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Evidence on the impact of minimum wage laws in an informal sector: Domestic workers in South Africa☆

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ABSTRACT

What happens when a previously uncovered labor market is regulated? We exploit the introduction of a minimum wage in South Africa and variation in the intensity of this law to identify increases in wages for domestic workers and no statistically significant effects on employment on the intensive or extensive margins. These large, partial responses to the law are somewhat surprising, given the lack of monitoring and enforcement in this informal sector. We interpret these changes as evidence that strong external sanctions are not necessary for new labor legislation to have a significant impact on informal sectors of developing countries, at least in the short-run.

1. Introduction

What happens to wages and employment in the informal sector after the introduction of a minimum wage in that sector? The vast minimum wage literature in economics has remarkably little to say on this question, since the informal sector has more often represented the uncovered sector in this research and has been used to help distinguish between models of the labor market. For example, if the wage and employment effects of a change in the formal sector minimum are mirrored by opposite-signed responses in the informal sector, this is consistent with a segmented two-sector labor market model.[1] However, if informal sector wages increase and employment decreases when a formal sector minimum is adjusted upwards, this is consistent with spillovers in an integrated labor market, or possibly with "lighthouse effects".[2] In this paper, we extend the minimum wage literature and investigate whether a minimum wage floor can have any effect when directly applied to the informal sector, where the institutional environment for monitoring and enforcement of penalties is weak. We use the introduction of the first minimum wage in South Africa's domestic worker sector in November 2002 to analyze what can happen in the short-run.

Whether a minimum wage can have any impact in the informal sector is broadly relevant for understanding more about the process of labor market formalization in developing countries. As economies develop, labor relationships shift from rural to urban areas and take place in larger and larger firms; employers and employees start to pay taxes and workers gain legal protections, often including a guaranteed minimum rate of pay. Such protections, when enforced, may significantly improve working conditions and reduce poverty among the least skilled workers (Lustig and McLeod (1997) summarize results from several studies), or may hinder the operation of markets

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[1] See Brown (1988) for a discussion of two sector models in minimum wage studies. Brown (1999), Card and Krueger (1997) and Neumark and Wascher (1995, 2007) provide comprehensive reviews of the large minimum wage literature.

[2] Spillover effects into the informal sector or other formal uncovered sectors have been documented by many researchers, particularly in Latin American countries. See Lemos (2009) for Brazil, Gindling and Terrell (2004) for Costa Rica, Maloney and Menendez (2004) for a variety of Latin American countries, Bell (1997) for Mexico and Colombia. See also Cortes (2004) for the USA. Brown (1988) notes that in the US, a large fraction of workers earn the minimum wage, even though they are employed by establishments not subject to the minimum wage law.

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and negatively affect productivity (as in Besley and Burgess, 2004) and employment.[3] For many countries contemplating such regulation, resources for monitoring and enforcement are limited. And, since most economic models of employer behavior require some penalty and a non-zero probability of being audited to predict any effects of minimum wage regulations (see Ashenfelter and Smith (1979) for the canonical model of minimum wage compliance), a first order question for many developing countries is what effects can be expected from new labor legislation in the face of limited monitoring and enforcement?[4]

In this paper, we shed light on the effects of new labor regulation in a context with little active enforcement and no clear penalties. We present an empirical example in which domestic work employers face an extremely high minimum wage (set at the 70th percentile of the pre-law wage distribution) with no effective penalties and a vanishingly small probability of being audited, and show that employers still choose to respond to the law. We document immediate, large and partial adjustment of wages upwards in the wake of the law and find no statistically significant effects on the intensive or extensive margins of work, at least in the short-run, sixteen months after the law is enacted.[5] We also see evidence of dramatic increases in the fraction of domestic workers who have a formal contract of employment, unemployment insurance coverage and employer-provided pension contributions after the law. This South African case indicates that the effects of labor legislation may not rest on the type of enforcement that exists in already formalized markets; rather, the introduction of the law itself may serve as a focal point for shifting markets in the direction of becoming more formal.[6]

The domestic work sector is important in its own right, and is under-studied given its prevalence over space and time. Historically, this sector has been important in developed countries. Rubinow (1906) uses census data to show that over 1.2 million women were employed in domestic work in the US in 1900. World-wide, the market for domestic workers currently employs many millions of women: foreign workers employed in private households make up around 10% of the labor force in a number of Middle Eastern countries (Kremer and Watt, 2006); foreign domestic workers constitute 6% of the workforce in Hong Kong (Cortes and Pan, 2009); while ILO data for OECD countries record an average of 100,000 female domestic workers per country. The UK and Germany are at the high end of this range with 400,000 and 460,000 female domestic workers respectively. In South Africa, about one in three working women are employed in domestic work.

In most of these labor markets, the domestic work sector fits the ILO's definition of 'informal': the majority of firms are one-employee enterprises (households) in which labor relations are predominantly uncontracted and workers do not enjoy minimum wage protections or other non-wage benefits like unemployment insurance or pensions. The small scale of employers makes this sector costly for unions to organize.[7] Work often extends beyond simple housekeeping services and can require a great deal of trust, particularly when child-care is involved or when the employer is absent during working hours. Additionally, and particularly in Asia, Latin America and the USA, domestic workers are often foreign migrant workers with tenuous legal status. This increases their vulnerability in the labor market.[8] For example, in the USA, domestic workers are largely undocumented and are not yet guaranteed all of the protections of the National Labor Standards Act.[9] We believe our analysis of the South African case sheds light on the short-run effects of introducing minimum wage legislation to these informal and uncovered work relationships that are found throughout the world.

The empirical exercise in this paper is straightforward. We evaluate the effects of South Africa's 2002 minimum wage law for domestic workers by exploiting time-series variation in the application of the law and pre-existing cross-sectional variation related to the intensity of the law to identify wage and employment effects. We use labor force survey (LFS) data from 2001 to 2004 to capture worker-reported wages, hours of work and employment every six months. Large shifts in the wage and earnings distributions of domestic workers are evident in our non-parametric kernel densities. Exploiting only the before-after variation, domestic worker wages increase by about 20% in the 16 months after the law. Although this sharp jump in wages in a relatively short period of time is strongly suggestive of the new law having had an impact in this sector, we are naturally concerned that contemporaneous shocks to the economy, or differential economic trends might show up as similarly large wage increases. We complement the before-after analysis with a difference-in-differences strategy that adopts the methods in Lee (1999) to statistically examine the effects of the law.[10] Specifically, we compare the change in wages and hours of work of domestic workers in places where the median wage was far below the wage floor in the pre-period (high wage gap areas), to places where the median wage was closer to the minimum (low wage gap areas), thereby combining cross-sectional and time-series variation in the application of the law. We find that wages increase by a statistically significant 13–15% in the wake of the law. In contrast, we find no statistically significant reduction in hours of work nor any significant change in the probability of a low-skilled female worker being employed as a domestic worker in the pre-versus post-period, in high wage gap compared to lower wage gap areas.

Although the minimum wage law as it was enacted exposed all urban areas at the same time, we make use of the fact that the law is more demanding of employers in urban areas with lower pre-law wages.[11] And, as in any difference-in-differences research design that exploits a change in policy at one point in time, a key identification assumption is that both the exposed and unexposed groups are

[3] In that paper, Besley and Burgess (2004) show how active and costly pro-worker regulation in the formal manufacturing sector in India led to increased informality, reduced investment and lower labor productivity.

