Identifying the wage penalty in the labour broker sector: Evidence for South Africa using administrative tax records

Aalia Cassim[1] and Daniela Casale[2]

[1] Senior Economist at National Treasury and PhD candidate, AMERU, School of Economic and Business Sciences, University of the Witwatersrand

[2] Associate Professor, AMERU, School of Economic and Business Sciences, University of the Witwatersrand

Corresponding author: Aalia Cassim (aalia.cassim@treasury.gov.za; +27825652996; 201 Sevenoaks, 21 Third Street, Killarney, Johannesburg, South Africa, 2193)

Aug 2018

Funding: The authors would like to acknowledge the World Trade Insititute that provided funding for this paper in terms of the Employment Project of the Swiss Programme for Research on Global Issues for Development (r4d).

Acknowledgements: This project was run through AMERU (African Microeconomic Research Unit) at Wits, and the support of Volker Schoer is gratefully appreciated.

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Abstract

This paper examines the wage penalty in the temporary employment services (TES), or labour broker, sector. Although this sector has been growing in recent decades and there has been much heated public debate on whether the jobs offered constitute 'decent work', there is little empirical research for developing countries especially. This is partly due to data constraints, as it is often not possible to accurately identify TES workers in household or labour force surveys. This paper uses unique administrative data released by the South African government based on company and employee income tax records for the period 2011 to 2015 to estimate the TES wage penalty. In the analysis, we account for individual and time fixed effects, and we control for a number of time-varying individual and job characteristics. We are also able to explore whether the penalty is driven largely by differences in the base wage or by differences in the benefit contributions between TES and non-TES workers. We find a substantial TES penalty of around 30 percent, 80 percent of which is due to differences in the benefit contributions. Providing empirical evidence on the labour broker penalty is an important first step to help inform debates on the role of this sector in the South African labour market as well as other developing countries.

Keywords: temporary employment services; wage differentials; administrative data; South Africa

JEL codes: J31; J41

1. Introduction

The growth of temporary employment over the last few decades has been well documented for both developed and developing countries (Segal and Sullivan 1997; Deakin 2002; Autor 2003; Benjamin, Bhorat, and van der Westhuizen 2010). In part, this is related to firms requiring lower adjustment costs in certain economic environments, such as poor macroeconomic conditions (Holmlund and Storrie 2002), or when there is a need to become more competitive (Matsuura, Sato, and Wakasugi 2011; Saha, Sen, and Maiti 2013). Holmlund and Storrie (2002) find that poor macroeconomic conditions in Sweden in the 1990s resulted in employers offering more temporary contracts, and employees being more willing to accept this form of employment. In Japan, global competition in tradable goods led to a rapid increase in temporary employment, specifically in those sectors where the bulk of sales was to foreign markets (Matsuura, Sato, and Wakasugi 2011). Similarly in India, both pro-worker labour institutions and increased import penetration led to greater use of contract labour in the Indian manufacturing sector (Saha, Sen, and Maiti 2013). In South Africa, it has been suggested that trade liberalisation led to firms externalising employment because of the drive to lower wages in sectors where there has been increased competition (Theron 2005).

Given the context in which temporary employment grows, it is widely expected that there would be a wage differential between temporary workers and non-temporary workers (Lass and Wooden 2017). Indeed, a wage penalty for temporary workers has been found in a number of countries including Portugal (Boeheim and Cardoso 2007), Germany (Pfeifer 2012), Britain (Brown and Sessions 2005) and the U.S. (Segal and Sullivan 1997; Houseman 2001). After adjusting for demographic factors, job characteristics or controlling for fixed effects, wage penalties are estimated to range from 6 percent in the UK (Booth, Francesconi, and Frank 2002) to around 20 percent in France and the U.S. (Segal and Sullivan 1997; Blanchard and Landier 2002). Picchio (2006) estimates a wage penalty for temporary workers of around 12-13 percent in Italy, but this declines with seniority of temporary workers, with a reduction in the wage gap of about 2.3 percentage points after one year of tenure. While the wage gap tends to decline after controlling for certain characteristics, where the gap persists for temporary workers is in terms of benefits, such as pension, medical aid and unemployment insurance. Temporary workers have been found to have far lower access to benefits than permanent workers, even after controlling for factors such as race, education and location (Houseman 2001). This suggests that employers use labour brokers as a way to lower costs both in terms of the base wage and benefits.

Almost all of the international evidence on the size of the wage penalty for temporary workers is for developed countries, despite the fact that temporary work forms a substantial component of the labour force in many developing countries and is on the rise (Benjamin, Bhorat, and van der Westhuizen 2010). This paper contributes to this literature by exploring the wage penalty in the temporary employment services[1] (TES) sector for South Africa, using unique administrative data from tax records. In 2015, the South African Revenues Services (SARS) and the South African National Treasury (NT) released company and employee tax records for research purposes (SARS-NT 2015), and to our knowledge, this is the first dataset of this kind for an African country.[2] The wages and conditions of workers in the temporary employment services sector have featured heavily in public debates on what constitutes decent work in developing countries. In South Africa, which has a strong and politically powerful union movement, there has been a particularly heated debate on the TES, or labour broker, sector, as it is often referred to. In fact, concerns that TES sector workers were being unfairly treated resulted in amendments being made in early 2015 to that part of the Labour Relations Act (LRA) that governs temporary employment. The new legislation attempted to better regulate the TES sector and offer greater protection to temporary workers by implementing stricter hiring conditions for TES workers. These changes were made despite there being little reliable empirical evidence on the extent of a penalty to TES employment in South Africa (or other developing countries).

[1] Workers in temporary employment services, as defined here, are employed by staffing agencies, where these agencies are

[2] There have been only a handful of research papers using these data in the past two years. These include studies on the employment tax incentive (Chatterjee and Macleod 2016; Ebrahim, Leibbrandt, and Ranchhod 2017) and wage inequality among employees (Bhorat et al. 2017).

The limited information about this sector is mostly due to a lack of suitable data. Labour market studies most commonly use data from national labour force or household surveys. However, these surveys rely on self- or proxy-reporting on income, sector, and the nature of the employment contract, leading to the well-known issue of high rates of missing data as well as errors in reporting (Juster and Smith 1997; Segal and Sullivan 1998; Riphahn and Serfling 2005). For example for South Africa, Wittenberg (2017) finds that the Quarterly Labour Force Survey (a household-level survey) under-reports income by around 40 percent on average compared to data from the tax records. Further, standard questions on the industry or nature of employment in these surveys do not allow TES or labour broker employment to be distinguished from other kinds of work.[3] In the administrative tax data released by SARS-NT, information on which firms belong to the TES sector is available because these firms are required to submit additional forms to the revenues services so that double-taxing of employees by the client firms is avoided. Finally, unlike in the available survey data, the administrative tax records differentiate between the basic wage and the value of benefits afforded to employees, which allows us to examine which component is largely responsible for the gap in earnings between TES and non-TES workers.

