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The Impact of Employment Protection on the Temporary Employment Services Sector in South Africa

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ABSTRACT

This paper uses administrative tax data to examine the impact of strengthening employment protection legislation in the temporary employment services (TES) sector in South Africa. The study uses a regression discontinuity design to investigate whether the 2015 amendments to the LRA had a causal impact on the employment, wages and contract duration of workers employed in the sector. The findings suggest limited impacts in the short-term. There were no significant effects on worker transitions, and improved working conditions only for the relatively small group that did move out of the TES sector into the non-TES sector after the reform.

JEL Classification: J31, J41

1 | Introduction

In recent decades, there has been significant growth in atypical work, which includes amongst other forms, firms hiring temporary workers directly on fixed-term contracts and firms leasing workers through temporary employment services (TES) agencies. The increase in the use of atypical work by firms has been fuelled by the need for flexibility and cost-cutting in a rapidly changing and competitive global environment. This practice has been exacerbated by strict employment protection legislation (EPL) for permanent workers (or workers in standard employment relationships) alongside historically fewer protective measures for atypical or temporary workers (Autor 2003; Daruich et al. 2022; Dolado et al. 2002; Zhou 2006).

An extensive literature has explored the effects of this duality in labour market legislation. While atypical or temporary work arrangements give firms flexibility, can improve profit margins and may reduce unemployment spells amongst workers themselves, there are concerns that instead of temporary work providing a stepping stone to more stable permanent jobs, workers can become trapped in a series of (potentially exploitative) short-term jobs, with detrimental effects on their acquisition of human capital and lifetime earnings (Bentolila et al. 2012; Daruich et al. 2022; Hijzen et al. 2017; Garcia-Louzao et al. 2022; Nunez and Livanos 2015).[1]

In recent years, the trend in some countries, particularly in Europe, has been to strengthen EPL for workers in temporary or fixed-term jobs, with the aim of reducing labour market duality. A number of studies have attempted to evaluate these reforms (see, e.g., Bottasso et al. 2023 on Italy; Cahuc et al. 2020 on France; and Cahuc et al. 2023 on Portugal). The general finding is that while temporary jobs are reduced, there is not a concurrent increase in permanent jobs, leading to an overall reduction in employment, with the negative effects stronger for unskilled workers. The trade-off between protecting temporary workers and the potential disemployment effects of stricter employment legislation for temporary work, therefore makes these decisions politically difficult.

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Research in this area has focussed on workers in fixed-term contracts employed directly by firms or on temporary work as a whole. There is limited research on temporary workers employed through agencies or TES workers (Nannicini 2006; Neugart and Storrie 2006; Baumann et al. 2011; Drenik et al. 2023), and specifically on the impacts of strengthening employment protection for this group of workers, as occurred in South Africa in 2015. TES workers are employed by staffing agencies, where these agencies are ultimately responsible for the salary, taxes and benefits of the leased employee. When a company, referred to as the client firm, contracts with a staffing agency for temporary help, it pays the staffing agency a set fee for the leased worker. TES workers can therefore be distinguished from other types of atypical workers such as those on fixed-term contracts or in part-time work, as these workers are typically the employees of the company that hired them directly.

The theoretical literature on the impact of stricter EPL governing temporary agency workers specifically is still underdeveloped. However, impacts are likely to depend on whether the regulations impose additional costs on the TES agency or the client firm, how these compare to the costs of hiring workers directly on fixed-term contracts or in permanent jobs, as well as on the institutional and labour market context (Baumann et al. 2011; Nunziata and Staffolani 2007). As Baumann et al. (2011) note, the impacts on TES employment are theoretically ambiguous and therefore become a matter for empirical testing.

In South Africa, the leasing of temporary workers through TES firms, commonly referred to as labour brokering, has been a feature of the labour market since the 1800s in the mining industry in particular. In 1994, with the country's transition to democracy, labour legislation was redrafted to try and level the bargaining power between workers and employers. However, legislative reform at the time focused on workers in standard employment relationships (SERs), largely leaving atypical work relationships, including TES sector workers, unprotected. Following these reforms, there was a surge in the use of labour brokering, with employers making use of this flexible working relationship to circumvent some aspects of the new labour legislation governing SERs (Theron 2014).[2] Concerns were raised in a number of quarters about the growth of TES employment, as there was a perception that wages paid to workers by TES agencies were low and that the sector was exploitative, avoiding paying fair wages and benefits while keeping workers on rolling short-term contracts (Benjamin 2013).[3]

It was only in 2015, after considerable strike action organised by TES workers as well as calls by organised labour to ban labour brokers, that the South African government enacted legislation to increase employment protection for atypical workers, including part-time, fixed-term and TES workers. For TES workers, the new regulations required that temporary work be limited to contract lengths of 3 months, after which the client firm would be 'deemed' the employer and the TES workers would need to be treated no less favourably than the permanent workers of the client firm. In brief, the *Labour Relations Amendment Act No.6 of 2014* (LRAA) was the government's attempt to regulate the triangular relationship between the client firm, the TES firm and the worker, which had been subject to limited regulation prior to April 2015.

Despite the support from certain sectors of society for stronger EPL for these workers, there were also concerns that the new legislation, which essentially raised the client firm's costs of using agency workers, could lead to significant disemployment effects. The amendments were introduced in the context of a very slack labour market in the post-Apartheid period. For example, in 2015, just under a quarter of the labour force was unemployed according to the official (or searching) definition of unemployment, and the informal sector was relatively small at around 17% of total employment (Statistics South Africa 2015a). Given the limited availability of jobs in the formal sector (and even limited employment in the lower-paying, more insecure informal sector), TES firms might have at least been performing the important role of matching intermediary, offering jobseekers the opportunity to access a network of employers and gain some valuable experience.

Although the amendments came into effect almost 10 years ago, there has been no rigorous empirical assessment of the impact of the amendments as, until recently, there was no reliable source of data on the TES sector in South Africa. Indeed, the lack of quality data on TES workers is one of the main reasons for limited research on this group of workers more generally in the international literature (Addison et al. 2019; Drenik et al. 2023). Studies that have examined the amendments in South Africa have either relied on a small sample of TES firms that was not nationally representative (Bhorat et al. 2015) or on very unreliable estimates of the TES workforce from the labour force survey (Bhorat and Lilenstein 2017), which does not allow the separate identification of TES employees in the data (as we explain further in the next section).

This paper uses the administrative tax data released by the South African Revenue Services and National Treasury (hereafter referred to as the SARS-NT data) to examine the (short-term) impact of the LRAA on the labour market status, wages and job contract length of TES workers that were affected by the legislation. A key feature of the dataset, which spans 2011 to 2016, is that it accurately identifies TES or labour broker firms and their employees. The reason labour brokers are identifiable in the administrative data is because they are expected to submit an additional form to SARS (the IRP30A) to avoid double-taxing of their employees through the client firms. Another key feature of the data is that information on individuals can be linked across the tax years via their South African identification number. The panel nature of the data therefore allows us to track individuals that were in the TES sector prior to the legislative amendments and to examine what happened to them once the amendments were implemented. A Regression Discontinuity Design (RDD) is used for the analysis, as the legislation applied only to workers earning below a threshold of R205,433 a year in 2015. This is made possible by the fact that the employee earnings data are collected from employers themselves and thus accurate point values are available, whereas in most labour force surveys a large share of earnings information is collected in brackets (see Kerr 2021 for a detailed description of the SARS-NT earnings data).