[4] Despite the greater availability of resources for enforcement in developed countries, non-compliance with minimum wage laws is widespread. Ashenfelter and Smith (1979) note that compliance with the US Federal minimum wage was only 65% in 1973; Cortes (2004) reports that in 1997, as many as 40% of US workers who qualified were paid less than the minimum wage; and non-compliance rates in excess of 50% have been reported for Mexico, Morocco (Squire and Suthiwart-Narueput, 1997) and other developing countries. See Neumark and Wascher (2007) for a comprehensive review of the literature on minimum wages from developing countries. All of the theoretical literature on compliance with a minimum wage hinges on employers choosing an optimal level of compliance in the face of penalties and enforcement. See Grenier (1982), Chang and Ehrlich (1985), Bloom and Grenier (1986), Chang (1992), Lott and Roberts (1995) and Weil (2005).

[5] One benefit of focussing on short-run effects is that there is little time for workers to sort across areas and relocate e.g. from rural to urban areas in search of higher wage jobs. In our data, roughly the same fraction of domestic workers report starting their current job in the past year, both before and after the law (12% in September 2001 and 14% in September 2003).

[6] The first minimum wage law introduced in the US in 1912 (in Massachusetts, for women) had some similar features to our setting. One penalty involved newspapers "naming and shaming" non-compliant firms by publishing their names (Thies, 1991).

[7] The ILO defines "informal employment" as "all remunerative work (i.e. both self-employment and wage employment), that is not registered, regulated or protected by existing legal or regulatory frameworks, as well as non-remunerative work undertaken in an income-producing enterprise. Informal workers do not have secure employment contracts, worker's benefits, social protection or workers' representation" (http://www.ilo.org/public/libdoc/ILO-Thesaurus/english/tr1746.htm).

[8] This idea of heightened vulnerability is not new. In 1906, Rubinow (1906) writes about the "servant girl's problem" and describes American preferences for hiring foreign women for domestic work as being related to the "greater ease of managing them", which translates into "longer hours, perhaps lowers wages, more work and, in general, conditions of service more favorable to the employer".

[9] The state of New York recently became the first state in the USA to sign into law a Domestic Workers Bill of Rights. Among other rights, the new law ensures that domestic workers have notice of termination, receive paid sick days and holidays, and other basic labor protections that are standard in the Fair Labor Standards Act. See the editorial "The Rights of Domestic Workers", *The New York Times* June 15, 2009. http://www.nytimes.com/2009/06/15/opinion/15mon3.html Also the article "Senate Passes Historic Bill To Protect Domestic Workers" at http://www.nysenate.gov/press-release/senate-passes-historic-bill-protect-domestic-workers posted on June 2, 2010.

[10] In that paper, Lee (1999) uses regional differences in the relative level of the US federal minimum wage to identify the effects of the minimum wage law on wage inequality in the 1980s separately from the effects of national trends in wages.

[11] Although there is some spatial variation in the level of the minimum, we explain in the next section why we do not use this variation.

on the same trend in the absence of the policy change. This is particularly important for economic variables like wages and hours, that are likely to be able to move quickly in response to changes in general economic conditions. An important defense of our identification strategy therefore relies on providing some evidence that areas with larger pre-law wage gaps were not simply experiencing faster trend growth in wages and were unlikely to be exposed to contemporaneous positive economic shocks. We address these concerns in two ways. First, we show that areas with the smallest pre-law wage gaps appeared to experience the fastest GDP growth over the period, suggesting that if anything, labor demand trends are stronger in these less intensely "treated" provinces. Second, we perform a placebo experiment using a set of similar workers who are unlikely to compete over jobs with domestic workers, but whose job conditions likely reflect general economic conditions: low-skilled male manufacturing workers in urban areas. We find that male manufacturing wages do not grow faster in periods after the law in the places where the domestic worker minimum wage was more binding. This gives us more confidence that the difference-in-differences results we estimate for domestic workers are not being driven by differential wage trends between high and low wage gap areas. This sample of male manufacturing workers is also helpful for a second reason, as they also allow us to rule out the possibility of strong mean reversion in wages as a reason for our positive domestic worker wage results.

Quite apart from the wage and employment effects of the law, we document that the introduction of the law had a substantive impact on more general conditions of work for domestic workers. We examine the probability that a domestic worker is protected by a formal job contract with their employer, or has an employer providing unemployment insurance or pension benefits after the law. The probability of an employee having a formal contract more than doubles in the sixteen months after the law, regardless of the intensity of the minimum wage floor in their area of work; the fraction of workers enjoying pension benefits increases by about 7 percentage points, and the fraction of workers having UIF contributions made for them increases by 18 to 20 percentage points. These improvements in the conditions of work for domestic workers are substantial, immediate, and importantly, occur across the distribution of wages (i.e. not just in areas where workers were originally earning below the minimum). These results suggest the beginning of the formalization of this industry, with potential far-reaching consequences for the nature of domestic work in South Africa.

Given the weak institutional environment for enforcement of the law, which we describe, it is somewhat surprising that we see such large wage responses and no employment effects in this informal sector. Isolating the exact reasons for the employer response is not easy; however, we propose two pieces of evidence that suggest that employers were voluntarily and only partially responding to the law. First, we develop a test for partial compliance, which shows that some workers get increases bringing their wages closer to, but not nearly up to, compliant levels. This partial compliance likely contributes to the lack of employment effects of the law.[12] Second, we show that the wage response of employers is not significantly different across places with different audit probabilities, where we use the presence of a local Labor Center (LC) as a proxy measure of this probability. Partial compliance that does not appear responsive to the likelihood of audit is consistent with the idea that employers may not have been primarily motivated by the threat of external sanctions.

Two related, unpublished papers have examined the effect of the minimum wage law for domestic workers in South Africa: Hertz (2005) and Yamada (2008). Our study differs from these studies in several ways. First, we focus on urban workers only, because we are concerned that identification of the impact of the new minimum wage law for rural domestic workers is likely confounded by the concomitant introduction of a minimum wage for agricultural workers, a plausible alternative sector of work for low-skilled workers in rural areas.[13] Second, we use an updated and consistent set of survey weights for the LFS data (Branson, 2009) which were not available for these studies.[14] Third, we use a higher level of aggregation (the province) to define areas in which the new law was more or less binding, based on pre-law characteristics. This choice presents other challenges for inference that we deal with using an appropriate two-step estimator, described in detail in our empirical analysis section. [15]Fourth, we present evidence from a placebo test using outcomes for male manufacturing worker that bolster our causal claims for the effects of the law on domestic worker wages; this sample also allows us to rule out the possibility that mean reversion could account for the wage effects. Fifth, our results differ from Hertz (2005) and Yamada (2008). Although we also find positive wage effects of the minimum wage law, we find little evidence for a statistically significant negative employment effect of the law on either the intensive or extensive margins of work. Finally, we also try to understand more about the motivations for response to the law. We show that some employers appear to make partial adjustments of wages towards the minimum wage, but not quite up to the legal wage floor. And, we show that the wage effects of the law do not seem to be driven by employers responding to a potential threat of audit, as proxied by the presence of labor centers in the district. Our interpretation of this evidence is that the law sparked the beginning of a formalization of this market (as also evidenced by the improvement in contract coverage, UIF and pension benefits) without strong external sanctions.

The paper begins with a description of the domestic worker industry in South Africa before the law and describes the characteristics of the new law introduced in 2002. After describing the data and presenting summary statistics of our sample, we turn to documenting the wage, earnings, employment and hours of work effects of the law using a combination of kernel densities for wages and earnings and difference-in-differences regressions. We discuss our test for and present evidence of partial compliance with the law. We show that wage increases are not larger in places with a higher probability of being audited, and that the law increased the probability of a domestic worker having a formal employment contract, UIF and pension benefits regardless of how large the initial wage gap in the province of residence was. We conclude with a discussion and interpretation of the results.

