

The impact of employment protection on the temporary employment services sector

Evidence from South Africa using data from tax records

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Aalia Cassim*

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Abstract: Attempts to regulate the temporary employment sector have had mixed results internationally. In South Africa, temporary employment was regulated in 2015 through amendments to the Labour Relations Act. This paper uses administrative data to examine the short-term impact of strengthening employment protection legislation in the temporary employment services (TES) sector, focusing on employment, job duration, and wages. A regression discontinuity design is used as the amendments applied only to employees earning below a certain threshold. The findings suggest that while working conditions improved for those that transitioned out of the TES sector into the non-TES sector, a larger proportion of individuals moved out of the data into informal employment, unemployment, or economic inactivity after the amendments were implemented. Providing empirical evidence on the impact of the amendments is an important contribution to the debate on regulating labour markets in developing countries, particularly in South Africa given its considerable unemployment rate.

Key words: administrative data, employment protection legislation, regression discontinuity design, South Africa, temporary employment services

JEL classification: J31, J41

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1 Introduction

Temporary work has attracted much attention globally due to its precarious nature. However, given challenging economic environments and increasingly competitive markets, the demand for temporary work has increased. The growth in temporary work has, in part, been exacerbated by stricter employment protection for permanent work, with employers trying to circumvent regulations by making use of atypical working arrangements (Dolado et al. 2002; Zhou 2006). In these cases, employers hire temporary workers either through temporary employment service (TES) firms or directly on fixed-term contracts. The bulk of the literature that examines the interaction between temporary work and employment protection legislation (EPL) focuses on this aspect. There is a dearth of literature on the impact of *strengthening* EPL for temporary workers, particularly for TES workers, the subject of this paper. While there are examples of relatively strong EPL for TES workers in Belgium, France, Germany, and Mexico, there has been no empirical analysis of the effects of these particular policies. In part, this is driven by the lack of quality data that can provide insight into fixed-term and TES workers (Addison et al. 2019).

In South Africa, temporary work through TES, or labour brokers¹ as they are commonly known, has been a feature of the labour market since the 1800s in the mining industry. In 1994, at the turn of the country's transition into democracy, labour legislation was redrafted to level the bargaining power of workers with employers. However, legislative reform really focused on standard employment relationships, largely leaving atypical work relationships unprotected. At this point, there was a surge in labour broking, so employers could make use of flexible working relationships and circumvent some aspects of the new labour legislation (Theron 2014). In the late 2000s, after a call from the unions to ban labour brokers, the South African government began investigating the protection of atypical work relationships. The government took a decision to strengthen EPL for marginal workers—part-time, temporary, and TES workers—in 2015. For TES workers in particular, the new regulations suggested that they be treated no less favourably than the permanent workers of the client firm, and that temporary work be limited to contract lengths of three months. Essentially, Labour Relations Amendment Act No. 6 of 2014² (LRAA) was the government's attempt to regulate the triangular relationship between the client firm, the TES firm or labour broker, and the worker, which was subject to limited regulation prior to April 2015.

The introduction of this stricter regulation was not without contention, however, given that around a quarter of the labour force in South Africa is unemployed (and closer to 40 per cent when a non-searching definition of unemployment is used). Groups such as the Free Market Foundation, the Small Business Project, and the National Employers Association of South Africa raised opposition to the legislation, suggesting that it was inappropriate for small firms, which would not be able to pay the wages or meet the working conditions that large firms could afford (Darroll 2017). Further, it was suggested that stricter regulation of TES workers would disincentivize client firms from using TES firms and potentially result in a disemployment effect. Unions argued, however, that

¹ Workers in TES, as defined here, are employed by staffing agencies, where these agencies are ultimately responsible for the salary, taxes, and benefits of the leased employee. When a company (the 'client' firm) contracts with a staffing agency for temporary help, the company pays the staffing agency a set fee for the leased worker. TES workers can also be distinguished from seasonal, temporary, or part-time contingent workers, who typically can be employees of the company that hired them, and who are usually let go when the work is complete.

² Labour Relations Act No. 66 of 1995 is one of South Africa's overarching pieces of labour legislation. It has been amended many times to reflect the changing dynamics of the labour market and provide protection to different types of worker.

because temporary workers had been excluded from adequate employment protection in the past, these laws were vital to avoid people being trapped in low-paying jobs (COSATU 2012).

While the amendments came into effect five years ago, empirical assessment of the impact of the amendments has been limited, as until recently there was no reliable source of data on the TES sector. The quantitative studies that have examined the impact of the amendments have relied either on a small sample of TES workers that are not nationally representative (Bhorat et al. 2015) or on estimates of the TES workforce from labour force survey data (Bhorat and Lilenstein 2017). In terms of the latter, the Quarterly Labour Force Surveys (QLFS) do not allow the separate identification of TES employees, as TES employees are grouped together with other categories of workers in the Standard Industrial Classification system.

In 2015, the South African Revenue Service (SARS) and the National Treasury (NT) made company and employee income tax administrative data available for research purposes for the first time.³ This is the only South African dataset from the last decade that explicitly captures which firms are labour brokers and also contains individual employee wages. This paper uses these data to examine the short-term impact of the LRAA on the employment status, wages, and job duration of TES workers that were affected by the legislation. The panel nature of the data 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. The legislation applied to workers earning below the threshold of ZAR 205,433 in 2015, so a regression discontinuity design (RDD) is used. Briefly, the findings suggest that individuals that were employed in TES in 2015 and were below the threshold were more likely to move into the non-TES sector than those above the threshold relative to being retained in the TES sector. Further, working conditions in terms of job duration and earnings seem to have improved for those that transitioned into the non-TES sector. However, a larger proportion of workers employed in the TES sector in 2015 moved out of tax-registered firms altogether into either unemployment, the informal sector, or the economically inactive population after the amendments were implemented. The amendments to the legislation therefore appear to have had mixed results for the intended beneficiaries.

This paper is an important contribution to the literature for two reasons. First, there are not many cases in which stricter EPL is imposed on temporary work (as opposed to permanent work), and empirical research in this regard is therefore lacking. Second, the South African case is an interesting one, as stricter EPL has been imposed in an environment where unemployment is unsustainably high and the prospects for economic growth are 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 paper contributes to the debate on the trade-off between workers' protection through formal EPL and the potentially negative disemployment effects of raising the costs associated with employing TES workers. This is pertinent in light of the fact that South Africa strengthened the position of workers even further by introducing a national minimum wage in January 2019.

The rest of this paper is structured as follows. Section 2 discusses the theoretical and empirical literature. Section 3 provides the context of the legislative framework. Section 4 discusses the data. Section 5 presents the descriptive analysis. Section 6 explains the econometric approach. Section 7 presents the results. Section 8 concludes.

³There have only been a handful of research papers that have used these data in the past five years. The research has mostly covered job flows (Kerr 2018), the employment tax incentive (Chatterjee and Mcleod 2016; Ebrahim and Pirttilä 2019; Ebrahim et al. 2017), and wage inequality among employees (Bassier 2019; Bhorat et al. 2017).

2 Literature review

There is a dearth of literature on the impact of EPL on TES workers, so understanding the potential impact of legislative reforms on temporary workers requires us to go back to the circumstances under which temporary workers are hired and the factors that might result in the growth of temporary work. Section 2.1 on the theoretical literature therefore examines the incentives to firms to hire temporary workers. In deciding on the optimal composition of labour, firms will weigh up the EPL faced by permanent workers relative to temporary workers. In other words, the growth of temporary work is often contingent on the EPL or firing costs related to permanent workers.

Section 2.2 below reviews the empirical literature on the interaction between EPL and temporary work and the various outcomes that have been observed. The empirical literature in this area does not tend to differentiate between fixed-term employment or TES jobs. It should be noted that although the focus of this paper is on the latter, the literature reviewed in this section tends to use a broader definition of temporary work that includes both fixed-term employment and TES work.

2.1 Theoretical predictions

In order to understand the theoretical impact of stricter EPL on temporary workers, we first need to understand why firms use temporary workers or outsource labour. In the main, a temporary contract allows flexibility, which means that firms can swiftly respond to changes in the economy and labour market (Nunez and Livanos 2015). Also, it can be used to screen potential employees before committing to permanent hires (Autor 2001). Autor (2003) was among the first economists to ask why firms do not outsource all of their workers to avoid the firing costs (adjustment costs) associated with stricter EPL for permanent workers. The hypothesis explored by Autor (2003) was that by outsourcing employment, firms forgo specific human capital investments (Becker 2002) that would be invested in permanent workers. In addition, workers with shorter contracts make smaller specific capital investments. Therefore, in firms where specific capital is highly productive, firms may commit to permanent hires even though temporary employment offers lower firing costs.

Autor (2003: 8) considers a two-period model of employment ‘where the first period consists of hiring and specific capital investment and the second period consists of production’. Workers choose specific capital investments to maximize their ‘expected utility’, which is the difference between expected earnings and the cost of specific capital investments such as human capital investment in skills. Because firing costs reduce the odds of termination in the second period, workers make larger specific skill investments when firing costs are greater. Although firing costs raise the cost of terminating workers, they also raise the expected profitability of the workers who are retained by increasing their incentives to make specific skill investments. This means that for occupations where specific capital is quite productive, stronger EPL for permanent workers is unlikely to induce outsourcing, because firms will have already written contracts with substantial firing costs, and outsourcing these jobs may discourage investment. However, for occupations where specific capital is less important, any increase in firing costs may be sufficient to result in employment outsourcing. Hence, the model suggests that firms will primarily respond to legislated firing costs or stricter EPL for permanent workers by outsourcing those occupations that require the least specific capital.