[3] To try and identify TES workers from South Africa's Quarterly Labour Force Surveys (QLFS), Benjamin et al. (2010) and Bhorat et al. (2016) used the standard industry classification code 889, *Business Activities Not Elsewhere Classified*, which falls under the broader category *Finance and Business Services*, and which includes 'labour recruitment and provision of staff; activities of employment agencies and recruiting organisations; hiring out of workers (labour broking activities)'. However, this code also lists another 10 activities which are not distinguishable from the labour broker sector, including employment in security services and debt collecting/credit rating agencies which have grown rapidly in South Africa over recent decades. In fact Budlender (2013) shows that a large percentage of the workers in this category had permanent contracts, leading her to conclude that 'while there is widespread agreement that a large number of workers are employed by temporary employment agencies in South Africa, and that the number has grown over time, there is similarly widespread agreement that the available numbers are estimates based on various assumptions rather than more reliable "counts" of the phenomenon' (Budlender 2013: 3).

Although the tax data do not contain many demographic or job characteristics, the panel nature of the data allows us to control for time and individual fixed effects. In other words, we can examine variation in wages for employees who switched between TES and non-TES jobs over the period of the panel. In addition, we examine the temporary employee wage differentials before and after the temporary employment spell. Temporary workers often accept such jobs due to factory closure or after being laid off and thus wage differentials may reflect the circumstances in which they accept the job, rather than the job itself (Segal and Sullivan 1998). Lastly, we examine to what extent the wage differential between TES and non-TES employment is driven by differences in the basic wage compared to differences in benefit contributions. Providing empirical evidence on the earnings differential between labour broker workers and other workers is an important first step to help inform debates on the role of this sector in the South African labour market as well in other developing countries.

In the following section we describe the data and definitions used in the analysis. Section 3 presents the descriptive analysis. Section 4 explains the methodology and Section 5 presents the results. Section 6 concludes.

2. Data and Definitions

Structure of the data

We use an employee panel dataset made available by SARS and the NT for the tax years 2011 (i.e. 1 March 2010 to end February 2011) to 2015 (1 March 2014 – end February 2015).[4] The dataset was created from employee income tax certificates submitted by employers (IRP5 and IT3(a)) to the South African Revenue Services (SARS). The unit of analysis is essentially at the job contract level as it includes records of employment for tax-paying firms over the period. However, the data can be

[4] The years in the IRP5 panel refer to the period 1 March of the previous year to the end of February of that year regardless of a firm's financial year. Pieterse, Kreuser, and Gavin (2016) show that 85 per cent of firms have their financial year at the end of February.

collapsed to the person level, as unique individual identifiers are available. Each IRP5 or IT3(a) submitting entity is identified through a Pay As You Earn (PAYE) reference number which can be linked to the Company Income Tax (CIT) records submitted to SARS for that entity. This allows employees to be matched to the firms they are employed in. (For a more detailed discussion of the structure of the data, see Pieterse, Kreuser, and Gavin 2016).

All employers must register with SARS within 21 business days after becoming an employer, unless none of the employees are liable for normal tax. Where no employee tax was deducted from remuneration and the employee receives R2000[5] or more per year, an IT3(a) form is provided to an employee. If an employee earns less than R2000 in a given tax year and no employee tax was deducted, the employee is not issued with an IRP5 or an IT3(a) form. IRP5 certificates of all employees in a company must be submitted within 60 days of the end of the tax year. The IRP5 and IT3(a) forms issued by employers are reconciliation forms that include information on the periods worked by the employee in the year of assessment, the total amount paid by that employer to the employee, as well as the amounts paid for taxes, the skills development levy[6], the unemployment insurance fund (UIF), and pension and medical aid. In addition to providing information on earnings, data from these forms can be used to identify a limited set of employee/job characteristics (namely, length of contract within the tax year, gender and age of employee) and firm characteristics (firm size and industry the firm operates in).

Importantly for the purposes of this research, the panel has a binary indicator which identifies whether or not firms belong to the TES or labour broker sector. Labour brokers are identified through an IRP30A form that they are expected to submit to SARS, which absolves the client firms from having to deduct tax from any payments made to a labour broker, as the labour broker is responsible for

[5] This is the equivalent of US$ 156.50 using an exchange rate of R12.78/$ for 2015.

[6] The levy is paid as a portion of an employer's salary bill to the revenue service. The levy is then distributed to encourage skills training and development.

paying tax on behalf of their employees. This eliminates the problem of misreporting of sector or type of employment common in household or employee surveys.

For our analysis, we make use of information on the individual's main job contract. About 80 percent of individuals in the data have just one job contract per year. However, for the rest, multiple entries per year can exist because individuals are either performing two jobs simultaneously or have sequential job contracts in the same year. Where individuals have overlapping job contracts at different firms, we identify the individual's primary job as the one with the highest earnings for that period.[7] Thus we end up with a sample of individuals with information at the job contract level, where each person may have a number of sequential job contracts per year (as long as the jobs are not overlapping).[8] Table 1 shows the number of individuals and job contracts in the final constructed *main job sample* for each year. Over the five-year panel, there are around 45 million individual observations and around 50.5 million job contract observations for the working age sample (16 to 65 years).

[7] Where individuals have overlapping job contracts *at the same firm*, we use the average earnings and average days for the overlapping contracts. Many of these overlapping contracts at the same firm have the same start and end dates and earnings information and are therefore likely to be duplicates. Where time period or earnings information differs, it is likely that they are IRP5 revisions. Revisions to the IRP5 could be submitted in the event of a mistake or a change to the employment duration. Unfortunately, we are unable to tell which version of the contract was revised and thus which is the most recent version, hence the averaging approach adopted (Chatterjee and Mcleod 2016).

[8] More detailed information on the precise cleaning and construction of the *main job sample* can be found in the published working paper version of this article *(reference suppressed, 2018)*. In this process, we closely followed the conventions used by others who have worked with the SARS-NT data.

Table 1: Description of Employee Panel, 2011 to 2015 (16-65 years)

Tax year	Job contracts	Individuals
2011	9 647 944	8 593 848
2012	10 087 428	8 900 761
2013	10 245 729	9 096 931
2014	10 194 275	9 135 393
2015	10 517 036	9 370 194
Total	50 692 412	45 097 127

Source: Authors' estimates based on IRP5 data.

Description of variables used

Job duration

Job duration is estimated as the days between the start date and the end date of the term of employment reported in the IRP5 or IT3(a) form. The variable is truncated at one year however. So for permanent employees, for example, the job contract length would be recorded as the maximum length of one tax year. As such, a '365 day contract' may refer to someone who is actually employed in a one-year contract or to someone employed for a duration of longer than a year in a particular job.