2. The domestic worker industry in South Africa

The domestic worker industry in South Africa employs 18% of all women, and 80% of all domestic workers are female. Poorly educated African and colored women make up the vast majority of these domestic workers.[16] In each year of our study, about 35% of urban African and colored female workers were in the domestic work sector, and about 60% of all domestic workers were employed in urban areas. Unlike many Latin American and Middle Eastern settings, and more like countries in the rest of Africa and parts of India, the majority of domestic workers in South Africa are not foreigners. Table 1 presents means and standard deviations of female domestic worker demographics for the period before the law (September 2001, March 2002 and September 2002) and the period after the law (March 2003, September 2003 and March 2004).

[12] This finding relates to a theoretical point made by Basu et al. (2007). The authors develop a model in which governments accept some non-compliance with minimum wage legislation to achieve distributional goals.

[13] Agricultural workers received protection under a minimum wage law, also for the first time, six months after the domestic worker minimum wage floor was imposed.

[14] Details of this choice are discussed in the data section.

[15] Hertz's (2005) working paper uses some of the same LFS cross-sectional data to estimate the impact of the law on wages and employment using a difference-in-differences approach that relies on a much smaller unit of analysis (the magisterial district).He relates the intensity of the minimum wage law to the fraction of workers in a magisterial district who initially earned below the minimum (following Card and Krueger, 1997). We choose not to use magisterial districts as the unit of analysis, since many districts contain only a few (under 10) individuals in each wave who are employed as domestic workers.

[16] Following much of the economic literature on South Africa, we use apartheid-era racial classifications: African for Black South African, and colored for individuals of mixed race.

Table 1
Sample summary statistics.

	N	Pre-law mean (s.d.)	Post-law mean (s.d.)	Post–Pre difference (s.e.)	P value of the difference
	(1)	(2)	(3)	(4)	(5)
A: Conditional on being in the labor force					
Employed at all	52,739	0.63	0.64	0.01	0.26
		(0.48)	(0.48)	(0.01)	
Employed as a domestic worker	52,739	0.35	0.34	−0.01	0.13
		(0.48)	(0.47)	(0.01)	
B: Conditional on being employed as a domestic worker					
Nominal monthly earnings (ZAR)	6160	546.26	658.39	112.14	0.00
		(386.73)	(451.04)	(26.08)	
Nominal hourly wage (ZAR)	6154	3.67	4.37	0.70	0.00
		(2.91)	(3.59)	(0.17)	
Hours of work per week	6876	39.44	38.72	−0.72	0.41
		(14.75)	(14.10)	(0.82)	
Fraction paid > = minimum	6155	0.29	0.42	0.13	0.00
		(0.45)	(0.49)	(0.02)	
Full-time worker	6876	0.79	0.79	0.01	0.88
		(0.41)	(0.40)	(0.02)	
Fraction with a job contract	6784	0.10	0.27	0.18	0.00
		(0.30)	(0.44)	(0.02)	
Fraction with a pension	6867	0.03	0.10	0.07	0.00
		(0.18)	(0.30)	(0.01)	
Fraction with UI coverage	6867	0.02	0.21	0.19	0.00
		(0.15)	(0.41)	(0.02)	
C: Characteristics of women employed as domestic workers					
Age	6876	40.33	40.47	0.14	0.59
		(9.37)	(9.29)	(0.24)	
Education (years)	6876	6.62	6.96	0.35	0.00
		(3.44)	(3.41)	(0.08)	
African	6876	0.90	0.89	−0.02	0.42
		(0.29)	(0.31)	(0.02)	

Data are from South African Labor Force Surveys (LFS 2001–2004). Sample includes African and colored females aged 20–59 inclusive, who have no more than a completed high school education and who live in urban areas. Panel A includes workers and unemployed women looking for work; Panels B and C restrict to women employed as domestic workers in any period. All statistics are weighted and the standard errors of differences and p-values are calculated taking these weights and province-level clustering into account. The pre-law period includes LFS waves in September 2001, March 2002 and September 2002; the post-law period includes LFS waves in March 2003, September 2003 and March 2004. A full-time worker is someone who reports at least 27 hours of work per week; UIF is unemployment insurance.

All statistics are weighted, and the data sources are described in more detail in the next section.[17]

The average age of these workers is around 40, the majority are African women and they have between 6 and 7 years of education, which is roughly completed primary school. This is 0.8 to 2 years below the average education of women working in the most closely related skill group: women in elementary occupations (e.g. newspaper vendors, office cleaners, hawkers, building caretakers, garbage collectors etc) and the female self-employed. 79% of domestic workers report working full-time, defined as 28 hours or more per week, making the majority subject to the full-time minimum wage.

Domestic workers are typically poorly remunerated. Mean wages are lower in this occupational category than in any other: the ratio of the mean domestic worker wage to the mean wage for other low-skilled African and colored elementary workers (self-employed women) was 0.49 (0.64) in September 2001. Prior to November 2002, there was no minimum wage in the domestic worker sector and no formal mechanism existed for domestic workers to negotiate wages. Wages were typically set unilaterally by the employer household or in consultation with other local employers (see Cock (1989) for a qualitative description of this process). Although some aspects of the 1997 South African Basic Conditions of Employment Act governed overtime provisions, leave considerations, minimum notice periods, fair dismissal procedures and severance pay for all workers (South African Department of Labor, 1997), these were rarely adhered to among domestic worker employers (Louw and Van der Berg, 2004). For example, only 10% of domestic workers had a formal contract of employment in 2001, compared with 55% of elementary occupation workers.

In setting the first national minimum wage for domestic workers, the Department of Labor (DoL) took into account the recommendations of a government-appointed Employment Conditions Commission. This group of government representatives and academics defined the scope of the Domestic Worker Sector and concluded that any wage floor should "improve the livelihoods of those worst off" and "retain jobs". Their recommendation for the actual minimum wage level was higher than that initially proposed by the government, and was the one eventually adopted (Budlender et al, 2002).

Under the new law, which became effective on 1 November 2002, domestic workers and gardeners working in private homes had the right to a minimum wage and to 8% annual wage increases.[18] The urban full-time hourly minimum wage was set at ZAR4.10 (USD 0.410) in November 2002; the part-time wage was ZAR4.51 (USD 0.451) where part-time work is defined as fewer than 28 hours per week.[19] Since about 80% of domestic workers in urban areas work full-time, we focus on the urban full-time minimum as the relevant wage floor throughout the paper.[20] In

[17] Throughout, we use the following definition of domestic workers: currently employed African or colored females aged 20 to 59 inclusive, who live in urban areas and have their occupation coded as "working as a domestic worker in a private household" for the week prior to the survey. We exclude a handful of these workers who also report having their own business on the side and those who have more than a high school level of education (12 years).

[18] Garden workers, most of whom are men, are also covered as domestic workers under this law. However, they make up a minority of domestic workers and we omit them from our analysis.

[19] The average Rand/USD exchange rate from June 2002 to January 2003 of ZAR10 = USD1.

[20] The law specifies different wages for two types of urban areas, 'Area A' and 'Area B' localities. These areas generally differ in size, and since Area A localities are the largest urban parts of the country, we use the full-time wage set for these areas as the urban minimum wage. We cannot distinguish between A and B areas in our data to create a finer measure of treatment.

addition, the new law enabled employers to deduct up to 10% of the total salary for rental value of any accommodation provided. A separate and related piece of legislation, introduced shortly afterwards on April 1, 2003, additionally required employers to register domestic workers with the DoL in order to pay unemployment insurance (UI).