Another point that Autor (2003) makes is that EPL will only have an impact on a firm if the EPL exceeds the firm’s own firing costs. Therefore, if legislated firing costs exceed optimal firm firing costs for permanent workers, a subset of firms will find it more profitable to outsource

employment despite the forgone specific capital investment. But in the case that legislated firing costs for temporary workers exceed or are equivalent to firm firing costs for permanent workers, firms are less likely to outsource part of their workforce. To put it differently, if strict EPL (higher firing costs) is imposed on temporary workers, then it disincentivizes firms from hiring them.

Further, when firing costs for permanent workers are high and there are rules forbidding temporary contract renewal, Bratti et al. (2018) suggest that firms might be reluctant to convert temporary jobs into permanent ones. This may incentivize firms to rely instead on a number of temporary employees one after the other, increasing worker turnover (Cahuc and Postel-Vinay 2002).

Lastly, in relation to the potential impact on TES workers specifically, Baumann et al. (2011) point out that the impact of EPL may differ for agency workers versus fixed-term workers who are contracted directly by the firm. The main difference is that client firms pay a fee for TES workers over and above the worker's wage. This means that when the client firm is making a hiring decision, it will consider the firing costs for temporary workers (relative to the firing costs for permanent staff) as well as the fee paid to the TES firm. If the fee and firing costs exceed the firm's own firing costs, then there is little incentive to hire TES workers. In the event of higher firing costs being imposed for TES workers, Baumann et al. (2011) further suggest that TES agency profits may decline, as the fee paid by client firms to the TES firm is offset by the higher firing costs borne by TES firms.

2.2 Empirical evidence

While a few countries have relatively strict EPL in place for TES workers, there are no empirical studies that examine the impact of strengthening EPL for TES workers specifically. In a related study, however, Cahuc et al. (2019) consider the impact of taxing fixed-term contracts in France, Italy, Portugal, and Spain, where taxation has been introduced to varying degrees. Often the tax is introduced to contribute to social benefits such as unemployment insurance, but it raises the cost to employers. Their main finding is that the taxation reduces mean job duration, decreases job creation, and increases unemployment for those who are on fixed-term contracts. This is because firms anticipate letting workers go in order to avoid paying higher costs on their contracts in the future. The authors suggest that while the regulation of temporary contracts means that the jobs are more secure until the contract comes to an end, it also induces greater job turnover.

In the main, the literature tends to focus on the consequences of strengthening EPL for permanent workers on atypical work arrangements, without differentiating between fixed-term and TES workers (Baumann et al. 2011). Since the early 1990s, EPL in developed countries has eased the hiring and firing of temporary workers but has left strict rules in place for permanent hires (Dolado et al. 2002; Zhou 2006). Zhou (2006) found that this type of reform, which was termed 'partial reform', fostered both job creation and job destruction for temporary workers. This was because strengthening EPL for permanent workers often incentivized employers to make use of atypical work arrangements to circumvent legislation, but they were also less likely to make temporary workers permanent, so job turnover increased. Hijzen et al. (2017) similarly found that the asymmetric liberalization of temporary work while stringent regulations for permanent contracts remained in place encouraged firms to substitute temporary for permanent workers. They found that in Italy, for instance, these types of amendment increased the incidence of temporary work, particularly among larger firms, and accounted for around 20 per cent of the incidence of temporary work.

Further, Kahn (2010) conducted a cross-country review of the effect of EPL on temporary work in Europe and found that policies making it easier to create temporary jobs on average increased

the likelihood that workers would be in temporary jobs, particularly when the unemployment rate was high, as workers were in a relatively weaker position. Consistent with these findings, in Spain Dolado et al. (2002) found that the deregulation of temporary work resulted in an increased share of temporary employment overall. Similarly in France, the deregulation of temporary work resulted in fewer transitions of temporary to permanent employment, as employers were more likely to employ a series of workers on fixed-term contracts given the lower cost of fixed-term workers relative to permanent workers (Blanchard and Landier 2002). In other parts of the developed world such as Japan, the deregulation of temporary work is also positively correlated with the replacement of permanent workers with temporary workers in domestic production (Machikita and Sato 2011). In addition, it has been found that industries losing their world share of value added tend to more greatly decrease their relative share of employment of permanent workers, highlighting the role of temporary workers as an employment buffer.

Unionization, another form of worker protection, has also been found to increase the relative share of temporary employment, because employers prefer hiring workers that are not unionized and therefore have less bargaining power (Devicienti et al. 2018). However, in a highly volatile economic environment, unions may be concerned about the weakening of their bargaining power associated with the extensive use of temporary workers.

There have also been reforms that have resulted in employers moving away from temporary employment. In Italy, it was found that the reform of fixed-term contracts, which aimed to make it easier to hire temporary employees, induced a substitution of temporary employees for permanent ones (Cappellari et al. 2012). Essentially, the reforms intended to broaden the reasons allowed for firms to use temporary employment, worded more generally as reasons of a ‘technical, organizational, production or replacement nature’ (Cappellari et al. 2012: 11). Previously there were specific reasons why firms could use temporary employment, including for example when workers were on sick leave, or during an increase in production. However, the reform had a high degree of uncertainty attached to it that may have added an additional burden to the law, as too much would be dependent on a judge’s interpretation of the law (Aimo 2006). The uncertainty around whether hiring on a fixed-term contract could result in court action reduced the incentive to use these types of temporary contract (Venn 2009).

Besides changing the composition of employment at a firm, strengthening EPL for permanent hires can also have an impact on training and job duration at the firm. Research from Italy has shown that as the share of temporary workers in a firm grows in response to stricter EPL for permanent hires, fewer workers at the firm receive training (Autor 2003; Hijzen et al. 2017). Further, it has been found that job duration for low-skilled workers is reduced, while job duration for skilled workers increases (Cahuc and Carcillo 2011). However, where changes to EPL in Italy resulted in more flexibility by increasing the maximum period of employment in TES agencies, there was a positive impact on the length of employment for TES workers (Antoni and Jahn 2006). This was particularly the case for marginal workers, including disadvantaged groups, poorly qualified workers, unemployed persons, foreigners, and young workers. However, average labour market job duration declined, as firms opted for temporary contracts (which tended to be shorter) instead of permanent hires.

While the international empirical papers reviewed here do not mirror the case study being examined in this paper, particularly since they do not differentiate between types of atypical employment, they still provide useful insights into the dynamics of EPL and how they may directly or indirectly impact on the incidence and nature of temporary work. On the one hand, stricter EPL and unionization for permanent hires and the deregulation of temporary work tend to increase the incidence of temporary workers. In these cases, firms tend to reduce training, and contract durations become shorter. On the other hand, stricter EPL for temporary hires and

uncertainty around the regulations governing temporary workers can reduce the incidence of temporary work. Although this has not been studied empirically in the case of TES firms specifically, stricter EPL for temporary workers might mean that the TES agency fee paid by the client firm is offset by higher firing costs, which in turn would likely mean that TES firms would provide fewer services, i.e. fewer workers. Notably, the literature reviewed above is based on developed countries, as there have been no quantitative studies on the impact of legislative reform on TES workers (or fixed-term contract workers) in developing countries outside South Africa.

2.3 South African evidence

As mentioned in Section 1, there have been only a few quantitative studies that have examined the impact of the 2015 LRAA in South Africa. Bhorat et al. (2015) use survey data collected by the Confederation of Associations in the Private Employment Sector (CAPES) on a cross-section of industries in four provinces for two periods, i.e. March/April 2014 (before the legislation was implemented) and March/April 2015 (just as the legislation became binding). CAPES collected the employment statuses of TES workers across various client firms a year before the amendments and then immediately after the amendments were implemented. Bhorat et al. (2015) found that the primary effect of the amendments in South Africa that strengthened temporary workers' rights was disemployment. Half of the TES workers were terminated by the labour brokers, and around a quarter of employees (in the full sample) were made permanent or taken on contract by the client firm. The rest were retained by the TES firms. Negative effects were felt most strongly in manufacturing, finance, real estate and business services, and public and social services, and in Gauteng and KwaZulu-Natal (relative to Western Cape and Mpumalanga). Positive employment effects were felt most strongly in the wholesale and retail trade and tourism industries, and in Gauteng and the Western Cape. While this study provides useful insights into the impact of the LRAA, the sample used is not nationally representative, and the analysis is purely descriptive, presumably relying on a manager's response regarding TES employment in 2014 and 2015.

In a different study, Bhorat and Lilenstein (2017) used the QLFS to examine the impact of the amendments descriptively. However, there is no way to separately identify TES workers in the survey, so the authors analysed the category of workers captured as being in 'businesses not elsewhere classified' (Standard Industrial Classification 889). This category includes TES workers as well as workers in other sectors.⁴ The authors found that there was a sudden decline in jobs in this category in the first quarter of 2015, but there was a reversion to pre-amendment employment levels by the first quarter of 2017. While this was a useful exploratory exercise, caution should be used, as the broad industrial classification overestimates the number of workers in the TES sector specifically, and may overestimate the impact of the amendments. Further, as the authors themselves recognized, since 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).