Earnings

Each IRP5 form reports *gross non-retirement fund income* (the salary paid to an individual from which contributions to medical aid and UIF are deducted), *non-taxable income* (which includes arbitration awards, purchased annuities, travel reimbursements, subsistence allowances, uniform allowances and other allowances) and *gross retirement income* (or pension contributions). The sum of these three variables provides *total earnings* for a specific job contract[9]. To estimate the earnings penalty, we use both *total earnings* and what we refer to as the *base salary* as dependent variables.

[9] For simplicity we use the term *total earnings*, but more specifically this variable represents *total gross earnings* as it still includes the tax portion.

The *base salary* is the gross non-retirement fund income (which already excludes pension contributions) less the contributions made to medical aid and UIF.[10]

We use monthly earnings for the analysis (as is done in Ebrahim, Leibbrandt, and Ranchhod 2017 and Chatterjee and Mcleod 2016). First, daily earnings are calculated using *total earnings* for a specific contract divided by the length of that contract (*job duration*). From this, monthly earnings are estimated by multiplying daily earnings by working days in a month.

Firm size

The IRP5 data does not include a variable indicating firm size and therefore this variable is imputed, taking into account that not all workers on a firm's payroll were employed for the entire year. Firm size is the total number of employees at the firm, weighted by the number of days an employee worked in a given year. Similar methods were employed in other studies using the IRP5 data (Pieterse, Kreuser, and Gavin 2016; Bhorat et al. 2017; Ebrahim, Leibbrandt, and Ranchhod 2017).

In addition, the IRP5 includes *date of birth* (used to calculate age) as well as *gender*. An industry variable, which is self-reported by the firm, is merged in from the CIT data matching on a firm's PAYE reference number.

Advantages and disadvantages of the data

There are clearly a number of advantages offered by the data. These include the larger sample size than in the labour force survey data; the longitudinal nature of the data that allows us to track individuals over time (and therefore control for individual fixed effects in identifying the wage penalty); more reliable reporting of income than in household surveys and information on benefit

[10] As has been done in other research using the SARS-NT panel, we trimmed the earnings data to remove observations where individuals earned more than R10 million per year as these are likely to be CEOs and directors of companies who are not comparable to TES sector workers. This excludes around 3000 non-TES sector contracts and 11 TES contracts. In addition, we removed those contracts where earnings were less than R2000 per year (or R167 per month) because they should not be included in the tax database. These are likely to be reporting errors or it is possible that a human resources employee unnecessarily included IRP5 forms for all workers despite the R2000 threshold. This results in a loss of around 1 million job contracts (around 1.6 percent of the original sample of 64 million observations), of which 204 000 are TES jobs.

contributions; and importantly for this work, the ability to accurately identify firms (and therefore employees) in the TES sector.

However, there are also a number of potential limitations. The dataset only contains tax registered firms, and among these, only the firms that actually completed a tax return in the relevant period. This means that employees of unregistered, small, very young or informal TES firms, which may be of interest in the South African context (as the employees in these firms may be the most vulnerable), have not been captured (Pieterse, Kreuser, and Gavin 2016). However, in terms of comparability when estimating the wage penalty for TES vs non-TES workers, of course low-wage workers or workers in informal firms in the non-TES sector are also excluded from the data.

Another limitation of the dataset is that there is no information on the number of hours worked per day in the job contract. This means any monthly wage difference between workers can be due to differences in the hourly wage or differences in the number of hours worked in a month, and we are unable to differentiate between these two factors.

Finally, TES workers are not differentiated from administrative staffing personnel working in the TES firm. This is unlikely to be a significant problem, however, given that staffing personnel tend to constitute a very small proportion of total employment in the firm (Kvasnicka 2008).

3. Descriptive Statistics

Employment Trends

Table 2 presents employment in the TES and non-TES sectors at the job contract and individual levels. TES employment constituted between 4 and 5 percent of total employment between 2011 and 2015. This is true if we consider individuals employed in the TES sector as a percentage of all employed individuals, or TES job contracts as a percentage of total job contracts. While TES employment as a percentage of total employment increased and then stabilised between 2011 and 2014, the percentage declined in 2015. In absolute terms, the number of TES employees grew between 2011 and 2013 and then fell to 2012 levels by 2015, while non-TES employment continued to grow. Figure 1 shows clearly how the growth rates diverged in the final year, which may be related to employers pre-empting the amendments to the LRA which were introduced in January 2015 and which made the conditions around temporary hire more stringent.

Table 2: Employment estimates in the TES and non-TES sectors

Tax year	Job contracts			Individuals		
	TES	Non-TES	Share	TES	Non-TES	Share
2011	414 338	9 233 606	4.29%	400 584	8 193 264	4.66%
2012	454 936	9 632 492	4.51%	438 140	8 462 621	4.92%
2013	477 932	9 767 797	4.66%	459 606	8 637 325	5.05%
2014	476 469	9 717 806	4.67%	459 840	8 675 553	5.03%
2015	452 070	10 064 966	4.30%	436 323	8 933 871	4.66%

Source: Authors' estimates based on IRP5 data.
Note: This is the "main job" sample as defined in Section 2.

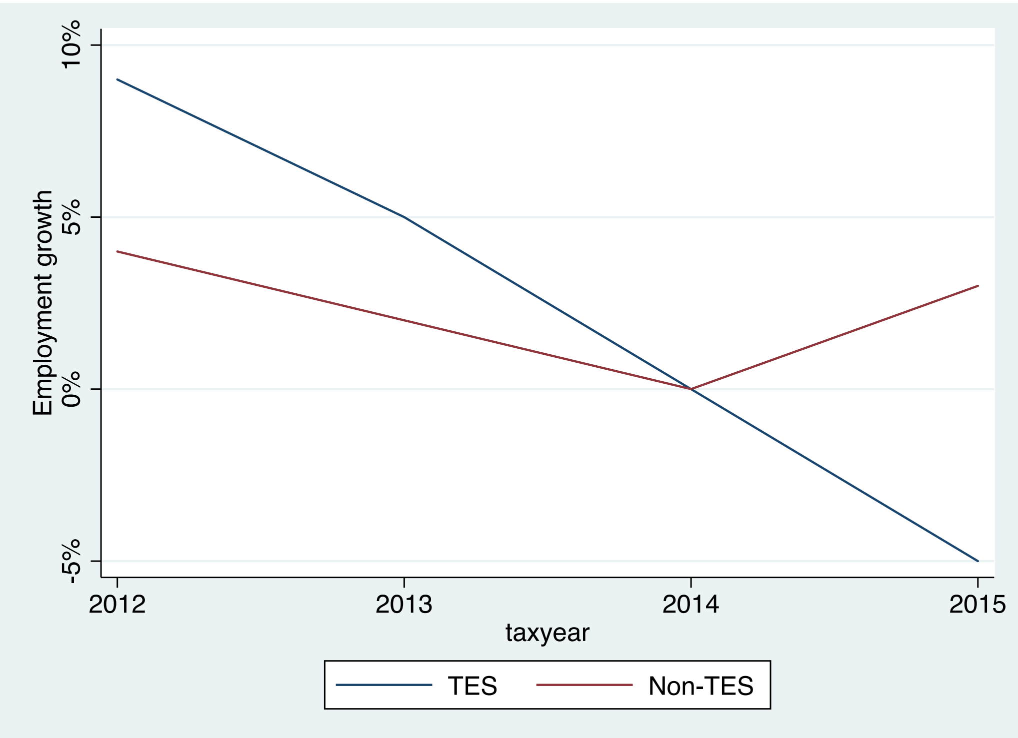

Figure 1: Growth rates in TES and non-TES employment

Source: Authors' estimates based on IRP5 data.
Note: Uses the "main job" sample as defined in Section 2 at the individual level.