It is important to note that the initial change introduced in this industry in November 2002 was an order of magnitude larger than typical changes in the value of the minimum that economists typically study: the wage floor was set at 1.5 times the median monthly earnings of domestic workers in 2002. Full compliance in this context would have entailed massive wage increases for a majority of workers and potentially large negative employment effects for most employees either on the extensive or intensive margin. Given existing high levels of unemployment in South Africa, this would be one possible reason for why the government did not commit substantial resources towards enforcement of the law.

In fact, in the first ten months after the law, both the audit probability and penalty for first time violators were very small.[21] As far as we have been able to establish, no inspections were carried out until August 2003, ten months after the law. At this time, 1600 households in five provinces were earmarked for inspection and only 25% of them were found to be in compliance with the law.[22] Although we have not been able to obtain official aggregate statistics on household inspections during these years, interviews with several labor centers from around the country indicate at most a couple of hundred household inspections per year.[23]

There were also no documented rules about penalties or back-pay for non-compliers at the time of implementation. Press releases from the DoL in February 2003 indicated that “Non-compliance with the UI law will result in penalties of up to ZAR5000 (USD500) per household or five years imprisonment”.[24] However, we have not been able to find official documentation of this or other penalties, nor has our search of newspaper archives revealed any reports of fines or prison sentences being imposed on non-compliant employers.[25] What we do know is that non-compliers might have expected three progressively more threatening warnings (telephonic, written, court order) before appearing at a court of law. At this time, the right of appeal would have been well exercised, as it is unclear how evidence for non-compliance could be substantiated in this predominantly cash payment industry.

All evidence from the DoL website and various legal documents and reports related to the law suggest that the general monitoring and enforcement regime in the domestic worker industry was weak and presented employers with an almost zero expected cost of non-compliance. Despite this lack of compliance incentive, the timing of the law coincides with substantial rightward shifts in the wage and earnings distributions of domestic workers. We next describe the data which we use to document these shifts.

3. Data description and empirical methods

3.1. LFS surveys

We use six cross sections of data from the nationally representative South African Labor Force Surveys (LFS): September 2001, March 2002, September 2002, March 2003, September 2003 and March 2004. These LFS surveys are biannual rotating panel surveys, conducted in February/March and September each year and include detailed data on the work and unemployment experiences of 60,000 to 70,000 working-age individuals living in 30,000 households. The six waves we use span the period just before and just after the minimum wage law becomes effective in November 2002. The survey instrument is similar to the US Current Population Survey, although the rotation pattern differs. In each wave, 20% of households interviewed in the previous wave are rotated out of the survey entirely.[26] These LFS data are high frequency and can be used to examine differences over a six month window. They help us to estimate the immediate impacts of the law while controlling for observable characteristics of domestic workers (i.e. age, race and years of education) and to see whether pre- versus post comparisons are sensitive to these controls. We also model the probability of being employed as a domestic worker in each wave, using the sample of employed and unemployed women with similar age and education profiles to domestic workers. We use these LFS data as repeated cross sections and exploit the cross-sectional variation in intensity of the law at the province level (9 provinces in total) in combination with the time-series variation in the application of the law to identify the effects of the law. [27]

3.2. Sample weights

One further aspect of the data that is worth noting concerns the appropriateness of the sample weights available in the LFS. Survey weights for LFS 2001 and 2002 are benchmarked to the 1996 Census; while survey weights for LFS 2003 are benchmarked to the later 2001 Census. With different benchmark years, this series of weights is potentially inconsistent over time. Hertz (2005) provides a detailed discussion of how these inconsistencies in the weights may affect any analysis of the minimum wage law. He points out that the 2001 Census under-counted the fraction of women of working age and that extrapolations between 1996 and 2001 overestimated growth in the adult population by underestimating the effect of the HIV epidemic on adult totals. He notes that “no consistent official series of sampling weights is available. This poses a serious problem, as both the changes in scale and the changes in the age, gender, province, and race group distributions result in artifactual changes in the measured employment of domestic workers

[21] During this time, employers might have expected a vanishingly small audit probability for two reasons. First, the chances of random inspection are small since inspections are labor intensive and each household yields only a single worker inspection. Bhorat et al (2010) provide evidence of only one labor inspector per one million workers for the entire country. If each domestic worker works in only one household, this yields over one million employers that are subject to the minimum wage law in this industry alone. Second, logistical difficulties in gaining access to employer premises make any inspection costly. A non-compliant employer can legally refuse to allow an inspector into their private residence, or simply not be present at the time of the inspection. A court order from a Labor Court is then required to enter the residence. There are also physical barriers to entry: inspectors have reported difficulties with impenetrable gates and “the presence of dogs” (Official release by the DoL, 27 th August 2003. Available at *www.labour.gov.za*).

[22] See media release at *www.labour.gov.za/media/statement.jsp?statementdisplay$_i$d=* 9685

[23] We conducted a snap survey of the 104 Labor Centers in the country and found that in several of them, between 100 and 250 household inspections were carried out each year after 2004.

[24] http://www.info.gov.za/speeches/2003/03050809461001.htm

[25] The Basic Conditions of Employment Act (1997) states that underpayment violations for any worker are penalized in the following manner: first offense—25% of the gap plus interest; second offense within 3 years—50% of the gap plus interest; third offense within 3 years or second offense within 2 years—75% of the gap plus interest; fourth offense within 3 years—100% of the gap plus interest; fifth offense within 3 years—200% of the gap plus interest. However, as noted in Louw and van der Berg (2004), the BCEA conditions were seldom adhered to in informal sectors of work.

[26] Although there are three earlier waves of data going back to 2000, the baseline sample was drawn anew for the September 2001 round which is why we begin our analysis with data from this round. And, although the LFS survey has continued biannually since March 2004, we cannot use additional waves of post-data in our analysis since the survey stopped reporting whether the individual resides in an urban or a rural area, thus making it impossible for us to condition our sample on urban domestic workers (Statistics South Africa, 2000–2003).

[27] There is a panel data component of the LFS survey, but we have some concerns about the representativeness and quality of the panel data set of workers. First, since the design of the panel includes a 20% out-rotation of dwellings in each six month period, in order to appear in the panel a worker needs to be living continuously in the same place for six waves and needs to escape the out-rotation group. We expect that workers who are more likely to retain employment are also more likely to appear in the panel, making the estimation of the effects of the law on such a selected sample difficult. Second, we learned from Statistics South Africa (the organization that collects the LFS data) that some panel matches could not be made, because some fraction of questionnaires from the pre-period were lost in a flood. We have no way to know what impact this would have on the representativeness of the panel.

that are too large to be ignored." Indeed, the original sample weights that he refers to have been shown to produce inconsistent aggregate statistics over time. More formally, Branson (2009) explains that "The StatsSA weights presented in the data are problematic for analyses over time … the auxiliary data used as a benchmark in the post-stratification adjustment are unreliable and inconsistent over time and hence result in temporal inconsistencies even at the aggregate level."

For these reasons, we do not use the weights provided with the LFS data. Rather, our results use new (individual-level) survey weights constructed in Branson (2009) using entropy estimation.[28] As shown in that paper, the weights produce "consistent demographic and geographic trends".

3.3. Sample selection and key variables

Our main analysis sample includes all urban African and colored woman aged 20 to 59, who report domestic work in the week before the survey, who do not also own their own business and who have no more than high school education. For the employment analysis, we use an expanded sample of all African or colored woman aged 20 to 59 who live in urban areas, who have no more than a high school level of education and who are employed or looking for work. For the placebo test, we make use of a sample of male urban African and colored workers, aged 20 to 59, who have no more than a high school education and who report working in the manufacturing sector.