There have also been qualitative studies examining the amendments. The Small Business Project used focus groups to interview 18 firms including employers of TES workers within the mining, manufacturing, finance, insurance, and petroleum industries, and employer associations for TES

⁴ Standard Industrial Classification 889 includes 'labour recruitment and provision of staff; activities of employment agencies and recruiting organizations; 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' (Bhorat et al. 2016).

workers between 2016 and 2017 (Darroll 2017). They asked nine questions regarding employers' understanding of the LRAA, the potential impact of the LRAA, compliance with labour laws, and whether the economic environment was suitable for these types of amendment. The industry respondents as well as those from TES firms broadly suggested that they had witnessed not only reductions in employment, but also fewer opportunities for the generation of future employment, particularly for young people. Lastly, they suggested that the LRAA would drive more firms to invest in capital rather than apply the new regulations.

Joubert and Loggenberg (2017) undertook semi-structured interviews about the perceptions of employees regarding the potential impact of the LRAA in the petroleum and chemicals sector. While this is not specified, the paper suggests that interviews took place in the calendar year 2015, after the implementation of the LRAA. A purposive sampling technique was used in which six participants were identified. The participants must have worked in the human resources department of an integrated petroleum and chemical company, and the company must have made use of labour brokers. Four questions were put to the participants regarding the potential positive and negative impacts of the LRAA on the organization and its employees. The potential negative outcomes of the LRAA that respondents reported included cost implications for the client firms, the administrative burden of the new policies and guidelines, and less employment flexibility for employers. The respondents suggested that there was a threat of unemployment for TES employees, while permanent employees felt that they would have to take on more work. The potential positive impacts identified by the respondents included being able to choose better-quality workers directly rather than getting less-skilled workers through a labour broker, and more job security for low-skilled workers that were retained by the firm.

This review suggests there are two gaps in the literature. First, none of the studies in South Africa have been able to examine the impact of the LRAA using a nationally representative sample of labour broker firms or labour broker workers specifically. Second, none of the studies have used a rigorous econometric analysis that attempts to isolate the impact of the LRAA on employment outcomes and explore conditions of work for employees that are retained in employment. This paper tries to fill these gaps by examining the impact of the LRAA on employment status as well as conditions of work for those who remained employed, using a large sample of TES workers from national-level data. The study is not only a useful contribution to the South African literature on the impact of the LRAA, but more broadly it is a contribution to the international literature on the impact of regulating temporary or atypical work, as there are few cases of this being examined empirically.

3 Background to legislative amendments in South Africa and hypothesized effects

In 1983, labour broking was added to Labour Relations Act No. 28 of 1956, merely allowing employees to employ workers through temporary agencies but not limiting the period for which workers could be placed. Theron (2005: 7) suggests that this was part of the reason for the growth in the TES sector: '[the Labour Relations Act] did not, in other words, specify what was "temporary" about a supposedly temporary employment service'. In the absence of a collective agreement, Theron (2014) suggests that employers were able to pay workers a fraction of what they paid permanent workers for doing the same work, which increased inequality and fragmentation in the workplace.

Just after South Africa transitioned to its first democratic government in 1994, the Labour Relations Act of 1995 was promulgated (and enforced from November 1996), which aimed to consolidate the workers' rights that had been negotiated in the previous two decades. At the same time, the Commission for Conciliation, Mediation, and Arbitration (CCMA) was formed to deal with labour disputes. At this point, there was a surge in labour broking, particularly of lower-skilled workers, in part so that employers could circumvent the newly adopted labour legislation (Theron 2014). A similar trend was observed in the United Kingdom and United States, where employers would reduce the number of permanent workers first by subcontracting portions of their business to service providers, and second by increasing the number of atypical workers to do their core work through labour broker firms (Brown and Sessions 2005; Houseman 2014; Segal and Sullivan 1996). When the International Labour Organization attempted to regulate 'contract work' in the late 1990s, it faced substantial pushback from employers and ultimately did not make the regulatory amendments it had hoped to make. This made it difficult for South Africa to consider regulating temporary work, even though it was clearly undermining labour relations (Theron 2014).

The decision by the South African government to increase employment protection for temporary workers followed a call by organized labour⁵ to ban labour brokers in 2008. The National Union of Mineworkers (South Africa) and the Congress of South African Trade Unions (COSATU) were two of the labour organizations that were strongly opposed to labour broking, and the abolishment of labour broking is a long-standing demand noted in their policy documents (COSATU 2018).

In 2012, the Labour Relations Amendment Bill added four new sections (198A–D) to deal with three categories of non-standard or atypical employee: TES employees, employees on fixed-term contracts, and part-time employees. The amendments were announced by the Labour Ministry in 2012 with the objective that the new legislation would come into effect in 2014. However, the LRAA was only published in August 2014 and became effective on 1 January 2015. It specifically targeted employees who earned below ZAR 205,433.30 per annum,⁶ which is the Basic Conditions of Employment Act (BCEA) threshold. The provisions in the act governing TES only came into effect on 1 April 2015, after the expiration of a three-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

⁵ In 1990 the Tripartite Alliance was formed, which was an alliance between the African National Congress (the current ruling party), the South African Communist Party, and COSATU. The alliance was key to negotiating the transition to a democratic government, and the unions have since had a close relationship with the ruling party.

⁶ This was equivalent to US\$13,803 in February 2020. The threshold has not changed since 2015.

employed to perform a genuine ‘temporary service’ for a client. If that was not the case, the employee would be deemed to be an employee of the client firm, not the TES firm.

Section 198A of the LRAA states that ‘temporary service’ is defined as work for a client by an employee:

- a. for a period not exceeding three 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 three-month period referred to in point (a) started on the effective date of the LRAA, which was 1 January 2015. An employee working for a client for longer than three months would be deemed to be permanent unless conditions (b) and (c) 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 would now become legally liable for the well-being of the TES employee. In other words, the client would be legally obliged to treat TES employees no differently from their permanent staff, even if the TES workers were not on the client firm’s payroll. It is assumed that this meant that the TES employee would be entitled to similar remuneration and benefits to the client’s other employees doing the same or similar work. However, this was not stated explicitly in the legislation. The legislation did not apply to TES firms with fewer than 10 employees, or TES firms with fewer than 50 employees that had been in existence for less than two years.

The legal case *Assign Services (Pty) Limited versus 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 three months. The matter was first heard at the CCMA in 2015, then taken to the Labour Court and Labour Appeal Court. Subsequently, the Constitutional Court upheld the Labour Appeal Court’s decision that after three 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 including working conditions, work allocation, and performance assessment would be conducted by the client (Milo 2018). This also meant that if the employee sought relief in terms of features of the LRAA such as unfair dismissal, they would seek relief against the client firm.

The legislative amendments essentially made the conditions around hiring and firing temporary employees more stringent and less flexible. While the objective of the amendments was to eliminate the exploitation of vulnerable temporary employees that were kept on rolling temporary contracts, the limited flexibility might also have disincentivized client firms from using TES agencies. In terms of the cost to the clients, prior to the amendment they were paying the labour broker an amount that included the wage for the TES employee as well as the markup that covered the fee to the labour broker. However, after the three months, the legislation suggested that if the client wanted to continue hiring the same worker through the TES agency, clients would have to pay the previous amount, plus (we assume) an additional amount that covered the higher wages and benefits afforded to their permanent workers, potentially making the cost of hiring through an intermediary more expensive than hiring workers directly. This would make it more attractive to hire workers directly, as the client would have a less flexible role in the triangular employment relationship with the employees and the TES/labour broker firm following the amendments.

Given the scope of the amendments, and the findings in the theoretical and empirical literature reviewed in Section 2, the hypothesized effects on employment, earnings, and job duration—the three key outcome variables of interest in this study—are as follows:

- **Employment:** the amendments might have led to fewer workers employed in the TES sector, because of the reduced flexibility and higher firing costs associated with the use of temporary workers through TES agencies. Further, when firing costs for non-temporary workers are high and there are rules forbidding temporary contract renewal, Bratti et al. (2018) suggest that client firms might be reluctant to convert temporary jobs into permanent ones, resulting in unemployment of workers previously employed through TES agencies. However, workers might also have been absorbed by client firms as per the intention of the amendments, in which case we would expect to see employment in non-TES/client firms increase following the legislative amendments (although the international literature suggests that this is less likely). This would depend on the value the client placed on the worker.
- **Earnings:** if workers remain employed, we might expect to see the earnings of TES employees increase, for two reasons. First, TES employers might have raised earnings above the threshold of ZAR 205,433 to avoid the legislation altogether. Second, if employers complied with the LRAA, then client firms may have taken on TES workers permanently, and instead of paying TES agencies a markup fee they may have paid these workers more, in line with what they paid their permanent workers. This would result in higher wages post-amendment for those who remained in the TES sector and those that moved into the client/non-TES sector.
- **Job duration:** while the intention of the legislation was to do away with short fixed-term contracts, it may have resulted in contract duration being shortened to under three months by client firms, as then the legislation would not apply to these contracts. However, if client firms complied with the LRAA, then contract durations might be expected to increase, because only workers doing actual temporary work could be employed on short-term contracts. Alternatively, we might see no impact on contract length, as employers might have used a sequence of different workers with short contracts, instead of rolling short-term contracts for the same workers (Bratti et al. 2018).

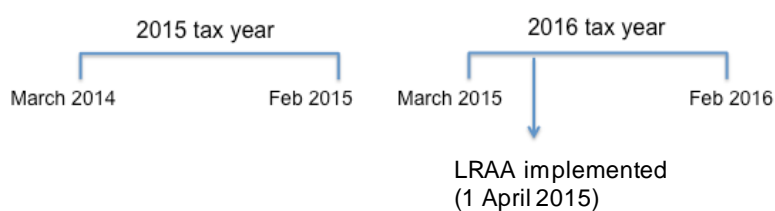
Given these potentially opposing effects, the overall outcome is ambiguous and becomes an empirical question for testing.