Table 3 presents descriptive statistics for TES and non-TES job contracts for the year 2014.[11] TES employees are younger than non-TES employees with around half of all TES job contracts filled by individuals aged between 16 and 29 years relative to 32 percent of non-TES contracts. This finding further motivates why we need to better understand this sector, as it may play a key role in absorbing young people into employment, especially in the context of a youth unemployment rate of around 39 percent in South Africa.[12] Males dominate the TES sector with around two-thirds of job contracts filled by male employees relative to 56 percent of job contracts in the non-TES sector. The vast majority of TES contracts, 74 percent, are less than 12 months. The most common job contract length for the TES sector is more than 6 months but less than a year (39 percent). In contrast, for non-TES employment, the most common job contract length is a year or more (53 percent).

In terms of firm size, the majority of TES employment, 73 percent, is in TES firms that have more than 1000 employees, whereas only 39 percent of non-TES employment is in very large firms of more than 1000 employees. TES firms are concentrated in the Finance and Business Services sector (84 percent) followed by the Construction sector (4 percent). These are also the sectors where employment growth has been observed over the last two decades according to QLFS data (Bhorat, Cassim, and Yu 2016). As we would expect, non-TES firms are more widely spread across the different industrial categories. Overall, the descriptive characteristics indicate that, relative to non-TES employment, TES employment is more likely to be held by young, male employees, employed on short contracts (of less than a year) and in firms with more than a thousand employees.

[11] Employment (and therefore employee characteristics) in 2015 may have been affected by the LRA amendments if there was a disemployment effect. For this reason, we use 2014 data here for illustrative purposes.

[12] This estimate is based on data from the QLFS, Quarter 1, 2017, and uses the narrow or 'searching' definition of unemployment.

Table 3: Characteristics of TES and non-TES employment (2014)

	TES		Non-TES	
	Proportion	***N***	***Proportion***	***N***
Age				
16-29	50.45%	233 125	31.94%	2 962 962
30-39	30.09%	139 075	29.92%	2 775 132
40-49	12.50%	57 759	20.82%	1 931 280
50-65	6.96%	32 170	17.32%	1 606 752
Total	*100%*	*462 129*	100%	*9 276 126*
Gender				
Female	32.36%	150054	43.78%	3 983 691
Male	67.64%	313641	56.22%	5 116 353
Total	*100%*	*463695*	*100%*	*9 100 044*
Contract duration				
less than 15 days	3.13%	14 843	1.75%	164 617
15 to 30 days	4.44%	21 040	2.52%	236 790
1 to 3 months	12.64%	59 938	8.79%	826 120
3 to 6 months	14.83%	70 295	10.98%	1 031 592
6 months to less than a year	38.94%	184 636	23.47%	2 205 659
A year or more	26.03%	123 409	52.49%	4 932 108
Total	*100%*	*474 161*	100%	*9 396 886*
Firm Size				
Small (0-50)	1.82%	8 693	26.28%	2 553 459
Medium (51-250)	6.49%	30 946	19.47%	1 891 747
Large (251-1000)	18.55%	88 390	15.34%	1 490 886
Very large (more than 1000)	73.13%	348 440	38.92%	3 781 714
Total	100%	476 469	100%	9 717 806
Industry				
Agriculture	1.53%	7 293	8.58%	827 997
Mining	1.12%	5 340	4.27%	412 415
Manufacturing	3.08%	14 661	16.79%	1 620 096
Utilities	0.08%	377	1.27%	122 366
Construction	4.34%	20 664	3.58%	345 562
Trade	2.23%	10 612	12.13%	1 169 929
Transport	0.76%	3 634	4.23%	408 106
Tourism	0.06%	285	2.78%	268 496
Financial	83.73%	398 929	25.73%	2 482 196
Government	0.00%	0	13.39%	1 291 725
Non-Government Community Services	3.08%	14 655	7.24%	698 292
Total	100.00%	*476 450*	*100.00%*	*9 647 180*

Source: Authors' estimates based on IRP5 data.

Note: This is the "main job" sample as defined in Section 2 and is at the job contract level.

Wage differentials

Kernel densities of the log of monthly wages for TES and non-TES jobs in 2014 (Figure 2), show that the non-TES earnings distribution sits to the right of the TES earnings distribution as expected, and has a much longer upper tail. Table 4 displays mean total and base earnings in TES and non-TES job contracts, as well as the ratio of TES to non-TES earnings at the mean, the 25th, 50th and 75th percentiles. Using *total* earnings, TES wages are 50 percent of non-TES wages at the mean and 59 percent at the median. The wage differential is lower at the bottom of the earnings distribution, with TES wages around 67 percent of non-TES wages at the 25th percentile, but 43 percent at the 75th percentile. Wage penalties are substantially lower when *base*[13] earnings are used, with TES wages now 74 percent and 88 percent of non-TES wages at the mean and median respectively. This indicates that benefits such as retirement and medical aid contributions are responsible for a large part of the wage differential between the TES and non-TES sectors.

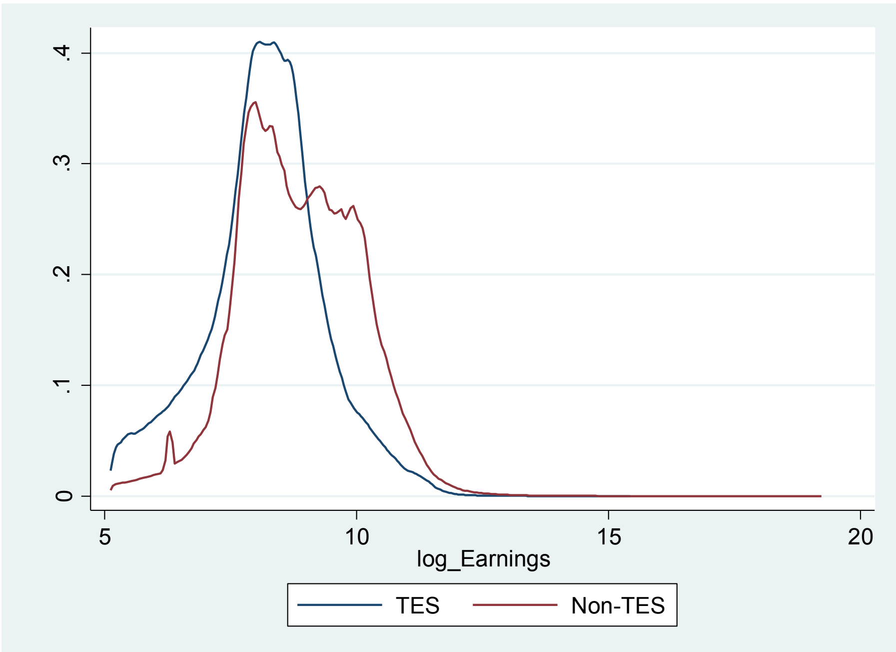

Figure 2: Earnings kernel density, 2014

Source: Authors' estimates based on IRP5 data.
Note: This is the "main job" sample as defined in Section 2 and is at the job contract level.