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This study makes an important empirical contribution to the literature for two reasons. First, much of the empirical work on the impact of stricter EPL for temporary workers is set in the European context, and most of this work has focused on workers in fixed-term contracts or temporary work more generally as opposed to TES workers specifically. Second, the South African case is an interesting one, as stricter EPL was imposed in an environment where unemployment was very high and the prospects for economic growth bleak. It is therefore important to examine whether the legislation made workers better off or had unintended negative consequences for employment and job conditions. This is poignant in light of the fact that South Africa has more recently strengthened the position of workers even further by introducing a national minimum wage in January 2019.

Briefly, the findings suggest that the amendments had mixed results for the intended beneficiaries. According to the RDD analysis, there was no significant immediate impact on TES worker transitions following the implementation of the new legislation, implying that the predicted disemployment effects did not transpire (at least not in the short-term). There were improved working conditions in terms of contract length and earnings for those TES workers that transitioned into the non-TES sector; however, this was the case for a relatively small proportion of workers employed in the TES sector prior to the amendments. In further robustness checks, we find no evidence to suggest that employers pre-empted the legislation and acted prior to the LRAA being enforced.

The following section provides context to the legislative framework in South Africa and summarises the hypothesised effects of the LRA amendments. Section 3 discusses the data and sample and presents some descriptive statistics. Section 4 explains the econometric approach, and Section 5 presents the results. Section 6 concludes.

2 | Background to Legislative Amendments in South Africa and Hypothesised Effects

In 2012, the Labour Relations Amendment Bill added four new sections to Section 198 (sections 198A–D) to deal with three categories of nonstandard or atypical employees: TES employees, employees on fixed-term contracts, and part-time employees. The Labour Relations Amendment Act was only published in August 2014 and became effective on 1 January 2015. It specifically targeted employees who earned below R205,433.30 per annum which is the Basic Conditions of Employment Act (BCEA) threshold.[4] The provisions in the act governing TES only came into effect on 1 April 2015, after the expiration of a 3-month grace period.

There were two particular changes to how TES employees were to be treated following the amendments (Section 198A). First, a worker would only be seen as a temporary worker if they were employed to perform a genuine 'temporary service' for a client. If that were not the case, the employee would be *deemed* to be an employee of the client firm, and not the TES firm.

Section 198A (p. 34) of the LRAA stated that a 'temporary service' is defined as work for a client by an employee:

a. for a period not exceeding 3 months;

b. as a substitute for an employee of the client who is temporarily absent; or

c. in a category of work and for any period of time which is determined to be a temporary service by a collective agreement concluded in a bargaining council, a sectoral determination or a Ministerial notice.

The 3-month period referred to in point (a) started on the effective date of the Amendment Act, which was 1 January 2015. A TES employee working for a client for longer than 3 months would be deemed to be permanent unless the conditions (b) or (c) above applied. Prior to the change in legislation, client firms could hire employees on a temporary contract from a TES firm for an unlimited amount of time.

Second, the 'deeming' provision meant that the client firm would become legally liable for the well-being of the TES employee after 3 months; in other words, the client would be legally obliged to treat TES employees no differently to their permanent staff even if the TES workers were not on the client firm's payroll. This new legislation would have resulted in increased direct and administrative costs for the client firm. The client firm would have to comply with the BCEA, which stipulates conditions of work regarding hours, overtime pay, leave and severance pay, amongst others. In addition, the TES employee would now be eligible to institute proceedings against either the TES agency or the client firm, or both (whereas previously only the TES employer was legally liable for the worker). Later, in 2018, the case of *Assign Services (Pty) Limited v National Union of Metalworkers of South Africa and Others [2018] CCT 194/17* clarified the role of the TES firm and the client firm after 3 months. It was held that after 3 months, the role of the TES firm would be 'to pay and manage the human resources component of employment, while the day-to-day management, working conditions, work allocations and performance assessment' would be conducted by the client (Milo 2018).

The amendments also sought to strengthen the position of other atypical workers, namely, fixed-term contract employees and part-time workers. While fixed-term workers could be employed on contracts of longer than 3 months, or successive fixed-term contracts, the scope of the work had to be defined as a task that required a finite amount of time. In addition, the LRAA Section 198(B)(8a) stated: 'An employee employed in terms of a fixed term contract for longer than three months must not be treated less favourably than an employee employed on a permanent basis performing the same or similar work, unless there is a justifiable reason for different treatment'. Similarly, part-time workers would have to be treated no less favourably than permanent workers if they were employed for longer than 3 months on a part-time basis. These provisions would therefore have reduced (although not eliminated) the incentive amongst employers to replace TES workers with other types of atypical workers, such as fixed-term or part-time workers.

The legislative amendments essentially made the conditions around hiring and firing temporary employees, and particularly TES workers, less flexible. While the objective of the

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amendments was to eliminate the exploitation of vulnerable temporary employees that were kept on lengthy or rolling temporary contracts, the limited flexibility might also have disincentivised firms from using TES agencies.

Only two studies have attempted to examine the impact of the 2015 amendments to the LRA in South Africa, both of them focussing on TES workers. Bhorat et al. (2015) used survey data collected by CAPES or the Confederation of Associations in the Private Employment Sector (an industry body for TES firms) on a cross-section of industries in four provinces for two periods, March/April 2014 (before the legislation was implemented) and March/April of 2015 (just as the legislation became binding). The authors found that the primary effect of the amendments was one of disemployment. Half of the TES workers were terminated by the labour broker firms and around a quarter were made permanent or taken on contract directly by the client firm. The rest were retained by the TES firms. While this study provides useful insights into the impact of the LRAA, the sample used was not nationally representative and the analysis purely descriptive, presumably relying on managers' responses regarding TES employment in 2014 and 2015.

Bhorat and Lilenstein (2017) used the official Quarterly Labour Force Survey to examine the impact of the amendments descriptively. However, because there is no way to separately identify TES workers in the survey, the authors analysed the category of workers captured as being in 'Businesses Services not elsewhere classified' (SIC code 889). This code includes TES workers as well as workers in a number of other sectors.[5] The authors find that there was a sudden decline in jobs in the first quarter of 2015 in this category, but there was a reversion back to pre-amendment employment levels by the first quarter of 2017. While this is a useful exploration, caution should be exercised, as the broader industrial classification overestimates the number of workers in the TES sector (Budlender 2013) and therefore may misrepresent the impact of the amendments. Further, as the authors themselves recognised, as the study was descriptive and did not use a control group, there was 'no attempt at isolating the pure effect of the labour regulatory amendment on the employment of TES workers, nor their conditions of employment' (Bhorat and Lilenstein 2017, 12).