4 Data and sample

This paper uses an unbalanced employee panel dataset made available by SARS and the NT for the tax years 2015 (i.e. 1 March 2014 to end February 2015) and 2016 (1 March 2015 to end February 2016)⁷—the years before and after the LRAA was implemented (Figure 1). The dataset was created from employee income tax certificates submitted by employers (IRP5 and IT3(a)) to SARS (Ebrahim and Axelson 2019). The unit of analysis is essentially the job contract level, as the dataset includes records of employment for taxpaying firms over the period. However, the data can be collapsed to the individual level, as the records also contain a person ID number.

⁷ The years in the IRP5 panel refer to the period from 1 March of the previous year to the end of February of that year, regardless of a firm's financial year. Pieterse et al. (2016) show that 85 per cent of firms have their financial year at the end of February.

Figure 1: Timeline of tax data and LRAA



Source: author's illustration.

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 paying tax on behalf of its employees. This eliminates the problem of misreporting of sector or type of employment, which is common in household or employee surveys, as discussed in Cassim and Casale (2018). It is important to point out that in the data, it is only possible to see whether an individual is employed 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.⁸ Unfortunately, this limits the analysis to a certain degree, as we would like to be able to identify more precisely whether TES workers moved to the client firm itself post-amendment, whereas we are only able to identify whether workers moved into the broader category of the non-TES sector (i.e. all non-TES firms). To be able to unpack the triangular relationship between TES firm, client firm, and worker in more depth would require a different kind of data structure which is not available in the tax data (and more than likely would require purposive primary data collection where TES firms were matched to various client firms).

Nonetheless, the dataset remains unique in that it allows us to accurately identify workers that are employed by TES/labour broker firms. In addition, the panel nature of the data allows us to track individuals over time, which means we can identify transitions between the TES and non-TES sectors after the amendments were imposed. 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, which forms part of the 2016 tax year (Figure 1). This means that although the timing for the analysis is not perfect (the 2016 tax year starts in March), it is a very close match.

In any one tax year, individuals can be employed on more than one job contract. For example, in the 2015 tax year, there were around 399,466 individuals employed on 459,762 TES contracts. Since we are interested in changes in employment outcomes just after the amendment was implemented, we restrict the sample to those individuals whose last contract of the 2015 tax year (i.e. the last contract before the reform) was in the TES sector. Of the 399,466 individuals that had TES contracts in 2015, 340,673 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

⁸The bulk (around 75 per cent) of labour broker firms classify themselves in the finance and business services sector, which includes financial services, insurance, real estate, and other administrative support services. However, since the client firm cannot be identified, the data do not allow us to identify the industrial sector the TES worker is actually employed in. We know from other research that TES workers are employed by client firms in a broad range of sectors including chemicals, clothing and textiles, communications, construction, health, local government, metal and engineering, mining, motor vehicles, printing and packaging, retail, road freight, and transport (Budlender 2013).

on the sample whose last contract of 2015 was in the TES sector, we may be overlooking some changes that occurred prior to implementation on 1 April 2015 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 7.4, we conduct a separate analysis in which we test the outcomes for the 2014 and 2015 tax years to try to gauge whether employers pre-empted the legislation.

For the sample of individuals (340,673) whose last contract was in the TES sector in the 2015 tax year, we then analyse their employment outcomes in the 2016 tax year (i.e. after the reform). In particular, we explore three main outcomes: employment status, earnings, and job duration. First, we analyse whether they remained employed in the TES sector, transitioned into the non-TES sector, or are no longer in the dataset. If they are no longer in the dataset, this implies they either became unemployed, were not economically active, or were possibly working but for unregistered/informal firms. For those who remain in the dataset and were therefore employed in the formal sector in the 2016 tax year, we then track whether the nature of their employment, in particular their earnings and job contract duration, changed.⁹

Since this study uses the discontinuity at the ZAR 205,433 earnings threshold to identify the impact of the LRAA, the accurate measurement of earnings is critical. In the labour legislation, the earnings threshold refers to gross income before deductions including tax, pension, and medical benefits. Each IRP5 form reports gross non-retirement fund income (taxable income not related to pension contributions), non-taxable income (including arbitration awards, purchased annuities, travel reimbursements, subsistence allowances, uniform allowances, and other allowances), and gross retirement income (gross remuneration from an employer used to calculate allowable pension fund contributions). For labour broker employees, the sum of these three variables provides the total earnings for a specific job contract.¹⁰

Given that the sample is limited to those whose last contract in the 2015 tax year was in the TES sector, earnings from only that last job are analysed. This becomes a problem when a person has more than one contract in the same firm and their total earnings at the firm for that year (summed over all their contracts) are higher than the BCEA threshold, even though for their last contract the total earnings are lower than the threshold. Take the example of a person with four contracts at the same firm in one year, with each contract for ZAR 60,000. Taking just their last contract of the year, this person will fall into the sample of workers who are below the BCEA threshold, as their last contract of ZAR 60,000 is below the threshold of ZAR 205,433. However, their total earnings at the firm for that year will amount to ZAR 240,000 (ZAR 60,000 multiplied by four), which means they fall above the threshold. Luckily, there are only 60 individuals (0.0002 per cent of the sample) for whom this is the case, and they do not have a material impact on the analysis.

⁹ For workers who were employed on more than one job contract in the 2016 tax year, we focus on the first job contract of the tax year, as we are interested in what happened to these workers just after the amendments became enforceable.

¹⁰ For simplicity we use the term ‘total earnings’, but more specifically this variable represents total gross earnings, as it still includes the tax portion.

5 Descriptive statistics

Table 1: TES employee transitions between 2015 and 2016

		2016			
		TES	Non-TES	Out of data	Total
2015 TES	Below threshold	173265	61291	93256	327812
		52.85%	18.70%	28.45%	100.00%
	Above threshold	10024	1081	1756	12861
		77.94%	8.41%	13.65%	100.00%

Notes: 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 ZAR 205,433.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

Table 2: Differences in job characteristics before and after implementation

	Pre-reform		Post-reform TES (2016)			Post-reform non-TES (2016)		
	N	Mean	N	Mean	Diff.	N	Mean	Diff.
Below threshold								
Probability of real earnings increasing in 2016			170137	56.99%		60133	58.71%	
Real earnings	323521	ZAR 43532.61	170137	ZAR 52272.09	20.08%	60133	ZAR 110273.5	110.96%
Probability of job duration increasing in 2016			164159	61.19%		59446	46.81%	
Job duration	322097	261.55	169115	294.21	12.49%	56737	256.54	-1.92%
Above threshold								
		Pre-reform (2015)		Post-reform TES (2016)			Post-reform non-TES (2016)	
	N	Mean	N	Mean	Diff.	N	Mean	diff
Probability of real earnings increasing in 2016			13152	42.37%		2239	30.68%	
Real earnings	17152	ZAR 523387.40	13152	ZAR 463933.80	-11.36%	2239	ZAR 292247.40	-44.16%
Probability of job duration increasing in 2016			12340	62.97%		2203	43.35%	
Job duration	17148	333.51	13120	295.09	-11.52%	2169	255.71	-23.33%

Notes: the differences (diff.) are statistically significant at the one per cent level. Earnings are deflated using 2016 as the base year. The sample is at the individual level. The threshold is ZAR 205,433. Job duration is based on number of days.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

In this section, descriptive statistics are presented on the outcomes of interest that are analysed further in Section 6. The transition matrix in Table 1 shows whether TES workers employed in 2015 remained in the TES sector, transitioned to the non-TES sector, or moved out of the data altogether in 2016. Those that moved out of the data were likely to be unemployed, economically inactive, or working for firms not registered for tax. The figures show that while the bulk of those that were in the TES sector in 2015 were employed in the TES sector again in 2016, of those that were not retained, a larger proportion of individuals moved out of the data than transitioned to the non-TES sector. This holds for those above and below the threshold. Second, if we compare those above and below the threshold, retention in the TES sector is about 25 percentage points higher for those above the threshold compared with those below the threshold (78 per cent versus 53 per cent). Those above the threshold were much less likely to transition to the non-TES sector (eight per cent versus 19 per cent) or to transition out of the data (14 per cent versus 29 per cent) relative to those below the threshold.¹¹ This suggests that those above the threshold were in a far less precarious employment position.

Table 2 examines the job characteristics, namely earnings and job duration, 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. Focusing on the earnings of those below the threshold (the upper panel of the table), we find that while the probability of real earnings increasing 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 per cent 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 per cent versus 20 per cent). For those above the threshold (the lower panel of the table), less than half of those that stayed in the TES sector and around a third of those that moved to the non-TES sector saw an increase in real earnings between the 2015 and 2016 tax years (42 per cent versus 31 per cent), and on average real earnings declined for both groups between 2015 and 2016. Those above the threshold that moved into the non-TES sector had a much more substantial decline in average real earnings than those that were retained in the TES sector. If we compare those below and above the threshold, regardless of whether they moved into TES or non-TES in 2016, 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.