[13] As explained above, this is gross non-retirement fund income (i.e. income excluding the pension) less contributions to medical aid and UIF.

Table 4: Monthly Total Earnings for TES and Non-TES Jobs (2014)

	Mean monthly values[a]		Ratio TES/Non-TES			
	TES (ZAR)	Non-TES (ZAR)	Mean	p25	p50	p75
Total Earnings	7 215.63	14 417.72	0.5	0.67	0.59	0.43
Base Earnings	6 212.60	8 353.37	0.74	0.84	0.88	0.83

Source: Authors' estimates based on IRP5 data.

Note: This is the "main job" sample as defined in Section 2 and is at the job contract level.

[a] The average US$-ZAR exchange rate for 2014 was R10.86/US$.

The gap between total and base earnings between sectors is particularly large at the upper end of the distribution. This is shown more clearly in Figure 3 which presents the ratio of base to total earnings by income category. Below R2000 a month, workers (regardless of sector) receive minimal benefits and the ratio of base to total earnings is close to 1. Thereafter, we see greater divergence in the base to total earnings ratio between the TES and non-TES sectors. For monthly earnings above R15 000, for example, we see the non-TES base to total earnings ratio ranging from 0.5 to 0.6, while for the TES sector the ratio is always above 0.8.

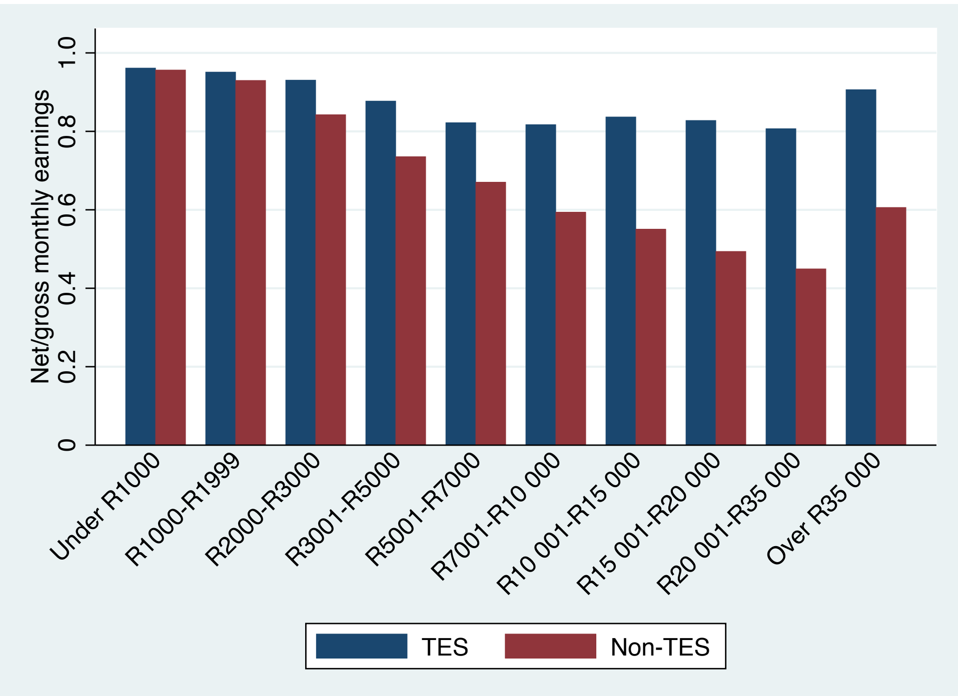

Figure 3: Ratio of Base/Total Earnings for TES and non-TES sector by Income Category, 2014

Source: Authors' estimates based on IRP5 data.

While these results provide a first insight into the wage penalties for TES workers, of course TES workers may be different from non-TES workers in terms of skill or human capital, or the nature of TES jobs may be different from non-TES jobs. We describe the empirical strategy to account for these differences in the next section.

4. Econometric strategy

The studies that have examined the temporary employment services wage penalty in other countries have used a variety of methods depending on the data available. Combining firm and labour force survey data, Tohario and Serrano (1993) estimate an ordinary least squares (OLS) regression and find a wage penalty of 8.5 to 10.8 percent in Spain. Blanchard and Landier (2002) use an employment survey and identify a wage gap of 20 percent in France using pooled ordinary least squares (POLS). In Britain, Booth, Francesconi and Frank (2002) make use of household survey data and find a wage gap of between 13 and 15 percent when using POLS and a wage gap of between 6 and 10 percent when using fixed effects, suggesting that not accounting for the impact of time-invariant factors results in an overestimation of wage penalties. Using household survey data and an instrumental variable approach, Picchio (2006) finds a wage penalty of around percent 13 percent in Italy. Hagen (2002) employs matching estimators and a dummy endogenous variable model controlling for self-selection, and finds a penalty of 23 percent using the German socio-economic survey. In the U.S., Segal and Sullivan (1998) use administrative employee data controlling for worker and time fixed effects and find a wage gap of 15 to 20 percent.

Given the lack of human capital variables and other individual and job characteristics in the SARS-NT data, we rely on the panel nature of the data to estimate the wage penalty (as in Segal and Sullivan 1998, who had administrative data structured in a similar way to ours). We use a fixed effects strategy which controls for time-invariant individual-specific effects at the employee level, where the variation in the earnings of individuals that switch into and out of TES employment over time is exploited. To put this into context, in Table 5 we examine transitions between the TES and non-TES sectors in consecutive years for those individuals that have one job contract per year (80 percent of the main jobs sample). While using a subset of data where individuals have just one job contract per year may underestimate the number of switches, it still gives an indication of the movement between sectors. Of those individuals that had a TES job in 2011, 147 707 (85 percent) stayed in the TES sector in 2012 while 27 007 (15 percent) moved into the non-TES sector. Of those that were in the non-TES sector in 2011, the majority remained in the non-TES sector, with 29 418 moving into the TES sector (this accounts for less than 1 percent of the non-TES sector). The percentages transitioning into and out of the TES sector are similar across the years except for the final year, with the percentage of workers transitioning out of the TES sector rising by about 2 percentage points between 2014 and 2015. Again, this could be related to the amendments to the LRA.