In this paper, we use the more reliable SARS-NT tax data to try and explore the causal impacts of the amendments on TES worker outcomes. The tax dataset provides reliable information on whether employees are hired by TES or non-TES firms as well as accurate point estimates of earnings, allowing us to identify whether the worker fell below the threshold and was therefore eligible to benefit from the amendments. However, there is limited information on the workers themselves or the aspects of the job. We are therefore restricted in our investigation by what is available in the dataset. Specifically, for the group of workers employed in the TES sector prior to the amendments, we explore three outcomes: their employment status following the policy change (i.e., whether employed in the TES sector, the non-TES sector, or no longer employed by a tax-paying firm), and whether their earnings and job contract length increased or not, disaggregated by whether they were retained in the TES sector or moved to employment in the non-TES sector.[6]

Given the scope of the amendments, and the findings in the broader literature, the hypothesised effects on employment status, earnings and job contract length for those employed in the TES sector prior to the policy change are as follows:

- Employment: We would expect the amendments to lead to fewer workers being employed in the TES sector because of the reduced flexibility and higher costs associated with the use of temporary workers through TES agencies. However, whether these workers are more likely to become unemployed or more likely to be employed in the non-TES sector (potentially by client firms) is an empirical question. When firing costs for permanent workers are high and there are also stricter rules governing fixed-term temporary contracts (as were introduced in South Africa in 2015), firms might be reluctant to employ these workers directly, resulting in unemployment of workers previously employed through TES agencies.
- Earnings: If workers remain employed, one might expect to see the earnings of TES employees increase for two reasons. One, TES employers could have raised earnings above the threshold of R205,433 to avoid the legislation altogether. Two, if employers complied with the LRAA, then client firms may have taken on TES workers permanently and instead of paying TES agencies a mark-up fee, they may have paid these workers more, in line with what they paid their permanent workers. This would result in higher wages post-amendments for those who remained in the TES sector and those that moved into the client/non-TES sector.
- Job contract length: We would expect some workers retained in the TES sector to see their contract length reduced to align with condition (a) of the new legislation that requires temporary work to be limited to 3 months or less. The effect might be quite minimal if firms were able to apply conditions (b) or (c), as then TES workers would remain on contracts of longer than 3 months with the TES firms. For those moving into the non-TES sector, we might expect their contract duration to increase because only workers doing actual temporary work could be employed on short-term contracts. Alternatively, there may be no impact on contract length as employers could have used a sequence of different workers with short contracts instead of rolling short-term contracts for the same workers (Bratti et al. 2018).

Given these potentially opposing effects, the overall outcome of the amendments for TES workers is ambiguous and becomes an empirical question for testing.

3 | Data and Descriptive Statistics

The SARS-NT data which we use for the analysis are for the tax years 2015 (1 March 2014 to end February 2015) and 2016 (1 March 2015 to end February 2016), that is, the year before and the year after the LRAA was implemented respectively (see Figure 1). In the Employment Panel (the dataset in the SARS-NT collection used in this study), the unit of analysis is the *job contract*, as it includes records of employment for tax-paying firms over the period based on information in the IRP5 certificate. However, the data can also be collapsed to the individual level as it includes anonymised identity

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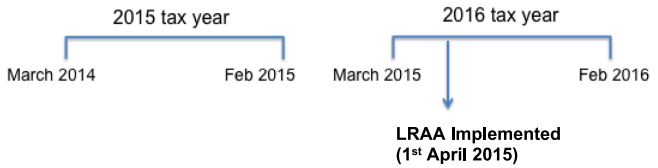

FIGURE 1 | Timeline of tax data and LRAA. [Color figure can be viewed at wileyonlinelibrary.com]

numbers.[7] While it is possible to see whether an individual is employed in a job contract by a TES firm or a non-TES firm, it is not possible to match the TES employee to the client firm that the TES employee physically works at. This limits the analysis, as it would be useful to identify more precisely whether TES workers moved to the client firm post-amendments, whereas it is only possible to identify whether workers moved into the broader category of the non-TES sector (i.e., all non-TES firms).[8]

The sample used in this analysis is restricted to individuals who were working in the TES sector in the 2015 tax year just before the amendments were enforced. These workers are then tracked to analyse what happened to them in the following tax year (2016), just after the amendments were implemented. As noted above, although the legislation was enacted in January 2015, it was only enforced in April 2015 that forms part of the 2016 tax year (see Figure 1). This means that although the timing for the analysis is not exact (the 2016 tax year starts in March) it is a very close match.

In any one tax year, individuals can be employed in more than one job contract. For example, in the 2015 tax year, there were around 399,466 individuals employed in the TES sector that had 459,762 contracts. Since we are interested in changes to employment outcomes just after the amendment was implemented, the sample was restricted to those individuals whose last contract of the 2015 tax year (in other words, the last contract before the reform) was in the TES sector. Of the 399,466 individuals that had TES contracts in 2015, 340,673 of these individuals held a TES contract as their last contract for the year (the rest held a TES contract in the first part of the year and then moved to the non-TES sector). By focusing on the sample whose last contract of 2015 was in the TES sector, certain changes that occurred prior to implementation on 1 April 2015 may be overlooked if employers pre-empted the legislation and acted early. However, for practical purposes, a cut-off closest to the implementation of the legislation was necessary for the analysis. In Section 5.4, additional analysis is conducted using the 2014 and 2015 tax years to gauge whether employers might have pre-empted the legislation.

The employment outcomes in the 2016 tax year (so after the reform) are therefore analysed for the sample of individuals (340,673) whose last contract was in the TES sector in the 2015 tax year. For those workers who were employed in more than one job contract in the 2016 tax year, the first job contract of the tax year is analysed, to identify what happened to these workers just after the amendments became enforceable. Three main outcomes are explored: employment status, earnings and job contract length. First, we analyse whether workers in TES employment in the 2015 tax year remain employed in the TES sector, transition into the non-TES sector, or are no longer in the dataset. If they are no longer in the tax dataset, this implies they either became unemployed, were not economically active, or were working but for unregistered/informal firms. We are unable to differentiate between these outcomes as no follow-up information is available on those who are no longer employed by tax-registered entities in that tax year.

Next, for those who remain in the dataset and therefore were employed in the formal sector in the 2016 tax year in either TES or non-TES employment, we track changes in the nature of their employment, specifically their earnings and job contract length. Earnings data include income, taxable allowances and benefits and deductions for medical aid and retirement schemes. Job contract length captured in the tax data is measured in days, with the maximum being the 365 days of that tax year. In other words, for those whose employment contract extends beyond a year, their information is truncated at 1 year. This means that for those who remained employed in the 2016 tax year, we are only able to measure whether their employment contract increased or not up to a maximum of 1 year and not whether they are employed in permanent jobs.[9]

The transition matrix of employment status in Table 1 provides a descriptive picture of whether TES workers employed in the 2015 tax year remained in the TES sector, transitioned to the non-TES sector or moved out of the data altogether in the 2016 tax year (either because they became unemployed, were economically inactive or were working for firms not registered for tax). The table shows that while the bulk of those that were in the TES sector in the 2015 tax year were employed in the TES sector again in the 2016 tax year (54%), of those that were not retained, a larger proportion of individuals moved out of the data (28%) than transitioned to the non-TES sector (18%). This pattern holds for those above and below the earnings threshold.

If one compares those above and below the threshold, retention in the TES sector is 25 percentage points higher for those above the threshold compared with those below the threshold (78% vs. 53%). Those above the threshold are much less likely than those below the threshold to transition to the non-TES sector (8% vs. 19%) or to transition out of the data (14% vs. 29%). These descriptive results suggest that those above the threshold are in a far less precarious employment position as a larger percentage retain their jobs; however, they retain them in the TES sector. Below the threshold, a larger percentage is moving into what may be better contractual positions in the non-TES sector, which would suggest that the legislation may have had, to some degree, its intended effect. However, a larger percentage is also moving out of the data altogether which could signal a disemployment effect. Of course, those above and below the threshold may be systematically different, hence the need for the more rigorous analysis using a Regression Discontinuity Design that we conduct in the next section.

Table 2 examines the job characteristics, namely, earnings and job contract length (within the tax year), of the sample before and after the LRAA, disaggregated by whether they were retained in the TES sector or employed in the non-TES sector in the 2016 tax year. Before looking at the changes in the post-reform period, it is useful to look at mean earnings and job contract duration in the pre-reform period to provide a contextual picture of the TES sector. The first thing to note is that

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TABLE 1 | TES employee transitions between the 2015 and 2016 tax years.