If we focus on job duration¹² for those below the threshold (upper panel of Table 2), job duration increased for more than half of individuals retained in TES but less than half of those who moved to the non-TES sector (61 per cent versus 47 per cent). Those retained in TES also saw a higher increase in average job duration relative to those that moved to non-TES, where average job duration declined (13 per cent versus minus two per cent). Similarly, for those above the threshold (lower panel of the table), more than half of those retained in TES experienced an increase in job duration, compared with less than half of those that moved to non-TES (63 per cent versus 43 per cent). Mean job duration, however, declined for both groups, although more so for those that moved into non-TES (-12 per cent versus -23 per cent). Overall, Table 2 suggests that those below

¹¹ Kerr (2018) uses the same tax data to show that worker flows—the sum of hires and separations—constituted around 52 to 54 per cent of average employment in the period between 2012 and 2014. This means that more than 50 per cent of those employed either left their current employer and/or arrived at their current employer in each year. Table 2 similarly shows that worker flows are substantial in the labour broker sector and thus seem to be a pervasive part of the South African labour market. Kerr (2018) also shows that worker flows are higher for those in the lower income quintiles.

¹² Job duration refers to the job contract length captured in the tax data, and is measured in days (with the maximum being 365, or one year).

the threshold that moved to non-*TES* benefitted the most in terms of earnings, while those below the threshold that were retained in *TES* benefitted the most in terms of job duration.

6 Econometric strategy

This section describes the regression discontinuity (RD) strategy used. It explains first why a sharp RDD is used; second, how we assess the validity of the RDD; and third, whether the covariates used are balanced on either side of the threshold.

6.1 Sharp RDD

A sharp RDD is used to estimate the impact of employment protection on *TES* workers' labour market status, wages, and job duration after the LRAA was enforced. A key 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 threshold 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 ZAR 205,433 earnings threshold. A sharp RDD is one in which the treatment condition assigned is identical to the treatment condition actually received for all units (Cattaneo et al. 2019). In the case of the LRAA, the treatment applied to all individuals earning below a certain threshold, and therefore there is no case in which the treatment is not taken up. An advantage of the RDD is that it relies on relatively weak assumptions compared with other non-experimental approaches, and consequently provides more credible results. In addition, the assumptions are testable in a similar manner to randomized experiments (Hijzen et al. 2017).

Studies examining the impact of employment protection on temporary work have also used a difference-in-difference (DID) strategy to isolate the impact of EPL amendments on temporary employees.¹³ However, a DID identification approach is not suitable in the South African case, as the LRAA applied to *TES* firms as well as non-*TES* firms that had fixed-term and part-time workers. Thus non-*TES* firms or their employees cannot be used as the control group, as those that were part-time or on fixed-term contracts were also affected by the legislation. A key challenge with this approach is potential spillovers between the treatment and control groups. One of the motivations for the LRAA was to encourage non-*TES* firms (which would be the control group) to hire people directly instead of through *TES* firms (which would be the treatment group). If this were the case, then the estimated impact in terms of a disemployment effect would be larger than it actually is, since the control group would break the parallel trends assumption of DID.

A few papers use a sharp RDD identification strategy similar to the one pursued in this paper (Bratti et al. 2018; Hijzen et al. 2017). The methodological difference between their work and this paper is that in the South African case the legislation applied to those earning below a certain level of income, instead of to firms above a certain size threshold as is the case in most other countries studied.

We employ a non-parametric local polynomial estimation method from Calonico et al. (2014) that is commonly used in RDD analysis (Bratti et al. 2018; Kantorowicz and Hlobil 2020). Using the non-parametric tool from Calonico et al. (2014) means that the bandwidth is chosen using a data-

¹³ See for example Autor (2003) in the case of the United States, as well as cross-country studies from Damiani et al. (2016) and Pierre and Scarpetta (2013).

driven process on the basis of non-parametric approximation.¹⁴ 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 larger 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. Bandwidths are identified in the data-driven process noted above to minimize the mean squared error (MSE) of the RDD estimator. This is preferred to the parametric method, which is based on an ad hoc chosen bandwidth and assumes away misspecification bias (Bratti et al. 2018).

After selecting the bandwidths, we run a weighted¹⁵ least squares regression to examine a number of outcomes for those who were employed in the TES sector prior to the amendments: the probability of having a certain labour market status in 2016; the probability of wages increasing between 2015 and 2016; and the probability of job duration increasing between 2015 and 2016.

The specification is as follows:

$$Y_{it+1} = \alpha + \tau(D_i) + \beta_1(E_{it} - T) + \beta_n(E_{it} - T)^n + \beta_3 D_i (E_{it} - T) + X_{it} + \varepsilon_{it} \quad [1]$$

$$D_i = 1[(E_{it} < T)]$$

$$T - h \leq E_i \leq T + h$$

Y_{it+1} is a binary variable representing the outcome in 2016. E_{it} is earnings in 2015. T is the threshold of ZAR 205,433. $(E_{it} - T)$ refers to the normalized forcing variable, and we also include a polynomial of the forcing variable. D_i is a dummy that equals one 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 (as well as the statistical programmes) assume that those above the threshold are treated, so RD estimates are reported in terms of the outcome for those above relative to those below the threshold. In this case, those below the earnings threshold are the treated group, but the RD estimates are presented in terms of those above relative to those below the threshold, which follows the standard approach in terms of RD analysis. Under a sharp RDD, the average treatment effect (τ) at the threshold (T) is the difference between the estimated parameters from the regression for those above (non-treated) relative to those below the threshold (treated).

$$\tau = E[Y_i(1) - Y_i(0)|E_i=T]$$

The RD estimates presented below provide the relative impact of the amendments, i.e. the impact on those above relative to those below the threshold within the specified bandwidth. Controls include age, gender, job contract duration, and firm size in 2015. h refers to bandwidths estimated, which provide the window around the earnings threshold.¹⁶

¹⁴ Bandwidths were chosen using the Stata packages ‘rdrobust’ and ‘rdbwselect’ (Calonico et al. 2014).

¹⁵ Each observation is weighted equivalent to $K\left(\frac{E_{it}-T}{h}\right)$, in which K is the kernel. Cattaneo et al. (2019: 43) recommend a triangular kernel because ‘when used in conjunction with a bandwidth that optimizes the MSE, it leads to a point estimator with optimal properties’. The triangular kernel function assigns zero weight to all observations with a score outside the bandwidth, and positive weights to all observations within the bandwidth.

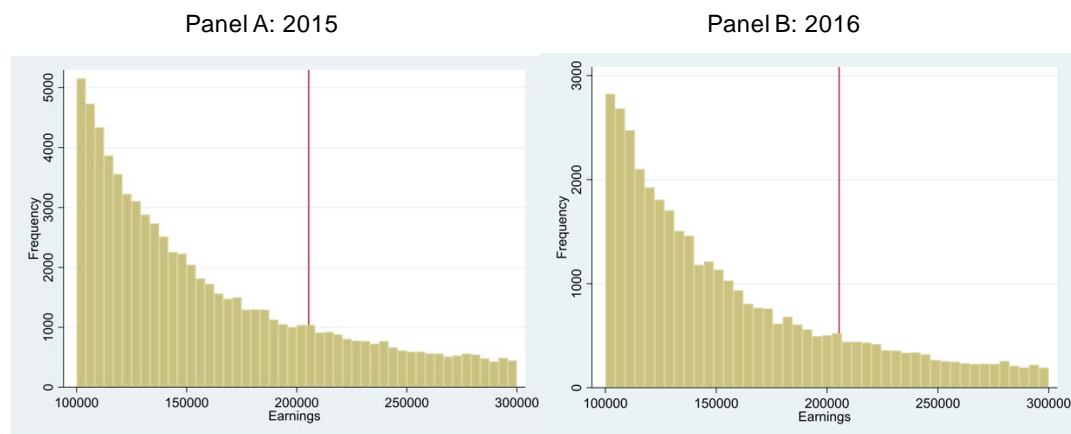
¹⁶ While the outcomes considered are binary in nature, a series of linear probability models are run to examine a combination of outcomes, as the RDD package does not support probits or multinomial logits.

6.2 Assessing the validity of the RD

One of the challenges with this analysis is to accurately distinguish between the independent effect of the amendments on earnings and the possibilities 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. Because the reforms prescribe that TES workers should be treated similarly to permanent workers in that they should be paid similar wages, an increase in wages may imply compliance with the legislation. However, if we see a number of workers moved 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. Figure 2 presents the distribution of real earnings for the group of TES workers in 2015 (left) and in their subsequent employment in 2016 (right). A visual inspection does not suggest a jump in earnings to just above the ZAR 205,433 threshold.

Figure 2: Histogram of average earnings for 2015 and 2016



Notes: the y axis presents the number of IRP5 contracts in a particular bin. The BCEA threshold is marked by the red line.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

The transition matrix in Table 3 shows that of those that were below the threshold in 2015, around 68 per cent remained below the threshold, just under four per cent moved above the threshold, and around 29 per cent moved out of the data. There was a larger proportion of individuals who moved from above the threshold in 2015 to below the threshold in 2016 (around 25 per cent). Given that so few individuals moved above the threshold, it is unlikely that there was a substantial manipulation of earnings that would bias the results.