In the LFS, all workers are asked about earnings, pay frequency and usual weekly hours of work. Most workers report earnings and a corresponding pay frequency. The vast majority– 89% of domestic workers— report a monthly pay frequency. We convert all earnings to monthly amounts using the pay frequency information. About 8–9% of domestic workers in any one wave do not report earnings or only report earnings in brackets; we exclude these individuals from our analysis.[29] To capture hours of work, we use the response to the question "How many hours do you usually work in a week?" We construct hourly wage measures by dividing monthly earnings by (Usual hours worked per week)*(Average weeks in a month). Just over 5% of workers report working more than 70 hours a week and we exclude these outliers from our analysis.

3.4. Empirical strategy

We describe the impact of the minimum wage law in two ways. First, we estimate non-parametric kernel densities of domestic worker wages and earnings and test for differences in the domestic worker distributions over time using Kolmogorov–Smirnov tests of the equality of distributions. Then, we statistically test for whether outcomes changed in the period after the law compared to before the law, and for whether these changes are larger in areas where the minimum wage initially had more "bite". We specify the following difference-in-differences regression model:

$$y_{ijt} = \alpha_0 + \alpha_1 POST_t + \alpha_2 WG_j + \alpha_3 POST_t * WG_j + X_{ijt}\gamma + \nu_{ijt} \tag{1}$$

where y_{ijt} is one of several main outcome variables for individual i living in province j in period t: log hourly wages, weekly hours of work, the possession of a formal job contract for the set of domestic workers, whether the individual gets pension or UI benefits, and whether the individual is employed as a domestic worker or not among the set of demographically similar women.

[28] These methods are described in Branson (2009). Entropy estimation essentially creates a new set of weights that are as close to the original weights as possible (to preserve the main features of the sample design) but that are adjusted to account for errors arising from time inconsistent benchmarks, from inconsistencies between household and person weights, and for errors in the trimming of weights in earlier survey years. We are grateful to Branson for making these weights available to us.

[29] The fraction of domestic workers with no earnings information is not significantly different across waves.

Eq. (1) is estimated with and without controls for worker characteristics (X_{ijt}), including age, years of education and an indicator variable for whether the person is African. We group data from the September 2001, March 2002 and September 2002 surveys into a "Pre" period and the remaining three waves into the "*POST*" period. Because the law was formally announced on August 30, 2002, but employers were only expected to be compliant from 1 November 2002, there is some ambiguity about whether the September 2002 wave belongs in the pre- or post-period. There was substantial publicity in the months prior to the law becoming effective, making it plausible that some employers started to react even before the November 1st deadline. For this reason, we also present the main results from regressions that omit the September 2002 cusp wave, for which "$POST_t$" is not well-defined. These are our preferred estimates.

To construct a measure of the intensity of the minimum wage law, we follow other examples in the minimum wage literature (notably Lee, 1999) that use aggregate (rather than individual) data on workers over time and construct a locally-specific wage gap using data on wages from the "PRE" period. We define the province-level wage gap as:

$$WG_j = \log[\min(w)] - \log\left[\text{median}\left(w_j\right)\right] \tag{2}$$

where *min(w)* is the urban full-time minimum wage in November 2002 and $median(w_j)$ is the median wage of all urban domestic workers in the province before the law. That is, the intensity of the minimum wage law going forward in time is measured according to domestic worker conditions in local labor markets in September 2001 and March 2002. Provinces with very low median wages prior to the law therefore have a large positive value for WG_j. To aid interpretation of the coefficients, we implement this difference-in-difference regression with demeaned versions of the WG_j and $WG_j{}^*POST_t$ variables and all continuous control variables.

Under the assumption that the wage gap measure (and therefore the difference-in-differences term) is orthogonal to the error in (1), the parameter α_1 tells us how outcomes changed on average across all areas after the law while α_2 tells us the average difference in outcomes for domestic workers in areas with larger than average wage gaps, across the entire period. Of course, any general economic trends that affect outcomes in the post-period would also be part of the α_1 parameter. α_3 is the difference-in-differences parameter: it tells us how much more outcomes changed after the law, in areas where the minimum wage was more binding.

There are two main concerns with using the median wage gap measure as our measure of the treatment intensity of the law. First, there is the obvious worry that high and low wage gap areas may have been trending differently in the POST period, in a way that confounds the effects of the law. Second, there is the potential for mean reversion in wages to account for the wage results.

To deal with the first concern, we do two things. First, we show that provinces with larger wage gaps (i.e. more intensely treated areas) do not appear to be growing faster over time, relative to small wage gap provinces, making differential trends in labor demand an unlikely explanation for our wage results. Second, we show for a specific subset of workers that there is no evidence of positive contemporaneous shocks to large wage gap provinces that could account for our results. We implement a placebo test by estimating the same regression model in (1) for a set of workers for whom the minimum wage law is irrelevant and who do not directly compete with domestic workers for jobs: male, low-skilled manufacturing workers in urban areas. The minimum wage law for domestic workers does not apply to them and, moreover, is set at a level far below their median wage. Therefore, if we see manufacturing worker wages increasing more in areas where domestic worker wages were substantially below the minimum, we would be concerned that all wages in high wage gap provinces were increasing over time. This is not the case.

To address the second concern about mean reversion in wages, we look for evidence of this in the manufacturing worker sample. We alter the definition of WG_j to be:

$$WG2_j = log[min(w)] - log\left[median\left(w_j\right) manufacturing\right] \quad (3)$$

where we use the domestic worker minimum wage as an arbitrary benchmark, and the median manufacturing worker wages in the period before the law to construct the differenced variable. We implement the difference-in-differences regression using this alternate measure of “treatment”. If there is strong mean reversion in wages, we should see manufacturing worker wages increase in places with higher values of $WG2_j$, after the law.

A third concern relates to the main unit of analysis that generates our identifying variation. Note that the measure of the intensity and application of the law, WG_j, is captured at the provincial level. This means that in estimating (1), we exploit variation at the group (province) level to identify the effects of the law. The fact that there are only nine groups (provinces) presents a challenge for inference. Consider the following error components model for the error term ν_{ijt} in (1):

$$\nu_{ijt} = v_j + \epsilon_{ijt} \quad (4)$$

Standard OLS without adjustment of the standard errors treats each individual as if they contribute equally and independently to the variation in outcomes. Since $\sigma_\nu = \sigma_v + \sigma_\epsilon$ and v_j is the same for each individual in the same province, this approach overstates the amount of variation in our data. The situation is not quite as dire as only having nine observations, since we also have individual-level control variables that differ within provinces and which account for a large part of the variation in outcomes. However, it is clear that some adjustment for the grouped nature of the error term is called for.

We take two approaches. First, we follow the common recommendation in the literature to estimate Eicker-White clustered standard errors at the level of the province.[30] However, the standard asymptotic arguments for the consistency of clustered standard errors may not apply with the small number of groups in our context, even given the additional variation found in the demographic controls. We still run the risk of underestimating standard errors (and over-rejecting the null) using this approach.[31]

As a second approach, we follow Donald and Lang's (2007) suggestion and implement their two-step estimator which, under some conditions, produces standard errors that appropriately take into account the group-specific term in Eq. (4). To implement this, we first regress outcomes on all individual level variables, the $POST_t$ variable, a full set of province dummy variables and a full set of $POST_t$*province interaction dummy variables. Then, we take the estimated coefficients and use them as the outcomes in a second stage regression. This second stage involves regressing the estimated coefficients on a $POST_t$ indicator, the WG_j measure and the interaction $POST_t * WG_j$. The resulting standard errors from this second stage model are calculated with the group-component of σ_ν taken into account and, together with the second stage coefficients, form t statistics that have the t distribution when the number of groups is small.[32] There is a nice intuition for this estimator: the first stage regression produces estimates of the group-level means in the post-period (these are the coefficients on the province indicator variables and the $POST_t$*province interactions) after taking into account variation in the other individual controls. In the second stage, we estimate how much of this variation in these group-level (estimated) means is predicted by variation in $POST_t$, WG_j and $POST_t * WG_j$.[33]

In addition to the graphical evidence from the kernel densities and the difference-in-differences statistical tests, we implement two tests to investigate the mechanisms through which the new minimum wage law had an effect. We are particularly interested in understanding whether or not employers are complying with the law primarily because they are concerned with penalties and external enforcement.