		2016			
		TES	**Non-TES**	**Out of data**	**Total**
2015 TES	**Below threshold**	173,265	61,291	93,256	327,812
		52.85%	18.70%	28.45%	100%
	Above threshold	10,024	1,081	1,756	12,861
		77.94%	8.41%	13.65%	100%
	Total	183,289	62,372	95,012	340,673
		53.80%	18.31%	27.89	100%

Source: Authors' estimates based on the SARS-NT employee tax data.
Note: The sample is at the individual level and includes all those whose last job contract of the 2015 tax year was in the TES sector. The threshold is R205,433.

the vast majority of the sample (roughly 95%) falls below the BCEA threshold of R205,433.30 per annum. Mean earnings amongst those below the threshold is only R43,532.61, compared with mean earnings for the much smaller group above the threshold of R523,387.40. This is consistent with research in Cassim and Casale (2022), which used the tax data to show that TES worker earnings were significantly below non-TES worker earnings, with the ratio of TES to non-TES wages being just above 0.5 at the mean in 2015.

Also interesting is that the mean contract duration within the tax year of these TES workers is well above the 3-month mark. Workers below the earnings threshold had a mean contract length of 262 days and workers above the threshold of 334 days. This aligns with the findings in Cassim and Casale (2022), which showed that only 15% of TES workers were in job contracts of 3 months or less with nearly 30% in job contracts of a year or more. This compares to just under 10% of workers in the non-TES sector in job contracts of 3 months or less and 54% in job contracts of a year or more. The large numbers of TES workers on lengthy contracts is consistent with concerns that TES workers were being used for jobs that were not necessarily temporary in nature.

If we focus now on the changes between the 2015 and 2016 tax years for those employed in the TES sector prior to the amendments, we see a mixed picture. Looking at the earnings of those below the threshold (the upper panel of the table), while the percentage of workers for whom real earnings increased between the 2015 and 2016 tax years was similar for those that were retained in the TES sector and those that moved into the non-TES sector (about 57% of both groups experienced real earnings increases), the actual increase in real earnings was much higher for those that moved into non-TES compared with those that stayed in the TES sector (111% vs. 20%). Regardless of whether they were in TES or non-TES jobs in the 2016 tax year, those below the threshold were more likely to see an increase in real earnings, as well as a higher average increase in real earnings, than those above the threshold. In fact, for those above the threshold (lower frame of the table), there was a real average earnings decline, with those who moved into the non-TES sector experiencing a more substantial decline on average than those that stayed in the TES sector (−44% vs. −11%).

Focusing on job contract length for those below the threshold (upper panel of Table 2), the length of the job contract increased for more than half of individuals retained in the TES sector (61%) but less than half of those who moved to the non-TES sector (47%). Similarly, for those above the threshold (lower panel of the table), more than half of those retained in the TES sector experienced an increase in job duration (63%) compared with less than half of those that moved to non-TES (43%). Mean job duration, however, declined for both groups, although more so for those that moved into non-TES (−23%) compared with TES employment (−12%). Overall, the table suggests that those below the threshold that moved to non-TES employment benefitted the most in terms of earnings, while those below the threshold that were retained in TES jobs benefitted the most in terms of job contract length. However, these changes need to be analysed using a more rigorous regression discontinuity design, which we describe in the following section.

4 | Econometric Strategy

A sharp regression discontinuity design is used to estimate the impact of the legislative amendments on TES workers' labour market status, wages and job contract length. A defining feature of the RDD is that the probability of being treated changes discontinuously at the cut-off (Cattaneo et al. 2019). Individuals earning above the earnings threshold who were not subject to the LRAA amendments provide a counterfactual to those below the threshold who were subject to the LRAA amendments. Specifically, there was a discontinuous change in the degree of regulation at the R205,433 earnings threshold.[10] An advantage of the RDD is that it relies on relatively weak assumptions compared with other nonexperimental approaches and consequently provides more credible results. In addition, the assumptions are testable in a similar manner to randomised experiments (Hijzen et al. 2017). A number of papers use a sharp RDD identification strategy similar to the one pursued in this paper to investigate the effects of EPL (e.g., Bratti et al. 2018; Hijzen et al. 2017). However, a key difference between their work and this study is that, in the South African case, the legislation applied to those earning below a certain level of income, whereas in most other countries studied, a threshold was set by the size of the firm.[11]

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TABLE 2 | Differences in job characteristics before and after LRAA implementation.

	Pre-reform (2015)		Post-reform TES (2016)				Post-reform non-TES (2016)			
	N	Mean	*N*	Mean	% change 2015–2016	% of sample with an increase	*N*	Mean	% change 2015–2016	% of sample with an increase
Below threshold										
Real earnings (Rands)	323,521	R43,532.61	170,137	R52,272.09	20.08%	56.99%	60,133	R110,273.5	110.96%	58.71%
Job contract length (days)	322,097	261.55	169,115	294.21	12.49%	61.19%	59,446	256.54	−1.92%	46.81%
Above threshold										
Real earnings (Rands)	17,152	R523,387.40	13,152	R463,933.80	−11.36%	42.37%	2239	R292,247.40	−44.16%	30.68%
Job contract length (days)	17,148	333.51	13,120	295.09	−11.52%	62.97%	2203	255.71	−23.33%	43.35%

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: The differences between 2015–2016 are statistically significant at 1% level. Earnings are deflated using 2016 as the base year. Sample is at the individual level. The threshold is R205,433. Job contract length is based on number of days employed in that contract in that year and is truncated at 1 year (as per the tax records).

Studies examining the impact of employment protection in other countries have sometimes used a Difference-in-Differences (DiD) strategy to isolate the impact of EPL amendments on employment (e.g., Autor 2003; Damiani et al. 2016; Pierre and Scarpetta 2013). However, a DiD identification approach is not suitable in the South African case because, as explained in Section 2, the LRAA applied to TES workers as well as fixed-term and part-time workers in the non-TES sector. As such, non-TES firms or their employees could not be used as the control group as those that were on part-time or fixed-term contracts were also affected by the legislation, violating the parallel trends assumption.

For the RDD analysis, we use a nonparametric local polynomial estimation method from Calonico et al. (2014) that is commonly applied in RDD analysis (Bratti et al. 2018; Kantorowicz and Hlobil 2020). This involves choosing the bandwidth around the earnings threshold using a data-driven process based on nonparametric approximation to minimise the mean squared error (MSE) of the RDD estimator.[12] In this case, the researcher needs to specify a given polynomial order and a specific kernel (Bratti et al. 2018). The bandwidth is chosen in a way that balances the lower variance associated with narrower bandwidths with the bias associated with including observations far from the threshold. The bandwidths should be wide enough to include a sufficient number of observations and produce precise estimates but should also be narrow enough to compare similar units and avoid selection bias. This is preferred to the parametric method that is based on an ad hoc chosen bandwidth and assumes away misspecification bias (Bratti et al. 2018).

After selecting the bandwidths, a weighted least squares regression is estimated to examine the outcomes of interest for those who were employed in the TES sector prior to the amendments, namely, the probability of being in a certain labour market status in the 2016 tax year; the probability of wages increasing between 2015 and 2016 and the probability of job contract length increasing between 2015 and 2016.