Table 3: Transitions across the threshold (2015–16)

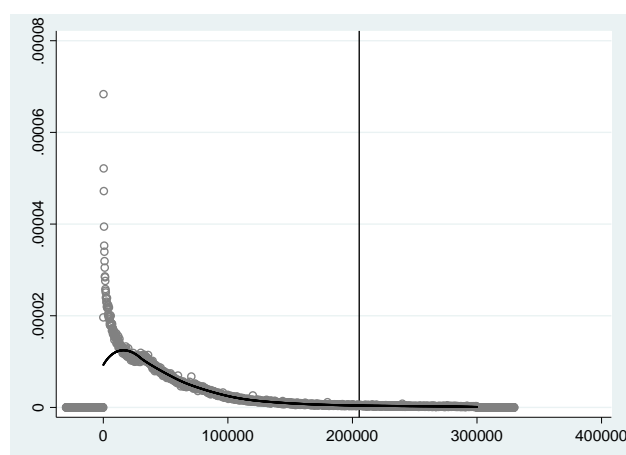
		2016			
		Below	Above	Out of data	Total
2015	Below	219071	11199	93251	323521
	<i>Share</i>	<i>67.71</i>	<i>3.46</i>	<i>28.82</i>	<i>100%</i>
	Above	4254	11137	1761	17152
	<i>Share</i>	<i>24.8</i>	<i>64.93</i>	<i>10.27</i>	<i>100%</i>

Notes: the sample is at the individual level. The threshold is ZAR 205,433.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

To examine this further, we run a McCrary (2008) test, 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 visualized in Figure 3, with separate weighted kernel estimations and 95 per cent 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 (standard error 0.0270), which is statistically insignificant.¹⁷ We cannot reject the null, 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.¹⁸

Figure 3: McCrary test



Notes: the x axis presents earnings in ZAR, and the ZAR 205,433 threshold is marked by the black line. The bin size is 138.99.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

¹⁷ The t-stat is 0.5, with a p-value >0.05, and therefore the null cannot be rejected using a five per cent significance level as the cut-off.

¹⁸ To further examine the sensitivity of the results to observations around the threshold, doughnut hole tests are run, and the results are reported in Section 7.

6.3 Balancing covariates test

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). Given the limited data in the SARS-NT panel, only the following covariates can be analysed: age, gender, job duration, and firm size. 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 non-parametric approach. The results are presented in Table A1 in the Appendix.

All estimates besides the coefficient on *Female* are insignificant when we use a polynomial of the first order.¹⁹ The significant coefficient on the *Female* dummy variable means that the probability of being female is lower for those above relative to those below the threshold. In order to determine whether the covariate that is significant will bias the analysis, the weighted least squares regression (Equation 1) is run with and without covariates in Section 7. From this it is found that the results are generally consistent whether or not we include covariates.

7 Results

For each outcome variable, the tables showing the regression results (Tables A2 to A10 in the Appendix) report the RD coefficient and standard error, the number of observations, the effective number of observations to the left and right of the threshold (within the bandwidth), the order of the polynomial (one or two), and the bandwidths estimated or selected. Equation 1 is estimated with and without covariates and using a polynomial of orders one and two, in all cases using the optimally specified bandwidths (columns 1 to 4). Our preferred specification is in column 3, which includes covariates and uses a polynomial of order one.²⁰

In order to test the sensitivity of the results to the response of units very close to the threshold, doughnut hole tests are run (Cattaneo et al. 2019) and shown in columns 5 and 6. The idea behind the doughnut hole regressions is to exclude observations close to the threshold that are most likely to be manipulated and then repeat the estimation. Cattaneo et al. (2019) suggest running this estimation a few times where the percentage of excluded observations around the threshold is varied. In the tables, we show the estimations where those that earned one per cent and three per cent above and below the threshold (which amount to around ZAR 2,000 and ZAR 6,000 respectively) are excluded. The tests are run using the bandwidth selected in our preferred specification from column 3 for the sake of comparison.

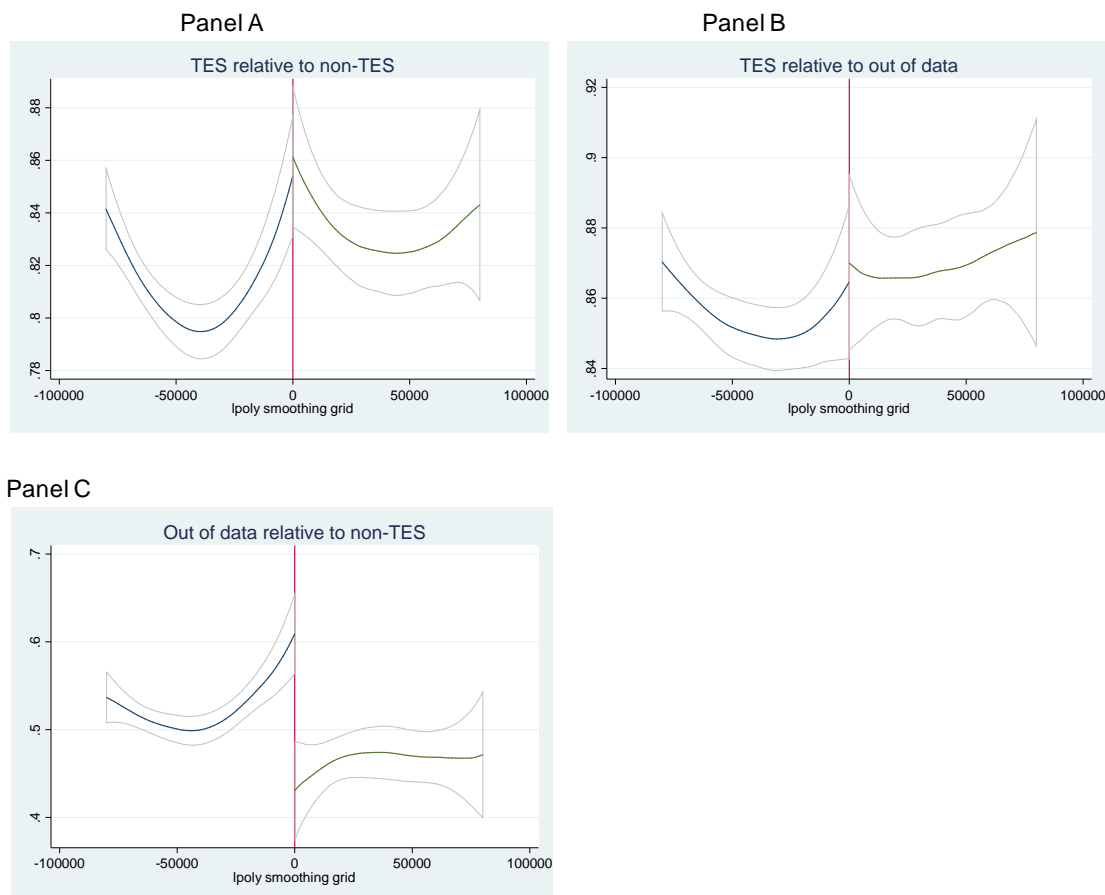
¹⁹ There are no substantial changes when a second-order polynomial is used.

²⁰ Cattaneo et al. (2019) suggest that higher-order polynomials tend to produce over-fitting of the data and lead to unreliable results near boundary points. They go on to say that researchers prefer the local linear RD estimator, which is also the default point estimator in most applications.

7.1 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 2016 of the sample of workers whose last contract of 2015 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; (iii) the probability of moving out of the data relative to being in non-TES. As described under the hypotheses in Section 3, if conditions of hiring a temporary worker become more stringent, employment of temporary workers is likely to decline as they are absorbed by the non-TES sector or become unemployed (Cahuc and Carcillo 2011). Figure 4 presents graphical evidence of the outcome variables to show whether there was a discontinuity at the threshold. This gives a sense of the expected outcomes in the regression analysis. The figure shows a small discontinuity in panels A and B, but a clear and substantial discontinuity in panel C. Figure 4 does not control for any of the covariates discussed above, nor does it use the optimal bandwidth that would be selected through a non-parametric analysis. When we present the RD estimates below, consistency between Figure 4 and the regression analysis will be discussed.

Figure 4: Polynomial plot depicting labour market outcomes in the 2016 tax year



Notes: the graphs are kernel-weighted local polynomial smoothing plots. The threshold is marked by the red line. The x axis presents the normalized forcing variable in ZAR. The y axis provides the probability of the labour market outcomes. All figures use a bandwidth of ZAR 80,000 on either side of the threshold.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

In terms of the first outcome (Table A2), the consistently positive RD estimates suggest there is a higher probability of being retained in TES relative to non-TES for those above (the non-treated) relative to those below the threshold (the treated); however, none of the estimates are statistically

significant. This result is consistent with what was observed in the transition matrix in the descriptive statistics in Section 5, and with the graph displayed in panel A of Figure 4. 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 in the doughnut hole tests in columns 5 and 6 are largely consistent with the comparable baseline result in column 3 (i.e. 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 terms of the second outcome (Table A3), 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. This result is also consistent with the transition matrix (Table 1), which suggested that those above the threshold (the non-treated) were in a less precarious position than those below the threshold, as would be expected. However, the RD coefficients are very small and not significant, suggesting only a marginal difference in the probability of being retained relative to being out of the data between those above and below the threshold. Panel B in Figure 4 also presents a very small discontinuity. The estimates from the doughnut hole tests in columns 5 and 6 (three per cent) are similarly small (in fact, close to zero) and insignificant.