Our first test shows that there is only partial compliance with the law. That is, some employers are responding to the law, but their response is insufficient for compliance. In the absence of individual-level panel data which we could use to test whether a worker experiences only partial wage adjustment to the law, we devise a related test for partial compliance using repeated cross-sections. First, we classify domestic workers into each of three bins, where the ‘near compliant’ bin consists of workers earning the minimum or up to 10% below the minimum, the ‘compliant’ bin contains all workers who earn above the minimum and the non-compliant bin contains workers that are earning more than 10% below the minimum. We estimate an ordered probit model of this new variable on a constant, a $POST_t$ indicator and a set of demographic and geographic controls (in a first specification) and then also include controls for the wage gap and the $POST_t * WG_j$ interaction (in a second specification). We then predict the relevant marginal effects to estimate the change in the probability of being in the ‘near compliant’ bin and test for whether this change is positive. Appendix 1 sets out the details of how we derive this test as the relevant one for partial compliance. The intuition behind the test is that if the probability of workers reporting wages in the non-compliant bin falls by more than the probability of workers reporting wages in the compliant bin rises, then some workers are earning more than they were before (shifting to the near-compliant region of the wage distribution), but not enough more to make them compliant with the law. Such a result would be consistent with an argument whereby employers are responding, but not because they are law-abiding. Note that this interpretation of the test relies on there being no large employment adjustment to the law, a fact that our earlier estimates demonstrate.

Our second test shows that the size of the wage effect is not related to a proxy for the probability of being audited by the DoL. Although we have described above how the probability of being monitored was very small, it is still possible that domestic workers themselves may have threatened to report non-compliant employers to the authorities. Indeed, Hertz (2005) reports an immediate increase in the number of domestic worker complaints made to the Commission for Conciliation, Mediation and Arbitration (CCMA) after the law is introduced. The relevant labor authorities we consider here are housed in local LCs. Under the assumption that it is easier to visit an LC in order to lodge a complaint if there is an LC in your local labor market, we can use the presence of an LC in the worker's local labor market as a proxy for a higher probability of audit. We control for this proxy and its interactions with $POST_t$, WG_j and $POST_t * WG_j$. We treat the magisterial district in which the domestic worker resides as the local labor market—a more disaggregated measure than the province. Although we have not been able to obtain quantitative evidence on the probability of being audited in relation to distance from an LC,

[30] This approach to estimating appropriate standard errors in a difference-in-differences specification is discussed in Bertrand et al. (2004) and in Donald and Lang (2007).

[31] In Appendix 3 Tables 1 and 2 we also present block bootstrapped standard errors, where the province is the block. Results do not differ substantially from the OLS results, in terms of which variables are statistically significant.

[32] The conditions required for this result are that the number of individuals in each province is large and that the underlying v_j's are normally distributed. We must assume the latter condition, and having at least 600 observations in each “group” gives us some confidence in the former condition. For more details on this procedure, see Donald and Lang (2007).

[33] In the absence of demographic or other controls that vary within province, this two-step estimator is equivalent to collapsing the data to the province-year level and estimating a form of the difference-in-differences specification on these province-level means. However, we use the two-step estimator here to take advantage of the fact that we have individual level controls that do matter for outcomes, while not overstating the amount of policy variation we have to identify the impact of the law.

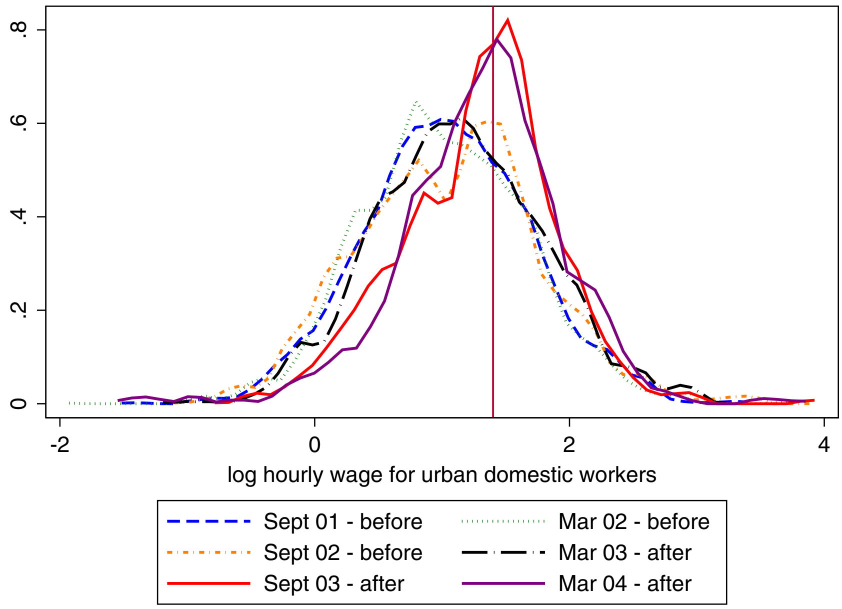

Fig. 1. Distribution of log hourly wages for urban domestic workers. Kernel density plots of log hourly wages (bandwidth 0.02). Data are from South African Labor Force Surveys (March 2001–March 2004). The vertical line is at the level of the full-time minimum wage (monthly income) for urban domestic workers. Each wave of data contains between 996 and 1260 observations. Kolmogorov–Smirnov tests of equality of distributions reject at the 5% level for each pairwise comparison of waves in the before and after periods.

several LC's reported to us that the majority of their complaints from this sector are received in person; moreover, LCs are legally and institutionally responsible for investigating complaints about working conditions and responsible for the enforcement of sectoral determinations across all industries (Bhorat et al, 2010).[34] If employers respond to the law because they fear external enforcement, we should see wages rising by more in places with an LC, compared to places without one.

4. Results

4.1. Before-after and difference-in-differences comparisons

The introduction of a minimum wage in the domestic worker industry appears to have had immediate and substantial effects on earnings and wages of the average domestic worker, yet limited effects on hours of work. Table 1 shows our initial evidence from pre-post comparisons of means. Panel B presents the means and standard deviations of several outcome variables for domestic workers before the law (column 2) and after the law (column 3) as well as the difference (standard error of the difference) in these means and the *p*-value of the difference in the final columns.

The first point to note is the large jump in mean earnings and wages from before to after the law: both monthly earnings and hourly wages increase by about 20% and this difference is statistically significant. After the law, the average wage of domestic workers is higher than the minimum, at ZAR4.37 per hour whereas before, the average worker was being paid significantly below the minimum at ZAR3.67. The fraction of workers paid above the minimum increases from under one-third prior to September 2002 to over 40% after the law.

Another striking result from the table is that the variance of wages and earnings in the post period illustrates a significant increase in dispersion relative to the pre-period. This is unusual, since wage compression is typically observed in response to increases in binding minimum wage laws. This suggests that either some employers at the lowest end are not responding to the law at all, or that employers at the high end are also increasing wages even though they are already compliant with the law, or both.