The specification is as follows:

$$Y_{it+1} = \alpha + \tau(D_i) + \beta_1(E_{it} - T) + \beta_2(E_{it} - T)^2 + \beta_3 D_i(E_{it} - T) + X_{it} + \varepsilon_{it} \quad (1)$$

$$D_i = 1\left[(E_{it} > T\right]$$

$$T - h \leq Ei \leq T + h$$

Y_{it+1} is a binary variable representing the outcome in 2016. E_{it} is earnings in 2015. T is the threshold of R205,433. The term $(E_{it} - T)$ refers to the normalised forcing variable and a polynomial of the forcing variable is included. D_i is a dummy that equals 1 if individual earnings are above the threshold and zero otherwise. While this may seem counter-intuitive for this analysis since the treated group are those below the threshold, RD models (in the standard statistical package) estimate the average treatment effect (τ) at the threshold (T) as the difference between the estimated parameters from the regression for those

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above (the 'nontreated' in this case) relative to those below the threshold (the 'treated' in this case).

$$\tau = E\left[Y_i(1) - Y_i(0) | E\text{i} = T\right]$$

The RD estimates presented in Section 5 below therefore show the impact of the amendments on those TES workers above relative to those below the earnings threshold within the estimated bandwidths h, which provide the window around the earnings threshold. Controls X_{it} include age, gender, job contract length and firm size in the 2015 tax year, that is, the baseline year.

One of the challenges with this type of analysis is to accurately distinguish between the independent effect of the amendments on earnings and the possibility that individuals self-select into earnings categories or employers manipulate earnings by moving employees above the threshold to circumvent the conditions of the LRAA. Our identifying assumption in this analysis is that in the absence of the reform, there would be no substantial change to real earnings between those above and below the threshold. Because the reforms prescribe that TES workers employed for more than 3 months should be treated similarly to permanent workers, an increase in wages may imply compliance with the legislation. However, if a number of workers are observed to move just over the threshold, then it may invalidate the RDD, as it could mean that employers are manipulating earnings, and the resulting outcomes are not a true reflection of what would happen if employers were complying with the amendments.

Analysis of the data shows that of those that were below the threshold in the 2015 tax year, around 68% remained below the threshold, just under 4% moved above the threshold and around 29% moved out of the data. There was a larger proportion of individuals who moved from above the threshold in the 2015 tax year to below the threshold in the 2016 tax year, that is, 25% (with 65% remaining above the threshold and the remaining 10% moving out of the data). Given that so few individuals move above the threshold, it is unlikely that there was substantial manipulation of earnings that would bias the results.

To examine this further, a McCrary (2008) test is run which is a formal test of the null hypothesis of continuity of the density of the forcing variable at the threshold against the alternative hypothesis that there is a jump at the threshold. The McCrary test is visualised in Figure 2 with separate weighted kernel estimations and 95% confidence intervals of the log of density of earnings on either side of the threshold. The test gives a log difference between the frequencies to the right and left of the threshold of 0.014 (SE 0.0270; *t*-stat 0.5) which is statistically insignificant. The null cannot be rejected which suggests that there is no sorting around the threshold in our case. Overall, there is no evidence of manipulation of earnings, which means that the composition of employment on either side of the threshold was not subject to any 'sorting'.

Finally, a balancing covariates test is conducted to understand whether the baseline covariates in the regression are balanced on either side of the threshold. If the covariates known to strongly correlate with the outcome of interest are discontinuous at the threshold, the continuity of the outcome functions may not hold (Cattaneo et al. 2019). The condition should be met that the assignment variable and any covariates should be random around the threshold and should not affect the estimates apart from the standard errors (Hijzen et al. 2017). The method outlined by Cattaneo et al. (2019) is used, where the dependent variable in Equation (1) is replaced with each of the covariates, and as above, a polynomial of the first order is chosen as well as a triangular kernel, and the bandwidths are selected using a nonparametric approach. The results are presented in Table A1. All RD estimates besides the coefficient on *Female* are insignificant when a polynomial of the first order is used.[13] The negative coefficient on the *Female* dummy variable means that the probability of being female is lower for those above relative to those below the threshold. As a robustness check, the weighted least squares regression (Equation 1) is run with and without covariates in the section that follows. The results are generally consistent whether covariates are included or not.

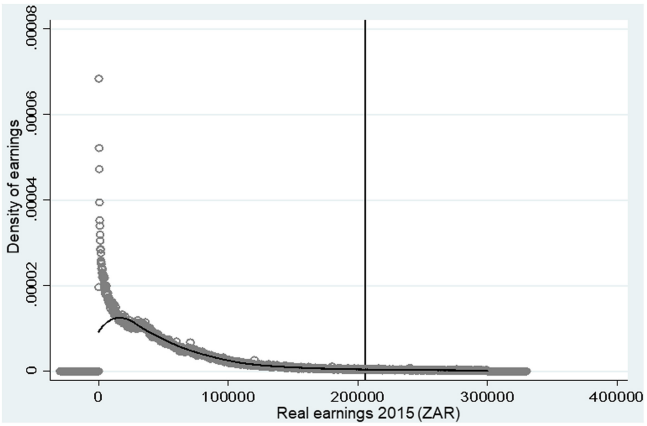

FIGURE 2 | McCrary test. *Source:* Authors' estimates based on IRP5 data. *Note:* The R205,433 threshold is marked by the black line. The bin size is 138.99. [Color figure can be viewed at wileyonlinelibrary.com]

5 | Results

For each outcome variable, the tables report the RD coefficient and standard error, the number of observations, the effective number of observations above and below the threshold (within the bandwidth), the order of the polynomial (one or two), and the bandwidths. Equation 1 is estimated with and without covariates and using a polynomial of orders 1 and 2, in all cases using the optimally specified bandwidths (Columns 1–4 of each table). Our preferred specification is in Column 3 which includes covariates and uses a polynomial of order 1.

In order to test the sensitivity of the results to the response of units very close to the threshold, 'donut-hole' tests are run (Cattaneo et al. 2019) and shown in Columns 5 and 6 of each table. The idea behind the donut-hole regressions is to exclude observations close to the threshold that are most likely to be manipulated and then repeat the estimation. Those that earned 1% and 3% above and below the threshold (which

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amounts to around R2000 and R6000, respectively) are excluded. The tests are run using the estimated bandwidth from our preferred specification from Column 3 for the sake of comparison.

5.1 | The Impact of the Amendments on Labour Market Status

Three binary variables are used to measure the impact of the amendments on the labour market status in the 2016 tax year of the sample of workers whose last contract of the 2015 tax year was in the TES sector: (i) the probability of being retained in TES relative to moving to non-TES; (ii) the probability of being retained in TES relative to moving out of the data and (iii) the probability of moving out of the data relative to being in non-TES.[14] As described in Section 2, if conditions around hiring a TES worker become more stringent, employment of TES workers is likely to decline as they are either absorbed by the non-TES sector or become unemployed.

The positive RD estimates in the first frame of Table 3 suggest there is a higher probability of being retained in TES relative to moving into non-TES for those above the threshold (the nontreated) relative to those below the threshold (the treated); however, none of the estimates is statistically significant. Although the samples are different given the bandwidth restrictions, this result is consistent with what was observed in the transition matrix in the descriptive table in Section 3. Adding covariates to the model and changing the order of polynomial affects the size of the estimate but not the significance. Furthermore, the RD estimates from the donut-hole tests in Columns 5 and 6 are largely consistent with the comparable baseline result in Column 3 (using the same bandwidth). The RD estimates are still positive and insignificant, but now the estimates are larger in size. This means that observations further from the threshold are driving the result rather than observations closer to the threshold.

In the second frame of the table, the positive RD estimates suggest that the probability of being retained in TES relative to moving out of the data is higher for those above relative to below the threshold (again consistent with the transition matrix in Table 1). However, the RD coefficients are very small and not significant. The estimates from the donut-hole tests in Columns 5 and 6 are similarly small (in fact close to zero) and insignificant.