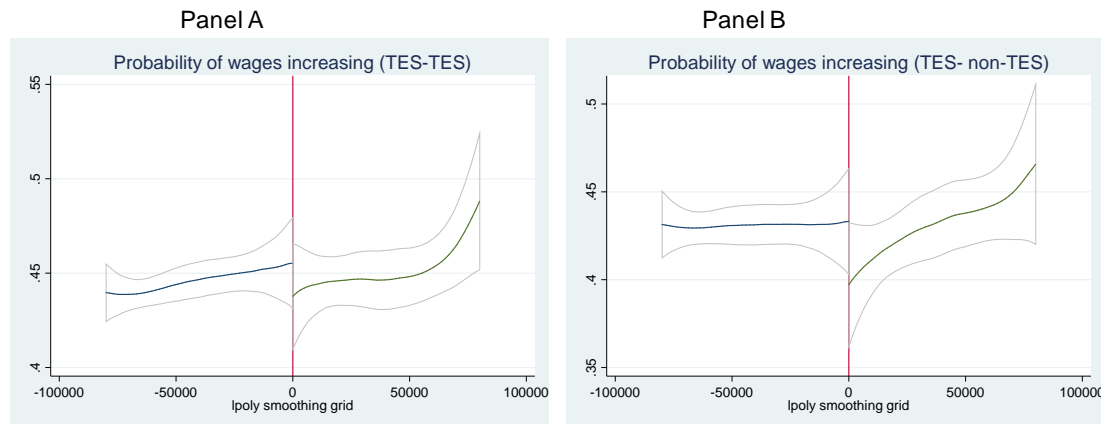
In terms of the third outcome (Table A4), the RD estimates yield a negative coefficient, which suggests that the probability of being out of the data relative to being employed in the non-TES sector in 2016 is lower for those above relative to those below the threshold. To put it differently, those below the threshold—the treated group—are more likely to move out of the data than into the non-TES sector, relative to the non-treated group. These results yield higher RD estimates than the previous outcomes and are consistent across specifications, but are still not statistically significant. The results are also consistent with the transition matrix (Table 1) and the graphical results in Figure 4, panel C. The doughnut 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 was 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, as TES workers would have to be treated as though they were permanent by the client firm, and the client firm would also become legally liable for the TES workers. So one might expect that the client firm would employ the TES workers on its own books. The results in this analysis suggest that there may have been some small positive outcome of the LRAA policy, as 2015 TES workers below the threshold were 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. We assume that this is contrary to the effect the legislation intended to achieve. So while the amendments may have led to some positive outcomes for those that moved to the non-TES sector, there was a greater probability that workers moved out of the data (i.e. out of formally registered tax firms) the year after the amendments. None of the effects were statistically significant, however, so the evidence is very tenuous.

7.2 The impact of the amendments on wages

In this section, the impact of the LRAA on wages is assessed. Specifically, the probability of wages increasing is examined for those above relative to those below the threshold. The outcome variable is a binary variable that equals one if wages increased between 2015 and 2016, and zero if wages stayed the same or declined. The results are presented separately for those that were retained by the TES sector in 2016 and those that moved to the non-TES sector in 2016. As in the previous section, graphical evidence is provided in Figure 5, and the RD estimates are presented in Tables A5 and A6.

Figure 5: Polynomial plot depicting the probability of wages increasing between 2015 and 2016



Notes: the graphs are kernel-weighted local polynomial smoothing plots. The threshold is marked by the red line. The x axis presents the normalized forcing variable in ZAR. The y axis provides the probability of wages increasing. All figures use a bandwidth of ZAR 80,000 on either side of the threshold.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

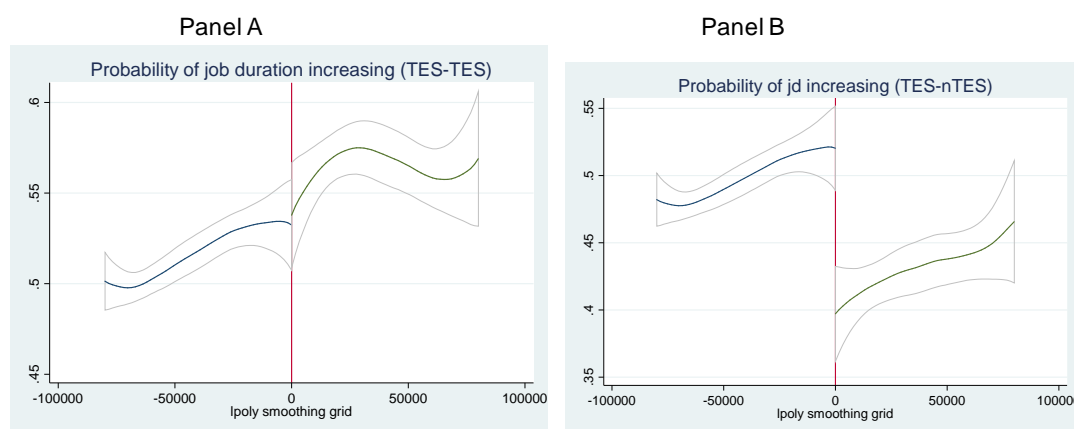
Both panels of Figure 5 suggest a discontinuity at the threshold; however, the discontinuity is slightly larger for those that moved from TES into non-TES (panel B) compared with those that remained in TES (panel A). The negative RD estimates in Tables A5 and A6 are in line with the results in the descriptive analysis in Table 2 and the graphical depiction in Figure 5. They indicate that the probability that real wages increased is lower for those above relative to those below the threshold. In other words, 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 and insignificant, and when the doughnut hole tests are conducted the effect tends to zero. For those that moved to the non-TES sector, however, the estimates are significant and quite sizeable, indicating that the probability that wages increased is 11 to 14 per cent 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 non-treated group. Above-inflation wage increases for this group might suggest that there was some level of compliance with the legislation, if those that moved to the non-TES sector were moved onto the client firm's payroll. 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.²¹

²¹ 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 suggested that wage growth rates were 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 with those that

7.3 The impact of the amendments on job duration

The next set of estimations considers the short-term impact of the LRAA on job duration, since the objective of the legislation was to do away with rolling short-term contracts. The same set of specifications as above are estimated, but the dependent variable is a binary variable which equals one if the individual experienced an increase in contract duration between 2015 and 2016, and zero if the contract duration declined or stayed the same after the amendments.²² As above, the results are differentiated for those that remained in TES and those that moved to non-TES. Figure 6 suggests that there were different effects for those that were retained in TES (panel A) and those that moved to non-TES (panel B). This is further explored in the regression analysis in Tables A7 and A8.

Figure 6: Polynomial plot depicting the probability of job duration increasing between 2015 and 2016



Notes: the graphs are kernel-weighted local polynomial smoothing plots. The threshold is marked by the red line. The x axis represents the normalized forcing variable in ZAR. The y axis is the probability of job duration increasing. All figures use a bandwidth of ZAR 80,000 on either side of the threshold.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER 2019).

For those that were retained in the TES sector, the descriptive statistics (Table 2) and panel A of Figure 6 indicate that the probability that job duration increased between 2015 and 2016 was marginally higher for those above the threshold relative to those below the threshold. The positive RD estimates in Table A7 are consistent with the descriptive statistics. When covariates are included (columns 3 and 4), the RD estimates are significant and suggest that for those retained in TES, the probability that job duration increased between 2015 and 2016 was 3.4 to 8.9 per cent higher for those above relative to those below the threshold. The doughnut hole tests are largely consistent with the baseline results, which indicates that even when we remove those close to the threshold, the result holds.

For those that moved into the non-TES sector (Table A8), the negative and significant coefficients on the RD estimates across specifications mean that the probability that job duration increased between 2015 and 2016 was 13 to 16 per cent lower for those above relative to those below the threshold. This is consistent with the descriptive results as well as Figure 6 (panel B). In other

stayed in the TES sector, which supports the result above that those that moved to the non-TES sector were better off.

²² As with wages, job duration growth between 2015 and 2016 was used as an alternative dependent variable to the binary variable used here. There were no substantive differences from the main results, and therefore we do not show them here.

words, the treated group did better than the non-treated group in terms of job duration only if they moved into the non-SES sector.

Overall, the results suggest that of the 2015 SES workers that remained employed in 2016 (70 per cent of the full sample), only the treated group (i.e. those below the threshold) that moved into the non-SES sector were better off. They had a higher probability of both earnings and job duration increasing relative to those above the threshold. The results also suggest that those just around the threshold are not driving the outcomes observed.

7.4 Further examination: did employers pre-empt the legislation?

Informal interviews with labour brokers conducted in June 2014²³ suggested that they were already seeing the impact of the legislation, with a number of client firms no longer using their services. In order to examine whether employers pre-empted the amendments and acted prior to the implementation of the LRAA on 1 April 2015, an additional exercise is undertaken in which we run the RDD analysis one year prior, i.e. 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 2014 was in the SES sector is used. The dependent variables are analogous to those above: the labour market status of the individual in 2015, the probability of earnings increasing between 2014 and 2015, and the probability of job duration increasing between 2014 and 2015.

The labour market status estimations, presented in Table A9, are run using our preferred specification, i.e. including covariates and a polynomial of order one. In contrast to the main set of results, these results suggest that SES workers below the threshold in 2014 were more likely to be retained in the SES sector than to move to the non-SES sector relative to those above the threshold. They were also more likely to be retained in the SES sector than to move out of the data relative to those above the threshold. Similar to the results for 2015 to 2016, however, SES workers in 2014 were more likely to move out of the data than to move into the non-SES sector. Again, these results are insignificant and the coefficients are small, so we cannot draw any strong conclusions about labour market transitions in the years prior to the amendments.

The results in Table A10 present the probability of wages and job duration increasing for those that were retained in SES and those that moved to non-SES between 2014 and 2015. The probability of wages increasing is consistent with those for 2015 to 2016 in that those below the threshold were more likely to see an increase in earnings relative to those above the threshold. However, now the only significant result is for those that were retained in the SES sector (rather than for those that moved to the non-SES sector, as with the 2015 to 2016 sample) (Table A10, column 1). If we focus on job duration (columns 2 and 4), the probability of job duration increasing is higher for those below relative to those above the threshold, regardless of whether workers were retained in SES or moved to non-SES, but neither of the coefficients is significant.