Table 5: Transitions matrices for consecutive years over the panel (2011 – 2015)

	Share (%)	Number	Share (%)	Number	Share (%)	Number
	TES 2012		non-TES 2012		Total	
TES 2011	84.54	147 707	15.46	27 007	100.00	174 714
non-TES 2011	0.56	29 418	99.44	5 203 347	100.00	5 232 765
	TES 2013		non-TES 2013		Total	
TES 2012	84.45	159 773	15.55	29 430	100.00	189 203
non-TES 2012	0.53	28 570	99.47	5 390 911	100.00	5 419 481
	TES 2014		non-TES 2014		Total	
TES 2013	84.75	167 180	15.25	30 077	100.00	197 257
non-TES 2013	0.53	29886	99.47	5 620 918	100.00	5 650 804
	TES 2015		non-TES 2015		Total	
TES 2014	82.41	163 342	17.59	34 872	100.00	198 214
non-TES 2014	0.44	25 373	99.56	5 761 315	100.00	5 786 816

Source: Authors' estimates based on IRP5 data. The unit of analysis is the individual.
Notes: The table only includes individuals who have stayed in the panel for every year therefore the totals will differ to Table 2. Around 10 million observations were dropped.

We describe the various specifications we estimate below, closely following the formulation in Segal and Sullivan (1998), although modified to reflect our own data structure. We begin by estimating a simple POLS model that treats the data as if it were cross-sectional:

$$Y_{ij} = \lambda TES_{ij} + \varepsilon_{ij} \quad (1)$$

where Y_{ij} is the log of real monthly earnings for individual i in job j; TES_{ij} is an indicator for whether or not the individual is in a TES job, λ is the TES earnings penalty, and ε_{ij} is the error term.

This model is unlikely to capture the true wage differential of course, as temporary workers are likely to be different from non-temporary workers. Therefore, we control for the time-invariant characteristics of employees (such as race, gender, education, etc) using a standard fixed effects model and including year dummies to control for time fixed-effects:

$$Y_{ij} = \alpha_i + \beta_t + \lambda TES_{ij} + \varepsilon_{ij} \tag{2}$$

where β_t are the fixed effects for each year and control for annual wage growth; and α_i are the individual-specific constants and control for the time-invariant characteristics of TES and non-TES workers.

Although we have very few variables in the SARS-NT dataset, in the next specifications we include controls for the time-varying factors that we do have information on. We include employee age in the form of three age dummies (as a proxy for experience):

$$Y_{ij} = \alpha_i + \beta_t + \lambda TES_{ij} + Age\_30to39_{ij} + Age\_40to49_{ij} + Age\_50to65_{ij} + \varepsilon_{ij} \tag{3}$$

Further, we include a vector of job/firm characteristics (X_{ij}), namely, job contract duration, size of the firm and industry. This model recognises that part of the TES wage penalty might be due to differences in the nature of the job itself or the type of firm it is located in.

$$Y_{ij} = \alpha_i + \beta_t + \lambda TES_{ij} + Age\_30to39_i + Age\_40to49_i + Age\_50to65_i + X_{ij} + \varepsilon_{ij} \tag{4}$$

Lastly, we examine TES workers' wages before and after their temporary employment spell. The reason for this, as Segal and Sullivan (1998) point out, is that temporary workers might accept a temporary job because of some setback such as a factory closure or after being laid off, and thus wage differentials may reflect the circumstances in which workers accept the job, rather than the job itself. If this is the case, the earnings received in periods far removed from the temporary employment spell may not be a good comparison but those immediately prior to the temporary spell will be. To explore this further, the approach in Segal and Sullivan (1998) is followed and dummy variables that reflect the jobs before and after the temporary employment spell are included. As they did, for the sake of simplicity we exclude individuals that had more than one temporary employment spell over the period, so that our sample of individuals in TES employment were employed in non-TES jobs before and after the temporary employment spell. Equation (5) includes a set of dummies where $1Before_{ij}$ indicates the (non-TES) job prior to the temporary employment spell and $2Before_{ij}$ indicates the job two jobs prior to the temporary employment spell. For example, $1Before_{ij} = 1$ for the first job prior to the temporary employment spell and 0 for all other jobs held by the individual, and $2Before_{ij}$=1 for two jobs prior to the temporary spell and 0 for other jobs held by the individual. The set of dummies $1After_{ij}$ and $2After_{ij}$ is similarly included to represent the first and second jobs after the temporary employment spell. The coefficients on the before and after dummies measure the earnings penalty in the jobs before and after the temporary employment spell.

$$Y_{ij} = \alpha_i + \beta_t + TES^k{}_{ij}\lambda + Age\_30to39_{ij} + Age\_40to49_{ij} + Age\_50to65_{ij} + 1Before_{ij} + 2Before_{ij} + 1After_{ij} + 2After_{ij} + X_{ij} + \varepsilon_{ij} \qquad (5)$$

Segal and Sullivan (1998) find that wage differentials are negative before the TES spell which they suggest is associated with the circumstances leading to workers having lower wages even before entering a TES spell.

5. Results

Table 6 presents the econometric results for the equations outlined above, where *total earnings* is the dependent variable. The coefficient on the TES variable in the simplest POLS specification (1) is -0.656 indicating a wage penalty of 48.11 percent. When we control for individual fixed effects (in 2A), the coefficient on TES declines substantially to -0.394 equivalent to a wage penalty of 32.57 percent. This is unsurprising, as we would expect a large difference in the time-invariant characteristics between TES and non-TES workers. In specification 2B, in addition to the individual-specific fixed effects, we also include year dummies to control for time-specific effects. The coefficient hardly changes at -0.383 (a wage penalty of around 31.82 percent), suggesting that year effects do not have a substantial bearing on real wage penalties.

To control for work experience, as per equation 3, we include age dummies. The coefficient on the TES dummy declines marginally to -0.382 (a penalty of 31.61 percent). The results suggest, as expected, that relative to the 16 to 29 age cohort, older workers earn more (with the quadratic effect evident from the lower coefficient for the 50-65 age group compared to the 40-49 age group). Interestingly, when controls for job contract duration, firm size and industry are included progressively in specifications 4A, 4B and 4C, the change in the wage penalty is relatively small. The coefficient on the TES variable in the final specification is -0.377 which is equivalent to a wage penalty of 31.41 percent. The coefficients on the firm size dummies are all negative and significant, indicating that, compared to small firms, wages are on average lower in firms with a larger number of employees. The contract duration dummies are positive and significant for the first two categories (less than 15 days and between 15 to 30 days), suggesting that workers in contract lengths of very short duration earn more on average compared to those in contracts of one year or more (the omitted category). However, those in contracts of more than 30 days but less than a year earn less on average compared to workers in contracts of a year or longer. Except for the trade, tourism, non-government community services and financial services sectors, the coefficients on the other industry categories are all positive and significant, indicating higher wages relative to the agricultural sector.[14]

[14] In addition, we ran the regressions with a panel including only individuals with one job contract per year (47 625 823 observations compared to 58 488 963 in Table 6), to see if those who switched frequently within years were driving the results. However, the coefficients ranged from -0.685 to -0.317, only slightly lower than what is observed in Table 6.