In the third frame of the table, the RD estimates yield a negative coefficient which suggests that, for those who were employed by TES firms in the 2015 tax year, the probability of being out of the data relative to being employed in the non-TES sector in 2016 is lower for those above relative to those below the threshold. Put differently, those below the threshold, or the treated group, are more likely to move out of the data than into the non-TES sector, relative to the nontreated group. These results yield higher RD estimates than the previous outcomes, are consistent across specifications, but are still not statistically significant. The donut hole tests in Columns 5 and 6 produce estimates that are close to zero and insignificant, suggesting that any small negative effect on the outcome that was observed in the baseline estimates is driven by those closer to the threshold.

While the policy amendment did not explicitly specify that individuals should be moved to the client firm in the non-TES sector, it made it less attractive for client firms to hire workers through an intermediary TES firm. So one might expect that the client firm would employ the TES workers on their own books. The results suggest that TES workers employed in the 2015 tax year who were below the threshold were marginally more likely to move into the non-TES sector relative to staying in the TES sector than those above the threshold. However, they were also less likely to be retained by the TES sector relative to moving out of the data, and more likely to move out of the data relative to moving to the non-TES sector, than those above the threshold. This disemployment effect is contrary to what the legislation intended to achieve. Nonetheless, the results are not statistically significant and the coefficients are very small. Overall, this suggests that the LRAA had a very limited effect on the employment status of TES workers (in the short-term at least).

5.2 | The Impact of the Amendments on Wages

In this section, the impact of the LRAA on wages is assessed for those who remained employed over the period. The outcome variable is a binary variable that equals 1 if wages increased between the 2015 and 2016 tax years and 0 if wages stayed the same or declined. The results are presented separately for those that were retained by the TES sector in the 2016 tax year (upper frame of Table 4) and those that moved to the non-TES sector in the 2016 tax year (lower frame of Table 4).

The negative RD estimates indicate that those below the threshold (or the treated group) were more likely to see an increase in wages relative to those above. For those that were retained in the TES sector, the effect is very small, insignificant, and when the donut hole tests are conducted, tends to zero. For those that moved to the non-TES sector, however, the estimates are significant and quite sizeable, indicating that the probability of wages increasing is between 11 and 14 percentage points lower for those above relative to those below the threshold. In other words, for those that moved into the non-TES sector, the treated group did better than the nontreated group. Above-inflation wage increases for this group could suggest that there was some level of compliance with the legislation, if those that did move to the non-TES sector were moved onto the client firm's payroll and treated no less favourably than their permanent workers. However, TES workers may also have moved into the non-TES sector but not necessarily to the client firm; therefore, this result could also be picking up that the non-TES sector pays better wages than the TES sector on average.[15]

5.3 | The Impact of the Amendments on Job Contract Length

The next set of estimations considers the short-term impact of the LRAA on job contract length. The same set of specifications

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TABLE 3 | Change in labour market status between the 2015 and 2016 tax years.

	1	2	3	4	5 (DH: 1%)	6 (DH: 3%)
Probability of retention in TES relative to moving into non-TES in 2016						
RD estimate	0.010 (0.017)	0.016 (0.022)	0.015 (0.014)	0.009 (0.029)	0.024 (0.018)	0.017 (0.019)
N	245,661	245,661	244,545	244,545	243,596	243,379
Effective N (below T)	5,854	6,943	9,133	6,946	8,637	8,509
Effective N (above T)	3,403	3,672	4,073	3,617	3,620	3,531
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	45,790	51,334	61,436	51,753	61,436	61,436
Covariates	No	No	Yes	Yes	Yes	Yes
Probability of retention in TES relative to moving out of data in 2016						
RD estimate	0.004 (0.016)	0.002 (0.019)	0.011 (0.015)	0.010 (0.019)	−0.000 (0.020)	0.002 (0.021)
N	292,297	292,297	290,699	290,699	289,724	289,490
Effective N (below T)	5,467	9,101	6,363	9,278	5,845	5,709
Effective N (above T)	3,323	4,182	3,513	4,150	3,056	2,958
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	43,190	60,328	48,584	61,645	48,584	48,584
Covariates	No	No	Yes	Yes	Yes	Yes
Probability of moving out of data relative to moving into non-TES in 2016						
RD estimate	−0.014 (0.033)	−0.046 (0.053)	−0.019 (0.033)	−0.042 (0.057)	−0.003 (0.036)	0.004 (0.038)
N	143,388	143,388	142,546	142,546	142,275	142,275
Effective N (below T)	5,187	4,143	5,541	3388	5,397	3,864
Effective N (above T)	1,356	1,258	1,340	1,133	1,213	1,087
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	90,597	80,637	93,626	72,613	93,626	93,626
Covariates	No	No	Yes	Yes	Yes	Yes

Source: Authors' estimates based on employment panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates while the rest are run with covariates including age, gender, job duration and firm size in 2015. In Models 1–4, bandwidths are selected using the mean square error (MSE) estimator for the RD treatment effect. Models 5 and 6 are donut-hole (DH) estimations and apply the same bandwidth as Specification 3, which is the preferred specification. Models 5 and 6 exclude those that earned 1% and 3% above and below the threshold (which amounts to around R2000 and R6000, respectively). All specifications use a triangular kernel.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

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TABLE 4 | Probability of wages increasing between the 2015 and 2016 tax years.

	1	2	3	4	5 (DH: 1%)	6 (DH: 3%)
TES to TES						
RD estimate	−0.010 (0.024)	−0.018 (0.031)	−0.004 (0.020)	−0.007 (0.029)	0.006 (0.022)	0.003 (0.025)
N	183,289	183,289	182,415	182,415	182,055	181,414
Effective N (below T)	5,591	8,924	10,451	10,874	10,261	9,917
Effective N (above T)	3,071	3743	3,935	3,991	3,765	3,468
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	51,275	68,523	75,156	76,690	75,156	75,156
Covariates	No	No	Yes	Yes	Yes	Yes
TES to non-TES						
RD estimate	−0.111** (0.052)	−0.130** (0.057)	−0.117** (0.053)	−0.140** (0.058)	−0.118** (0.060)	−0.091 (0.075)
N	62,372	62,372	62,130	62,130	62,082	61,965
Effective N (below T)	1,252	3,230	1,123	3,018	1,098	1,033
Effective N (above T)	579	871	550	838	527	475
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	48,578	87,028	45,584	84,204	45,584	45,584
Covariates	No	No	Yes	Yes	Yes	Yes

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates while the rest are run with covariates including age, gender, job duration and firm size in 2015. In Models 1–4, bandwidths are selected using the mean square error (MSE) estimator for the RD treatment effect. Models 5 and 6 are donut-hole (DH) estimations and apply the same bandwidth as Specification 3, which is the preferred specification. Models 5 and 6 exclude those that earned 1% and 3% above and below the threshold (which amounts to around R2000 and R6000, respectively). All specifications use a triangular kernel.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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as above are estimated, but the dependent variable is a binary variable which equals 1 if the individual experienced an increase in contract duration between the 2015 and 2016 tax years and 0 if the contract duration declined or stayed the same after the amendments.[16] As above, the results are differentiated by those that remained in the TES sector and those that moved to the non-TES sector in the 2016 tax year.

For those that were retained in the TES sector, the positive RD estimates shown in Table 5 (upper frame) indicate that the probability that job contract length increased between the 2015 and 2016 tax years was higher for those above the threshold relative to those below the threshold. These estimates are significant when the covariates are included (Columns 3 and 4) and suggest that job contract length was 3.4 to 8.9 percentage points more likely to increase for those above relative to those below the threshold. The lower likelihood of an increase in job contract length for the treated group below the threshold might signal that some TES employers were trying to comply with the legislation by limiting job contracts to the acceptable length for TES workers who were actually being employed for temporary work by the client firm.