While these results might suggest higher levels of retention in SES for workers below the threshold in 2014 to 2015 relative to 2015 to 2016, because the coefficients are so small and insignificant, it is not possible to conclude that employers acted differently prior to the amendments. The finding on wages and job duration might suggest that even prior to the amendments, those below the threshold that moved into the non-SES sector had a higher probability of wages and job duration

²³ Informal interviews were undertaken as part of the author's research for Borat et al. (2016), and were conducted with three SES firms in Gauteng.

increasing (as coefficients are larger compared with those that were retained in the TES sector). However, these results are insignificant. While employers may have pre-empted the legislation and acted prior to April 2015, there is no evidence in our data to suggest that this was so. It is also possible that we cannot pick up the results using the same method and earnings threshold in the year before because the ZAR 205,433 threshold had not yet been specified.

8 Conclusion

The legislative amendments in 2015 were a 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 less flexible. Short-term contracts could not be renewed on a continuous basis, and temporary employees had to be treated similarly to the permanent employees of the client firm. This study is the first to try to isolate, using econometric methods, the impact of the EPL on TES workers. An RD was used, as the legislative reforms applied to those earning below the threshold of ZAR 205,433.

In terms of labour market outcomes, the direction of the coefficients suggests that treated TES workers (i.e. those below the threshold) were more likely than the non-treated to move to the non-TES sector or out of the data than to be retained in the TES sector just after the amendments were implemented. However, of the two outcomes, treated TES workers were more likely to move out of the data than into the non-TES sector. This suggests that the disemployment effect was larger than the intended effect of the legislation, which was ideally to transfer workers to the non-TES sector or the client firm. However, because the coefficients are not significant in any of the specifications, the most we can say is that any changes were likely very small, at least in the short term.

The analysis then went further to explore the job characteristics of those that remained employed after the amendments were implemented (70 per cent of the original sample). The results differed depending on whether the workers were retained in the TES sector or moved to the non-TES sector. Among those that moved to the non-TES sector (20 per cent of the full sample of TES workers in 2015), the treated group seemed to be better off, with the probability of wages and job contract duration increasing being significantly larger than for the non-treated group. While workers being absorbed by the non-TES sector was in line with the intention of the amendments, it may mean that working conditions improved for those who did move into the non-TES sector at the expense of the larger proportion of workers that moved out of the formal sector altogether in the 2016 tax year. It should be noted, however, that the data do not allow us to tell whether those that moved into the non-TES sector were absorbed by client firms directly or moved into other firms in the non-TES sector. This means the results may also be picking up that the non-TES sector has better-paying jobs than the TES sector (Cassim and Casale 2018). This suggests that TES firms may be a stepping stone into better employment, and perhaps TES employment serves as a screening tool for employers in the client/non-TES sector, as has been found internationally (Booth et al. 2002; Graaf-Zijl et al. 2011; Ichino et al. 2008).

Given that the labour market status results were largely insignificant, it is possible that when we control for other factors, the amendments had a very limited impact on worker transitions, but improved conditions for the relatively small group that did move out of the TES sector into the non-TES sector. The limited impact in terms of labour market status might be because employers did not comply with the legislation when it was enforced in the 2016 tax year, and changes to

employment (if any) only happened later.²⁴ They may have only complied in 2018 after the Constitutional Court judgement in the Assign Services case clarified the role of the client firm in terms of the amendments. Unfortunately, the updated TES data for the later tax years (2017 and 2018) are not yet available, but this would be an interesting area for future research.

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²⁴ The analysis run for the 2014 to 2015 tax years does not suggest that employers pre-empted the legislation and acted prior to the amendments being implemented, as these results were also found to be largely insignificant.

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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	338895	338962	338962	338962
Effective N (left)	10118.0	5540.0	6969.0	6388.0
Effective N (right)	4533.0	3446.0	3630.0	3708.0
Order of polynomial (p)	1.0	1.0	1.0	1.0
Bandwidth (h)	60027.5	40247.0	51843.6	44645.0
Covariates	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Bandwidths are selected using the MSE estimator for the RD treatment effect. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A2: Probability of retention in TES relative to non-TES in 2016

	1: No covs, $p=1, h=MSE$	2: No covs, $p=2, h=MSE$	3: Covs, $p=1, h=MSE$	4: Covs, $p=2, h=MSE$	5: DH: 1%, $p=1, h=col\ 3$	6: DH: 3%, $p=1, h=col\ 3$
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	245661	245661	244545	244545	243596	243379
Effective N (left)	5854.0	6943.0	9133.0	6946.0	8637.0	8509.0
Effective N (right)	3403.0	3672.0	4073.0	3617.0	3620.0	3531.0
Order of polynomial (p)	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	45789.5	51334.0	61435.8	51752.7	61435.8	61435.8
Covariates	No	No	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A3: Probability of retention in TES relative to moving out of data in 2016

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	292297	292297	290699	290699	289724	289490
Effective N (left)	5467.0	9101.0	6363.0	9278.0	5845.000	5709.0
Effective N (right)	3323.0	4182.0	3513.0	4150.0	3056.0	2958.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	43189.9	60327.7	48584.0	61645.3	48584.0	48584.0
Covariates	No	No	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A4: Probability of moving out of data relative to transitioning into non-TES in 2016

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	143388	143388	142546	142546	142275	142275
Effective N (left)	5187.0	4143.0	5541.0	3388.0	5397.0	3864.0
Effective N (right)	1356.0	1258.0	1340.0	1133.0	1213.0	1087.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	90596.6	80636.7	93626.0	72612.8	93626.0	93626.0
Covariates	No	No	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A5: Probability of wages increasing (TES to TES) between 2015 and 2016

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	183289	183289	182415	182415	182055	181414
Effective N (left)	5591.0	8924.0	10451.0	10874.0	10261.0	9917.0
Effective N (right)	3071.0	3743.0	3935.0	3991.0	3765.0	3468.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	51274.6	68523.0	75156.1	76689.9	75156.1	75156.1
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A6: Probability of wages increasing between 2015 and 2016 (TES to non-TES)

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	62372	62372	62130	62130	62082	61965
Effective N (left)	1252.0	3230.0	1123.0	3018.0	1098.0	1033.0
Effective N (right)	579.0	871.0	550.0	838.0	527.0	475.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	48578.1	87027.9	45584.3	84203.8	45584.3	45584.3
Covariates	48029.1	69564.5	57360.5	73556.5	57360.5	57360.5

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A7: Probability of job duration increasing between 2015 and 2016 (TES to TES)

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	176499	176499	175669	175669	175337	174750
Effective N (left)	3444.0	6847.0	10411.0	4605.0	10021	9925.0
Effective N (right)	2349.0	3248.0	3792.0	1904.0	3549	3359.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	38574.8	61530.9	78497.1	68855.6	78497.1	78497.1
Covariates	No	No	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A8: Probability of job duration increasing between 2015 and 2016 (TES to non-TES)

	1: No covs, p=1, h=MSE	2: No covs, p=2, h=MSE	3: Covs, p=1, h=MSE	4: Covs, p=2, h=MSE	5: DH: 1%, p=1, h=col 3	6: DH: 3%, p=1, h=col 3
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	61649	61649	61463	61463	61415	61302
Effective N (left)	2326.0	3997.0	1454.0	3754.0	1429.0	1367.0
Effective N (right)	771.0	945	617.0	904.0	594.0	543.0
Order of polynomial	1.0	2.0	1.0	2.0	1.0	1.0
Bandwidth (h)	73284.6	97047.86	55408.6	94669.3	55408.6	55408.6
Covariates	No	No	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10. All regressions are calculated using the Calonico et al. (2018) rdrobust routine for Stata. Specifications 1 and 2 are run without covariates (covs), while the rest are run with covariates including age, gender, job duration, and firm size in 2015. In models 1 to 4, bandwidths are selected using the MSE estimator for the RD treatment effect. Models 5 and 6 apply the same bandwidth as specification 3, which is the preferred specification. All specifications use a triangular kernel.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A9: Labour market status in 2015

	1: TES relative to non-TES	2: TES relative to out of data	3: Out of data relative to non-TES
RD_Estimate	-0.008 (0.015)	-0.006 (0.017)	-0.045 (0.038)
N	215724	273163	129613
Effective N (left)	6387.0	4687.0	2708.0
Effective N (right)	3394.0	2860.0	1131.0
Order of polynomial	1.0	1.0	1.0
Bandwidth (h)	55779.1	43749.1	77758.8
Covariates	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 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) and a polynomial of order one, and using a triangular kernel. A sample of 310,953 individuals whose last contract in 2014 was in the TES sector is used.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).

Table A10: Probability of wages and job duration increasing between 2014 and 2015

	TES to TES		TES to non-TES	
	1: Wage	2: Job duration	3: Wage	4: Job duration
RD_Estimate	-0.045* (0.024)	-0.015 (0.025)	-0.072 (0.059)	-0.019 (0.055)
N	174440	130352	41284	38938
Effective N (left)	6161.0	5072.0	830.0	1241.0
Effective N (right)	3074.0	2250.0	467.0	571.0
Order of polynomial	1.0	1.0	1.0	1.0
Bandwidth (h)	59081.5	68426.0	58115.4	77723.0
Covariates	Yes	Yes	Yes	Yes

Notes: standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 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) and a polynomial of order one, and using a triangular kernel. A sample of 310,953 individuals whose last contract in 2014 was in the TES sector is used.

Source: author's estimates based on Individual Panel v2018_2 (National Treasury and UNU-WIDER, 2019).