Table 6: Estimating the TES wage penalty (Dependent variable: log of monthly total earnings)

	1	2A	2B	3	4 A	4 B	4 C
TES	-0.656***	-0.394***	-0.383***	-0.382***	-0.380***	-0.384***	-0.377***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2012			0.051***	0.047***	0.047***	0.050***	0.050***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2013			0.108***	0.100***	0.100***	0.102***	0.102***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2014			0.184***	0.171***	0.171***	0.172***	0.170***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
2015			0.247***	0.230***	0.230***	0.229***	0.227***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Age: 30- 39				0.100***	0.100***	0.097***	0.099***
				(0.001)	(0.001)	(0.001)	(0.001)
Age 40 - 49				0.136***	0.136***	0.131***	0.131***
				(0.001)	(0.001)	(0.001)	(0.001)
Age 50 -65				0.114***	0.114***	0.109***	0.103***
				(0.001)	(0.001)	(0.001)	(0.001)
Medium (50-250)					-0.013***	-0.014***	-0.014***
					(0.000)	(0.000)	(0.000)
Large (250-1000)					-0.014***	-0.014***	-0.016***
					(0.001)	(0.001)	(0.001)
Very large					-0.014***	-0.022***	-0.025***
					(0.000)	(0.000)	(0.000)
Less than 15 days						0.643***	0.648***
						(0.001)	(0.001)
15 to 30 days						0.004***	0.005***
						(0.001)	(0.001)
30 to 60 days						-0.083***	-0.082***
						(0.000)	(0.000)
3 to 6 months						-0.092***	-0.091***
						(0.000)	(0.000)
6 months to less than 1 year						-0.084***	-0.083***
						(0.000)	(0.000)
Mining							0.089***
							(0.001)
Manufacturing							0.020***
							(0.001)
Utilities							0.069***
							(0.002)
Construction							0.014***
							(0.001)
Trade							-0.030***
							(0.001)
Transport							0.051***
							(0.001)
Tourism							-0.032***
							(0.001)
Financial							-0.023***
							(0.001)
Government							0.046***
							(0.001)
Non-Govt Community Services							-0.086***
							(0.001)
Constant	8.931***	8.918***	8.797***	8.728***	8.738***	8.770***	8.772***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
N	58 488 963	58 488 963	58 488 963	58 488 963	58 488 963	58 488,963	58 488 963

Notes: 1. The dependent variable is the log of monthly total earnings, deflated such that 2015 is the base year.
2. The 2011 financial year, agriculture, small firms and contracts of a year or more are the omitted categories.
* p<=0.1 ** p<=0.05 *** p<=0.01

Table 7 shows the same set of estimations as in Table 6, but using the *base salary* as the dependent variable, i.e. gross income net of retirement fund, medical aid and UIF contributions. We find that the earnings differentials are much lower compared to when total earnings were used as the dependent variable. The coefficient in specification 1 from the POLS estimation is -0.274 (a wage penalty of 23.97 percent) versus a coefficient of -0.656 (a wage penalty of 48.11 percent) from Table 6. In the final specification 4C (fixed effects including all controls), the coefficient on the TES dummy is -0.068 (a wage penalty of 6.57 percent) versus a coefficient of -0.377 (a wage penalty of 31.41 percent) in Table 6. This suggests that, on average, the TES wage penalty is driven to a large extent by the benefit contributions afforded to those in the non-TES sector.

Table 7: Estimating the TES wage penalty (Dependent variable: log of monthly base salary)

	1	2A	2B	3	4 A	4 B	4 C
TES	-0.274***	-0.149***	-0.141***	-0.140***	-0.049***	-0.061***	-0.068***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2012			0.044***	0.042***	0.044***	0.044***	0.044***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2013			0.087***	0.083***	0.085***	0.089***	0.089***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2014			0.140***	0.133***	0.135***	0.139***	0.180***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
2015			0.183***	0.174***	0.177***	0.185***	0.225***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Age: 30- 39				0.056***	0.059***	0.065***	0.065***
				(0.001)	(0.001)	(0.001)	(0.001)
Age 40 - 49				0.077***	0.082***	0.091***	0.090***
				(0.001)	(0.001)	(0.001)	(0.001)
Age 50 -65				0.060***	0.067***	0.075***	0.071***
				(0.001)	(0.001)	(0.001)	(0.001)
Medium (50-250)					-0.184***	-0.180***	-0.180***
					(0.001)	(0.001)	(0.001)
Large (250-1000)					-0.288***	-0.276***	-0.278***
					(0.001)	(0.001)	(0.001)
Very large					-0.400***	-0.384***	-0.385***
					(0.001)	(0.001)	(0.001)
Less than 15 days						1.192***	1.189***
						(0.001)	(0.001)
15 to 30 days						0.275***	0.275***
						(0.001)	(0.001)
30 to 60 days						0.185***	0.186***
						(0.001)	(0.001)
3 to 6 months						0.098***	0.098***
						(0.000)	(0.000)
6 months to less than 1 year						0.027***	0.028***
						(0.000)	(0.000)
Mining							0.003**
							(0.001)
Manufacturing							-0.039***
							(0.001)
Utilities							0.082***
							(0.002)
Construction							0.016***
							(0.001)
Trade							-0.162***
							(0.001)
Transport							-0.014***
							(0.001)
Tourism							-0.100***
							(0.002)
Financial							-0.014***
							(0.001)

Government							-0.025***
							(0.001)
Non-Govt Community Services							-0.102***
							(0.001)
_cons	8.299***	8.293***	8.199***	8.161***	8.403***	8.324***	8.326***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
N	56 955 731	56 955 731	56 955 731	56 955 731	56 955 731	56 955 731	56 955 731

Notes: 1. The dependent variable is the log of the monthly base salary, deflated such that 2015 is the base year.
2. The 2011 financial year, agriculture, small firms and contracts of a year or more are the omitted categories.
3. The sample size is not the same as in Table 6 due to missing data on gross non-retirement fund income.
* p<=0.1 ** p<=0.05 *** p<=0.01.