For those that moved into the non-TES sector (lower frame), the negative and significant coefficients on the RD estimates across specifications mean that the probability that job contract length increased was 13 to 16 percentage points lower for those above relative to those below the threshold. In other words, the treated group are more likely to see their job contract length increase than the nontreated group if they moved into the non-TES sector. This is in line with expectations, as the legislation attempted to ensure that only workers doing actual temporary work could be employed on short-term contracts.

TABLE 5 | Probability of job contract length increasing between the 2015 and 2016 tax year.

	1	2	3	4	5 (DH: 1%)	6 (DH: 3%)
TES to TES						
RD estimate	0.012 (0.029)	0.001 (0.033)	0.034* (0.020)	0.089** (0.041)	0.023 (0.015)	0.054** (0.025)
N	176,499	176,499	175,669	175,669	175,337	174,750
Effective N (below T)	3,444	6,847	10,411	4,605	10,021	9,925
Effective N (above T)	2,349	3,248	3,792	1,904	3,549	3,359
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	38,575	61,531	78,497	68,856	78,497	78,497
Covariates	No	No	Yes	Yes	Yes	Yes
TES to non-TES						
RD estimate	−0.130*** (0.045)	−0.147** (0.058)	−0.132** (0.052)	−0.155*** (0.059)	−0.165*** (0.056)	−0.152** (0.064)
N	61,649	61,649	61,463	61,463	61,415	61,302
Effective N (below T)	2,326	3,997	1,454	3754	1,429	1,367
Effective N (above T)	771	945	617	904	594	543
Order of polynomial (p)	1	2	1	2	1	1
Bandwidth in ZAR (h)	73,285	97,048	55,409	94,669	55,409	55,409
Covariates	No	No	Yes	Yes	Yes	Yes

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates while the rest are run with covariates including age, gender, job duration and firm size in 2015. In Models 1–4, bandwidths are selected using the mean square error (MSE) estimator for the RD treatment effect. Models 5 and 6 are donut-hole (DH) estimations and apply the same bandwidth as Specification 3 which is the preferred specification. Models 5 and 6 exclude those that earned 1% and 3% above and below the threshold (which amounts to around R2000 and R6000, respectively). All specifications use a triangular kernel.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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TABLE 6 | Change in labour market status between the 2014 and 2015 tax years.

	1: Probability of retention in TES relative to moving into non-TES in 2015	2: Probability of retention in TES relative to moving out of data in 2015	3: Probability of moving out of data relative to moving into non-TES in 2015
RD estimate	−0.008 (0.015)	−0.006 (0.017)	−0.045 (0.038)
N	215,724	273,163	129,613
Effective N (below T)	6,387	4,687	2,708
Effective N (above T)	3,394	2,860	1,131
Order of polynomial (p)	1	1	1
Bandwidth in ZAR (h)	55,779	43,749	77,759
Covariates	Yes	Yes	Yes

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications are run with covariates (age, gender, job duration and firm size in 2014), a polynomial of order 1 and using a triangular kernel. A sample of 310,953 individuals whose last contract in 2014 was in the TES sector is used.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

TABLE 7 | The probability of wages and job contract length increasing between the 2014 and 2015 tax years.

	TES to TES		TES to non-TES	
	1: Wages	2: Job contract length	3: Wages	4: Job contract length
RD estimate	−0.045* (0.024)	−0.015 (0.025)	−0.072 (0.059)	−0.019 (0.055)
N	174,440	130,352	41,284	38,938
Effective N (below T)	6,161	5,072	830	1,241
Effective N (above T)	3,074	2,250	467	571
Order of polynomial (p)	1	1	1	1
Bandwidth in ZAR (h)	59,082	68,426	58,115	77,723
Covariates	Yes	Yes	Yes	Yes

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications are run with covariates (age, gender, job duration and firm size in 2014), a polynomial of order 1 and using a triangular kernel. A sample of 310,953 individuals whose last contract in 2014 was in the TES sector is used.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Overall, the results suggest that while the LRAA seemed to have very little short-term effect on the employment status of TES workers, of the 2015 TES workers who remained employed in the 2016 tax year (just over 70% of the full sample), only the treated group (those below the threshold) that moved into the non-TES sector were better off. They had a higher probability of both earnings and job contract length increasing relative to those untreated or above the threshold.

5.4 | Further Examination: Did Employers Pre-Empt the Legislation?

Informal interviews conducted with three labour broker firms in Gauteng in June 2014 (as part of the exploratory research in Bhorat et al. 2016) suggested that the TES sector may have experienced the impact of the legislation in 2014 already, with a number of client firms no longer using their services. In order

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to examine whether employers pre-empted the amendments and acted prior to the implementation of the LRAA on 1 April 2015, an additional exercise was undertaken in which the RDD analysis was run 1 year prior, for the 2014 (1 March 2013 to end February 2014) and 2015 (1 March 2014 to end February 2015) tax years. A sample of 310,953 individuals whose last contract in the 2014 tax year was in the TES sector was analysed, and the dependent variables are analogous to those used above. All the estimations, presented in Tables 6 and 7, were run using the preferred specification, which includes covariates and a polynomial of order 1.

In Table 6, the dependent variables are binary variables used to measure the change in labour market status in the 2015 tax year for the sample of workers whose last contract of the 2014 tax year was in the TES sector. Similar to the labour market transitions for 2015/16, these results for 2014/2015 in Table 6 are insignificant, suggesting that employers did not behave differently prior to the implementation of the LRAA. For those TES workers who stayed employed, no significant effect on earnings and job duration is recorded in Table 7. The one exception is for those who remained in the TES sector, where those below the threshold were somewhat more likely to see a wage increase than those above the threshold (this effect was not significant for the 2015/2016 analysis above). Overall, based on these findings, there is no strong evidence to suggest that employers acted differently in the year prior to the amendments, and pre-empted the legislation.

6 | Conclusion

The legislative amendments to the LRA in 2015 were the first attempt to regulate and protect atypical work arrangements in South Africa. Specifically, for TES workers, the intention of the amendments was to create less precarious employment by making the conditions around employing temporary workers through labour brokering less flexible. This study is the first to try and isolate, using econometric methods, the impact of the LRAA on those workers who were employed in the TES sector as the amendments came into effect. This was possible because of various unique features of the SARS-NT data not available in other nationally representative South African datasets, most importantly the ability to accurately identify (and track over time) workers employed by TES firms and the availability of reliable point estimates for earnings. The study therefore makes an important contribution to the South African research and policy agenda, and it also adds to the relatively small international literature that investigates the conditions of TES workers specifically.

The study made use of a regression discontinuity design to analyse the employment outcomes for workers who were employed in the TES sector just prior to the amendments coming into effect (i.e., the 2015 tax year), as the legislative reforms only applied to those earning below a threshold of R205,433. The results of the RDD suggest that the LRAA had very little effect on TES worker transitions in the labour market in the year after the amendments were implemented (i.e., in the 2016 tax year). Even though the descriptive analysis suggested that those below the threshold were more likely to transition into the non-TES sector and even more so into unemployment (or out of formal employment) than those above the threshold, there was no significant causal evidence in the RDD analysis to support this. In sum, there was no rigorous evidence to suggest that the LRAA resulted in TES workers being more likely to move into non-TES employment (potentially with client firms, as the legislation had intended) or that there were unintended disemployment effects of the legislative changes, at least not in the short-term.