Figs. 1 and 2 underscore the findings in Table 1 and show the movement of the entire wage and earnings distribution over time. Each figure is a kernel density-smoothed plot of the earnings and wage densities in September 2001 (14 months before the law), March 2002 (8 months before the law) September 2002 (2 months before the law), March 2003 (5 months after the law), September 2003 (10 months after the law) and March (2004) (16 months after the law). The vertical line represents the urban full-time minimum. Pre-law, there is no evidence in the graph that earnings are shifting, despite annual inflation rates of 6 to 7%.[35] In fact, in these pre-periods, mean domestic worker wages are declining in nominal terms. However, each figure shows that the entire (log) earnings and wage distributions shift to the right in March 2003 with a pronounced mass in the lower tail moving towards the minimum wage line. The shift is even more pronounced by September 2003 and March 2004. We test for significant differences between these distributions using Kolmogorov–Smirnov tests in all pairwise comparisons of each wave and find that each of the post-law distributions is significantly different from each of the pre-law distributions. In addition to the prominent shift in wages and earnings after the law, it is clear from these figures that a large fraction of domestic workers continue to earn less than the minimum, and that although there is a modal wage, there is no sharp minimum wage “cliff” that is characteristic of US data for low wage workers (Dinardo, Fortin and Lemieux, 1996).[36]

Turning back to Table 1, there is little evidence of a large disemployment effect after the introduction of the law: hours of work fall by 0.72 hours, but this difference is not statistically different from zero. Also, there appears to be no change in the fraction of domestic workers who are employed in full-time positions, nor in the fraction of workers who are employed as domestic workers in Panel A. Combining this lack of change in employment with the large increases in mean wages and earnings and the rapid shifts in these wage and

[34] Several labor centers reported to us that all citizens are able to report complaints directly to them, either by phone or in person, and that both methods are used. These are also the units responsible for doing communications “outreach” with workers and employers and for conducting mass inspections across several workplaces, in all industries.

[35] CPIX index provided by Statistics South Africa.

[36] In Appendix **2**, we use propensity-score reweighting to rule out the possibility that these shifts in the wage distribution are driven by compositional changes in the type of domestic workers employed after the law.

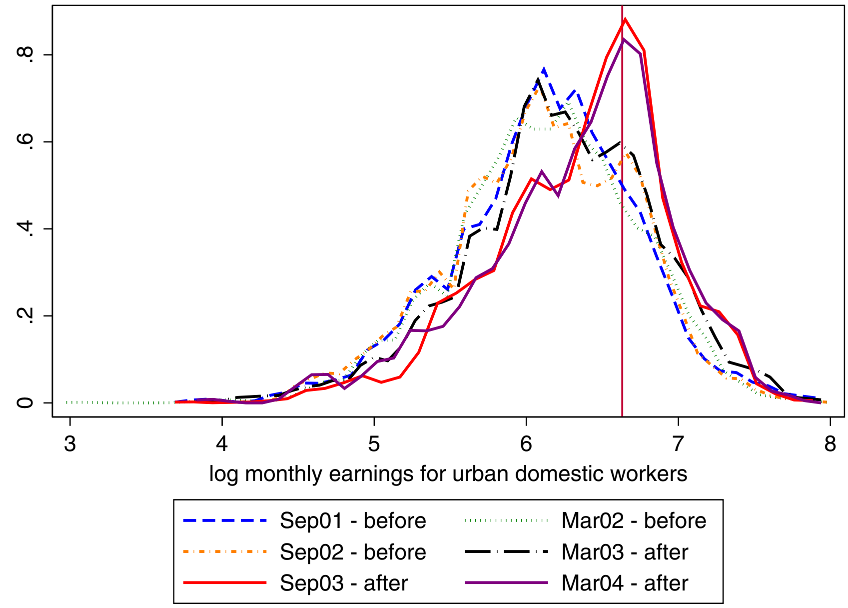

Fig. 2. Distribution of log monthly earnings for urban domestic workers. Kernel density plots of log monthly earnings (bandwidth 0.08). Data are from South African Labor Force Surveys (March 2001–March 2004). The vertical line is at the level of the full-time minimum wage (monthly earnings) for urban domestic workers. Each wave of data contains between 996 and 1260 observations. Kolmogorov–Smirnov tests of equality of distributions reject at the 5% level for each pairwise comparison of waves in the before and after periods.

earnings distributions that line up well with the timing of the law, we have initial, strongly suggestive evidence that the law had a dramatic impact on the domestic worker sector. Changes in other aspects of the work relationship also seem important: the fraction of workers with a job contract increases by 18% in the post period, the fraction with UI coverage increases by 19 percentage points and the fraction with any pension contributions increases by 7 percentage points.

All of these comparisons so far rely solely on variation in the application of the law. Particularly for earnings, wage and hours variables that are likely to fluctuate with general economic conditions, it is possible that other factors could influence some of these outcomes post-law, thereby confounding the effects of the law. For this reason, we go beyond the simple pre-post comparison and investigate whether there are larger wages and hours changes in places where the new wage floor is more binding.

Fig. 3 provides the basic information we use in our difference-in-differences regression. This figure shows the mean (log) hourly wage for domestic workers in each wave, for each of nine provinces.

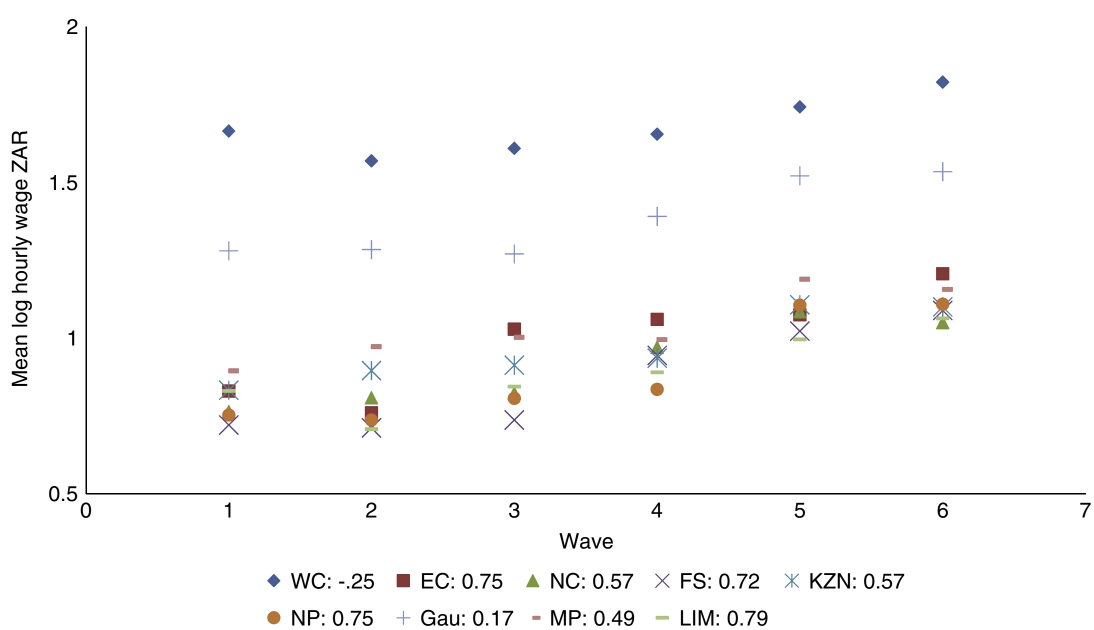

Fig. 3. Mean hourly domestic worker wages by province over time. Each point represents a province-wave level average hourly wage for domestic workers, for waves before and after the law. The black vertical dashed lines demarcate the period before (to the left) and the period after (to the right) the law, with the "cusp" period falling between these lines. The horizontal dashed line represents the urban full-time minimum wage (ZAR4.1) established in November 2002. All data are from the South African Labor Force Surveys (March 2001–March 2004) and means are weighted. Provinces are: Western Cape (WC), Eastern Cape (EC), Northern Cape (NC), Free State (FS), Kwazulu-Natal (KZN), North West Province (NP), Gauteng (Gau), Mpumalanga (MP) and Limpopo (LIM). Province-level wage gap measure calculated prior to the law is shown in the legend for each province: e.g. EC 0.75 means that the median wage was 75% below the minimum wage in the pre-law period.