Finally, Table 8 presents the estimation of equation 5 where dummies associated with the two jobs before and after entering the TES sector are included. As explained above, we exclude those who had more than one TES job spell in the panel.[15] For comparison we first rerun equation 4C, i.e the specification with time dummies, individual fixed effects and a full set of controls, using this reduced sample (shown in Column 1 of Table 8). The coefficient on TES employment for this reduced sample is only slightly larger than for the full sample used above in Table 6 (-0.387 vs -0.377). However, of interest are the coefficients on the dummy variables representing the jobs before and after the temporary employment spell shown in Column 2. The coefficients on the dummies representing non-TES jobs before the temporary employment spell are negative, suggesting that periods prior to entering into a TES contract are associated with events leading to workers having lower wages even before they joined a TES firm (as per Segal and Sullivan). The coefficient on the dummy '1 job prior to the TES spell' of -0.305 (which is equivalent to a 26.28 percent penalty) is larger than the coefficient on the dummy '2 jobs prior to the TES spell' of -0.131 (which is equivalent to a 12.28 percent penalty). The negative coefficients on the dummies for the jobs after the temporary employment spell (-0.068 and -0.006 for one and two jobs post the TES spell respectively)show that the wage penalty is far smaller in the period after the TES spell and tends to decline for each successive job. The coefficient on the TES dummy (-0.494) in Column 2 is larger than in Column 1 (-0.387) because the jobs just before and just after the TES spell, during which wages tend to be lower than for the periods outside the 'two job prior and two job post' window, are removed from the non-

[15] Around 10 million observations or 17 percent of the sample from Table 6 were dropped.

TES comparison group. The largest differential is still observed in the period associated with being in a TES firm.

Table 8: Econometric results including before and after effects

	4C	5
TES	-0.387***	-0.494***
	(0.001)	(0.001)
Age: 30- 39	0.096***	0.095***
	(0.001)	(0.001)
Age 40 – 49	0.126***	0.126***
	(0.001)	(0.001)
Age 50 -65	0.097***	0.099***
	(0.001)	(0.001)
2012	0.050***	0.049***
	(0.000)	(0.000)
2013	0.101***	0.100***
	(0.000)	(0.000)
2014	0.171***	0.170***
	(0.001)	(0.001)
2015	0.228***	0.224***
	(0.001)	(0.001)
Medium (50-250)	-0.015***	-0.015***
	(0.000)	(0.000)
Large (250-1000)	-0.015***	-0.016***
	(0.001)	(0.001)
Very large (1000+)	-0.025***	-0.026***
	(0.000)	(0.000)
Less than 15 days	0.649***	0.653***
	(0.001)	(0.001)
15 to 30 days	0.001*	0.004***
	(0.001)	(0.001)
30 to 60 days	-0.084***	-0.080***
	(0.000)	(0.000)
3 to 6 months	-0.091***	-0.087***
	(0.000)	(0.000)
6 months to less than 1 year	-0.082***	-0.080***
	(0.000)	(0.000)
Mining	0.087***	0.087***
	(0.001)	(0.001)
Manufacturing	0.019***	0.017***
	(0.001)	(0.001)
Utilities	0.068***	0.067***
	(0.002)	(0.002)
Construction	0.011***	0.008***
	(0.001)	(0.001)
Trade	-0.031***	-0.031***
	(0.001)	(0.001)
Transport	0.050***	0.047***
	(0.001)	(0.001)
Tourism	-0.033***	-0.033***
	(0.001)	(0.001)
Financial	-0.024***	-0.028***
	(0.001)	(0.001)
Government	0.044***	0.045***
	(0.001)	(0.001)
Non-Govt Community Services	-0.087***	-0.088***
	(0.001)	(0.001)
2 jobs prior		-0.131***
		(0.001)
1 job prior		-0.305***

		(0.001)
1 job post		-0.068***
		(0.001)
2 jobs post		-0.006***
		(0.001)
Constant	8.775***	8.785***
	(0.001)	(0.001)
Fixed effects	Yes	Yes
N	48,172,843	48,172,843

Notes: 1. The dependent variable is the log of monthly total earnings, deflated such that 2015 is the base year.
2. The 2011 financial year, agriculture, small firms and contracts of a year or more are the omitted categories.
* p<=0.1 ** p<=0.05 *** p<=0.01

6. Concluding discussion

In this paper, we estimate the wage penalty associated with being in the TES or labour broker sector in South Africa, using recently released administrative tax data for 2011 to 2015. We find a large penalty associated with TES employment, even after various controls are introduced. The raw total earnings penalty of close to 50 percent diminishes substantially (by 15 percentage points or a third of its original size) when controlling for individual fixed effects, suggesting that TES and non-TES workers have different time-invariant characteristics. The penalty declines slightly further when controlling for year effects and the time-varying characteristics available in the data, namely, age, job contract duration, firm size and industry. Nonetheless, even in our fullest specification, comparing wages during a TES job spell relative to wages at other times in someone's career suggests a wage penalty of around 30 percent when using total earnings. However, some of this effect appears to be due to factors associated with the circumstances of the worker rather than the job itself, as there is a penalty, albeit a smaller one, also on the non-TES jobs just prior to the temporary job spell.

The penalty of around 30 percent found using the data for South Africa is higher than that found in the international literature cited in this paper, where the maximum wage penalty was 23 percent. However, the results are not directly comparable, as most of the work uses household, labour or firm surveys in which the data and thus the controls available are substantially different to those available in administrative employee data. The paper which uses data and methods most similar to ours is Segal and Sullivan (1998), which used administrative data with a limited set of variables to estimate the TES wage penalty for the U.S. They found an hourly wage differential of 15 to 20 percent, which is still lower than what was found in this study.

We also found that a large part of the TES wage penalty - 24 percentage points or close to 80 percent of its original size - is due to differences in the benefit contributions (for pension, medical aid and UIF) for TES and non-TES workers. The penalty declines to 6 percent when using the base salary, rather than total earnings, as the dependent variable. The descriptive statistics suggested that the benefit gap is much higher at the upper end of the income spectrum, whereas at the lower end, workers in both sectors receive few such benefits.

It is possible that the size of the penalty might fall further if we were able to control for additional factors. While we use a fixed effects estimation strategy to control for time-invariant characteristics at the individual level, we have not been able to control for an extensive set of time-varying individual or job characteristics. Controlling for occupation, skill level or union coverage, for example, might affect the results, as literature elsewhere has shown these are also important determinants of earnings (Booth et al. 2000). Further, since we do not have data on hours worked, we cannot tell whether the earnings differential is related to differences in the actual hourly wage versus the number of hours worked.

Despite these limitations, the administrative tax dataset at least provides a first opportunity to explore the labour broker wage penalty in South Africa, using a reliable identifier for the sector. More broadly, this paper makes an empirical contribution to the study of a sector that is growing, but still relatively under-examined in developing countries. This paper has also provided an example of how administrative data can be used to highlight policy-relevant issues. As more waves of the data become available, there will be further opportunity to explore this sector. In particular, in future work, it will be important to explore whether there has been a trade-off between the protection of temporary employees and employment, by examining the potential disemployment effects of the amendments to the LRA of 2015.

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