The study also went further to explore the job characteristics of those TES workers that remained employed after the amendments were implemented (72% of the original sample). The results differed depending on whether the workers were retained in the TES sector (54% of the original sample) or moved to the non-TES sector (18% of the original sample). According to the RDD results, for those who were retained in the TES sector, job contract length was less likely to increase amongst the treated group (below the threshold) than the nontreated group (above the threshold). This might be signalling that some TES employers were trying to comply with the legislation by only leasing out workers who were actually performing temporary work for client firms, although the size of the effect recorded suggests this was likely to be quite limited. For those who moved from the TES into the non-TES sector in the 2016 tax year, there were some positive outcomes. Specifically, the treated group were more likely to experience an increase in real wages and job contract length than the nontreated group.

Overall then, the results point to quite limited effects of the legislative changes for TES workers. There was no significant impact on worker transitions, and somewhat improved working conditions only for the relatively small group that did move out of the TES sector into the non-TES sector. There is also no evidence to suggest that TES employers pre-empted the legislation and made hiring and firing decisions before the LRAA was formally implemented. A robustness check in which the analysis was repeated for the 2014/2015 tax years produced insignificant results. A possible explanation is that most TES employers did not comply with the legislation and simply continued leasing out workers to client firms on extended contracts. Compliance with labour legislation in South Africa has typically been low, as has been shown more recently in work by Bhorat et al. (2021) with respect to the introduction of the national minimum wage in 2019. In the South African context of high unemployment and a relatively small informal sector, workers may have also been willing to continue accepting TES jobs, regardless of the conditions of employment.

Of course there is the possibility that employers complied at a later stage once the intention and meaning of the legislation had become clearer, and changes to employment occurred only in subsequent years.[17] Unfortunately, these longer-term effects cannot be analysed with the existing SARS-NT data, but this is an important avenue of research to pursue in the future as more years of administrative data become available. We also recognise that, although we provide much more robust empirical evidence than previous studies, our analysis of the quality and nature of the jobs held by TES workers after the amendments were implemented is limited by the availability of information in the tax data. This serves as a useful

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reminder that labour force survey data remain an important source of information for empirical work in South Africa and should include more targeted questions that allow separate identification of atypical forms of work.

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Endnotes

[1] The literature on this topic is vast and beyond the scope of this paper to review extensively. See Bottasso et al. (2023) and Pijoan-Mas and Roldan-Blanco (2022) for a fuller discussion.

[2] Using the tax data for South Africa from 2011 to 2015, we found TES workers to comprise an average of 4.8% of total employment in tax-registered businesses over the period (Cassim and Casale 2022), a not insubstantial figure compared with the prevalence of such work in other countries. In Argentina, for example, 1.7% of employees were reported as being agency workers in 2005 (Drenik et al. 2023), and across a range of European countries the share ranged from 0.1% (in Greece) to 2.5% (in the Netherlands) in 2006 (Baumann et al. 2011).

[3] TES employees were also afforded little protection by the unions, partly because they were unable to organise at their physical workplace (viz., that of the client firm), which is an important part of how unions sign up members, conduct inspections and disclose information for the purposes of collective bargaining. TES employees were unable to process a demand calling on the client firm to bargain with them because by law they were not employed by the client firm. This also meant that different wages paid to TES and permanent workers (even if they were performing the same job) did not constitute 'proscribed discrimination' (Benjamin 2013, 126).

[4] The threshold value of R205,433.30 per annum amounts to US$16,074, using the average exchange rate of R12.78 for 2015. This compares to mean earnings of R90,287 per annum for workers in the TES sector and R176,983 per annum for workers in the non-TES sector using the 2015 tax data. Although not directly comparable (as it captures both formal and informal sector workers), the Labour Market Dynamics Survey of 2015 found mean earnings for employees to be R105,300 per annum (Statistics South Africa 2015b). The threshold was therefore set at a level that would have resulted in substantial coverage.

[5] The category includes 'labour recruitment and provision of staff; activities of employment agencies and recruiting organisations; hiring out of workers (labour broking activities)' as well as 'disinfecting and exterminating activities in buildings; investigation and security activities; building and industrial plant activities; photographic activities; packaging activities; other business activities; credit rating agency activities; debt collecting agency activities; stenographic, duplicating, addressing, mailing list or similar activities; other business activities'.

[6] Research in this area (largely in the European context) has examined whether strengthening EPL to protect temporary workers affects lower-skilled versus higher-skilled workers more, or whether temporary work more generally has differential impacts on workers depending on skill or seniority (see, e.g., discussions in Bottasso et al. 2023 and Garcia-Louzao et al. 2022). We are unable to explore these issues, as we do not have information on education, occupation or seniority in the job in the tax data.

[7] Details on the cleaning of the SARS-NT dataset are available in [Cassim and Casale 2022].

[8] Unfortunately, no such data on the triangular relationship between TES firm, TES worker and client firm exists for South Africa.

[9] We do not have information on hours worked in the tax data and are therefore unable to identify part-time workers either.

[10] We use a sharp rather than a fuzzy RD design because the LRAA applied to all workers earning below the earnings threshold and participation in the treatment was not optional (Cattaneo et al. 2019).

[11] The South African legislation effectively only applied to firms employing more than 10 workers, or more than 50 workers if the firm was younger than 2 years. However, these additional restrictions were not applied in this paper as there were no TES firms in the dataset with less than 10 employees, and additionally, the dataset did not include information on the age of firms. In 2015, for example, around 98% of employees in the dataset who worked in the TES sector, worked in firms that employed over 50 employees (Cassim and Casale 2022).

[12] Bandwidths were chosen using the Stata packages 'rdrobust' and 'rdbwselect' (Calonico et al. 2014).

[13] There are no substantial changes when a second-order polynomial is used.

[14] While the outcomes considered are binary in nature, a series of linear probability models are run, as the RDD package does not support probits or multinomial logits.

[15] As an additional exercise (data not shown here), the rate of real wage growth between 2015 and 2016 was used as an alternative dependent variable. Consistent with the main results that use the binary variable as the outcome, the RD estimates suggest that wage growth rates are lower for those above relative to those below the threshold. Larger growth rates were also observed for those that moved into the non-TES sector compared to those that stayed in the TES sector, which supports the result above that those that moved to the non-TES sector were better off following the LRAA.

[16] As for wages, the growth in job contract length between 2015 and 2016 was used as an alternative dependent variable to the binary variable used here. There were no substantive differences to the main results, and therefore they are not shown here.

[17] Indeed, TES employers and client firms may have been waiting to hear judgement on the Constitutional Court case of 2018 mentioned earlier which helped clarify the role of the TES firm versus the client firm (*Assign Services (Pty) Limited v National Union of Metalworkers of South Africa and Others [2018] CCT 194/17*).

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Appendix

TABLE A1 | Balancing covariates

	Age	Female	Job duration (baseline 2015)	Firm size (baseline 2015)
RD estimate	0.221 (0.403)	−0.061*** (0.022)	1.306 (4.784)	−245.049 (359.588)
N	338,895	338,962	338,962	338,962
Effective N (below T)	10,118	5,540	6,969	6,388
Effective N (above T)	4,533	3,446	3,630	3,708
Order of polynomial (p)	1	1	1.	1
Bandwidth in ZAR (h)	60,028	40,247.	51,844	44,645
Covariates	Yes	Yes	Yes	Yes

Source: Authors' estimates based on Employment Panel (National Treasury and UNU-WIDER 2019).
Note: Standard errors in parentheses. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Bandwidths are selected using the mean square error (MSE) estimator for the RD treatment effect. All specifications use a triangular kernel.
***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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