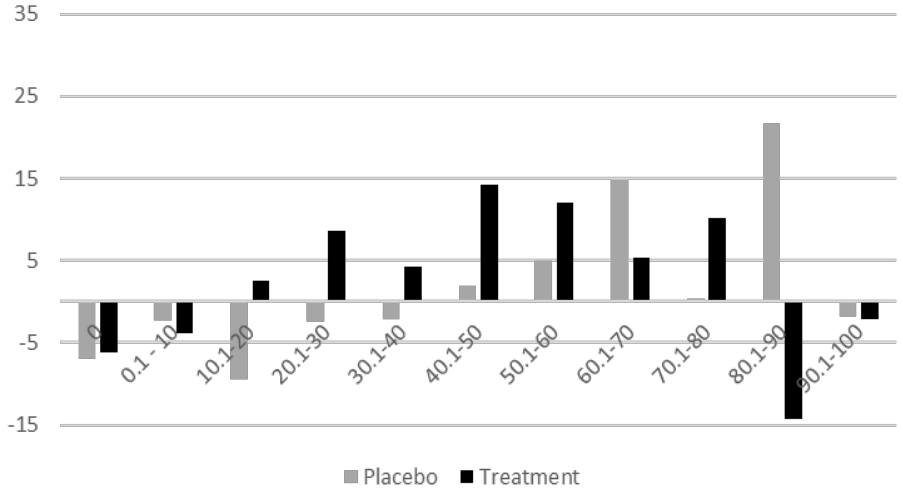
The first striking thing that becomes visible is that there is substantial evidence to believe that mean reversion is important to understand the panel results. This is clear from the placebo observations, where we see large negative wage growth amongst high wage earners and large positive wage growth amongst low wage earners. It is extremely unlikely that high earners are experiencing actual wage decreases, and the more plausible conclusion is that this observation is a result of the measurement error.

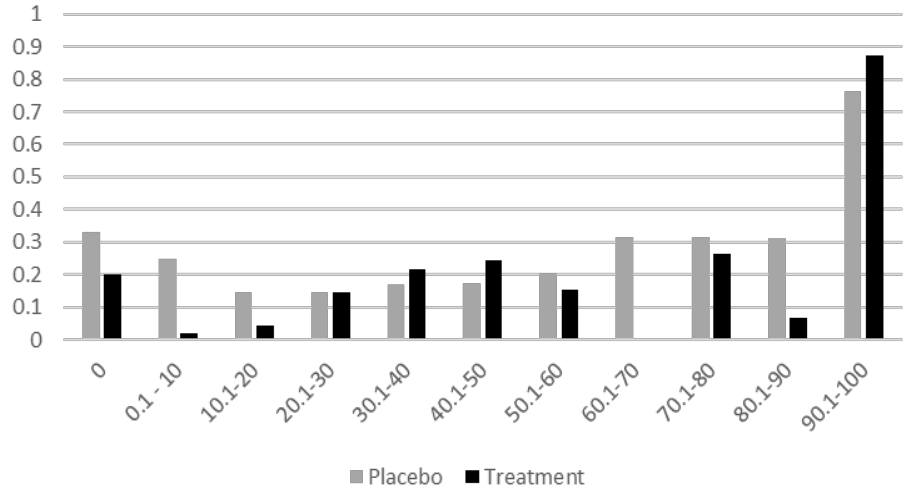
[13] We have omitted the corresponding graphs for three period changes as they tend to have very small cell sizes, especially for high values of the *percent GAP* variable and for the treatment observations.

Figure 7: Wage gains and job losses by decile of percent GAP









Second, we see that wage increases after the law were clearly not linearly (or even monotonically) higher, relative to earlier waves, for high values of the percent GAP variable. In fact, the greatest differentials for wave 13 were in the 10-60 percent range of GAP values. These graphs help us to see why the regression models were not identifying significant coefficients for our interaction term.

When we look at the two graphs on the right-hand side of Figure 7, it is difficult to see any clear patterns. Having a wage gap in the 10-20% range was related to a much higher chance of losing employment within one quarter in wave 13 relative to earlier waves, but for values of GAP above 30% this pattern is reversed although not as striking. Furthermore, the one substantial spike after 1 quarter is no longer present after 2 quarters, either due to attrition of the sample or because some respondents may have found another job as a farmworker. Indeed, after 2 quarters the placebo observations have higher job loss probabilities (than the treated observations) for values of GAP between 0-20%, as well as between 50-90%. It is only for GAP values between 30-50% that the wave 13 observations are more likely to lose employment, and even this does not align perfectly with the wage gains shown on the left-hand side of Figure 7.

To summarize, having considered all of the different caveats and evidence, we conclude that the minimum wage did indeed cause a substantial increase in wages for some subset of farmworkers, but that there is insufficient evidence to attribute a substantial decrease in employment to the minimum wage.

8. Conclusion

We investigated the effects of a large (52%) increase in the minimum wage for agricultural workers in South Africa in 2013. Our descriptive evidence indicated sizable rightward shifts in the wage distribution, with the mode shifting close to the new minimum, although there remained substantial levels of non-compliance. At the same time, there was a gradual but sustained decrease in employment in this sector in the year following the increase in the minimum.

By using the rotating panel component of the QLFS data, we estimated several difference in differences models using pre-existing wages for identification. We found no evidence that workers who previously earned below the minimum subsequently received greater wage increases. Nor were they more likely to lose their jobs after the minimum was introduced,

relative to similar workers from earlier waves. Our robustness checks support the argument that the decrease in employment observed is not systematically related to the increase in the minimum.

As with all minimum wage studies, the welfare effects of the law need to be considered quite carefully. There are both winners and losers from the law, as well as distributional implications both within and between households, which this paper has not considered. While South Africa is about to implement a national minimum wage, based on the evidence here we remain agnostic about the welfare effects of an exemption for agricultural workers.

One of the contributions of this research to the broader minimum wages literature relates to the large magnitude of the increase. We found at best limited evidence of employment losses, but substantial levels of non-compliance; and these two findings are almost surely related. If the general minimum wages literature is interested in what happens when large increases are enforced, then our study is not informative. On the other hand, perhaps a primary question for minimum wage policies relates to enforcement policy, as violations become more lucrative when the increases are large.

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Appendix

Table A1: Sample sizes from rotating panel

Entering wave	Size of entering cohort	Number of periods observed		
		2	3	4
1	31,880	12,672	10,391	8,817
2	12,919	2,439	2,082	8,398
3	11,768	2,374	1,855	7,539
4	11,298	2,389	1,571	7,338
5	11,898	2,442	1,920	7,536
6	11,542	2,274	1,688	7,580
7	13,222	2,988	2,321	7,913
8	12,469	2,314	1,888	8,267
9	12,644	2,486	1,815	8,343
10	12,619	2,105	1,910	8,604
11	12,432	2,473	1,800	8,159
12	11,959	2,115	1,949	7,895
13	12,703	2,480	1,988	8,235
14	13,145	2,620	2,090	8,435
15	12,791	2,602	1,939	8,250
16	12,552	2,502	2,762	7,288
17	12,446	3,812	1,573	7,061
18	10,287	1,853	8,434	0
19	10,986	10,986	0	0
Total	251,560	65,926	49,976	135,658

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