Table 2
Log hourly wages of domestic workers: Difference-in-differences.

	All waves				Excluding “cusp” wave			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	0.202***	0.193***	0.201***	0.189***	0.215***	0.203***	0.217***	0.202***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Pre-law wage gap (WG)	−0.858***	−0.820***	−0.829***	−0.792***	−0.902***	−0.862***	−0.868***	−0.827***
	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Pre-law wage gap WG_j*$POST$	0.090**	0.097**	0.100	0.100	0.133***	0.145***	0.138*	0.150**
	(0.04)	(0.04)	(0.07)	(0.06)	(0.04)	(0.04)	(0.06)	(0.05)
Age, education, African controls	N	Y	N	Y	N	Y	N	Y
N	6154	6154	18	18	5205	5205	18	18

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. Each regression contains a constant (coefficient not shown). In columns 1, 2, 5 and 6, standard errors are Eicker-White, clustered at the province level. Critical values for significance for two-step estimates are taken from the t distribution, see text for details. Columns 1–4 include domestic workers in all waves. Columns 5–8 exclude domestic workers in the September 2002 “cusp” wave. POST = 1 in March 2003, September 2003 and March 2004; otherwise zero. Pre-law wage gap is the province-level difference in the log(4.1) − log(median wage), where 4.1 is the urban full-time minimum wage introduced in November 2002 and median wage is the median wage of domestic workers in each province across all of the pre-waves (September 2001 and March 2002).

The dashed vertical lines demarcate the pre-, cusp (September 2002) and post-periods, while the dashed horizontal line denotes the urban full-time minimum hourly wage set in November 2002. In the legend of the figure, we also show the value of each province's pre-law wage gap measure: for example the Western Cape wage gap measure is −0.25 (meaning that median wages in this province are higher than the minimum wage by 25% prior to November 2002) while Limpopo province has a wage gap measure of 0.798 (meaning that median wages are almost 80% below the minimum wage before November 2002). Provinces can be grouped into one of three categories: those provinces paying relatively high wages before the law (Western Cape and Gauteng), provinces paying middle-range wages before the law (Mpumalanga, KZN and the Northern Cape) and provinces paying very low wages before the law (Eastern Cape, Free State, North West Province and Limpopo).

The graph shows that every province except the Western Cape had mean hourly wages far below the minimum wage prior to the law. There is no clear evidence of pre-trends in wages that differ between provinces; in a couple of provinces, mean wages look like they increase somewhat in the “cusp” wave of September 2002 (WC, NP and MP). And, although all provinces evidence an increase in mean wages after September 2002, this increase appears to be steeper for provinces falling further below the minimum prior to the law. The figure shows clearly that mean log hourly wage measures are “bunching together” for provinces further away from the minimum in the post-law period.

Using the cross-province variation in WG_j combined with the timing of the law, we estimate difference-in-differences regressions of the form in Eq. (1). Results for domestic worker wages are presented in Table 2. Columns 1, 2, 5 and 6 present OLS estimates with robust standard errors clustered at the province level. In columns 3, 4, 7 and 8 we present the results from the two-step estimator of Donald and Lang (2007) along with appropriate standard errors and significance levels taken from the relevant t-distribution. The first four columns contain results for the full sample of domestic workers and the last four columns restrict the sample by excluding domestic workers in the “cusp” wave of September 2002—these are our preferred estimates. We present results without demographic controls (age, education and African indicator) in each odd-numbered column and results from regressions that include the demographic controls in each even-numbered column. All regressions are weighted.[37]

Across all columns, there is a large, significant increase in wages in the post period, of between 18.9 and 21.7%. This reflects the information in Table 1 and in Fig. 3: average wages across all domestic workers increase significantly after the law.[38] Recall that the pre-law wage gap (WG_j) is defined such that the further below the minimum wage the provincial median wage lies, the larger (more positive) this variable is. Not surprisingly, in places with larger WG_j, average wages are significantly lower in the pre-period. However, in the *POST* period, provinces that were further behind are the ones where the wage response is the largest, as indicated in Fig. 3. The coefficient on $POST_t{}^*WG_j$ is large and positive in each specification and significantly different from zero in both the OLS and two-step estimator results, for the sample that excludes September 2002.[39] Focusing on the last four columns of this table, our estimate of α_3 suggests that domestic worker wages increased by 13 to 15% after the law. Or, to take a particular example: for a worker living in the province with the largest (demeaned) pre-law wage gap (0.36), the average increase in wages after the law is about 25% using either the OLS (0.203 + 0.36*0.145) or the two-step (0.201 + 0.36*0.15) results.

In contrast to these large wage effects that appear shortly after the law, hours of work do not exhibit similar significant declines in the *POST* period.[40] Table 3 presents results of the form in Table 2 for usual weekly hours of work. Across specifications, the point estimate on $POST_t$ is between −0.76 and 1.149 and never statistically significant. Regardless of the method of estimation, the coefficient on $POST_t{}^*WG_j$ is larger and negative, suggesting that hours may have declined more (between −2.8 and −5.1 hours more) in areas where the initial wage gap was larger. This is between a 7 and 12% fall in employment on the intensive margin. As an example: for a worker living in a province with the largest de-meaned pre-law wage gap, the average reduction in hours of work is about 6% $[(-1.11+0.36*(-3.57))/40]$ using the OLS results. However, in all specifications, we cannot reject that these estimated changes in hours of work are zero; none of the coefficients on the $POST_t{}^*WG_j$ variable is close to being precisely estimated. It is possible that measurement error in reports of hours of work undermines

[37] Results from the unweighted regressions for log hourly wages are presented in Appendix 3 Table 1 for comparison. In these tables, we also present alternate block-bootstrapped standard errors for the OLS estimates, where the province is treated as the block.

[38] Recall that the wage gap measure is demeaned; so the coefficient on $POST_t$ can be interpreted as the average change in wages for domestic workers in areas with the average wage gap measure.

[39] The relevant critical values from the t distribution for a one-tailed test with 4 degrees of freedom are 2.13 (p<0.05 significance) and 1.53 (p<0.1 significance). For a two-tailed test, the relevant critical values are 2.77 (p<0.05) and 2.13 (p<0.1). We use the t distribution because the number of observations in the second step of the estimation is small.

[40] Sample size changes across tables as more workers report hours of work information than report monthly earnings.

Table 3
Usual weekly hours of work of domestic workers: Difference-in-differences.

	All waves				Excluding "cusp" wave			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	−0.887	−0.901	−0.848	−0.761	−1.149	−1.111	−0.874	−0.77
	(0.82)	(0.81)	(1.58)	(1.39)	(1.18)	(1.14)	(1.62)	(1.40)
Pre-law wage gap (WG)	5.151	3.98	5.758*	4.996*	5.383	4.1	6.675*	5.729*
	(3.21)	(2.58)	(2.52)	(2.17)	(3.84)	(3.01)	(2.94)	(2.48)
Pre-law wage gap (WG)*POST	−2.832	−3.309	−4.157	−4.283	−3.064	−3.579	−5.074	−5.128
	(2.17)	(2.28)	(3.79)	(3.50)	(3.20)	(3.23)	(4.08)	(3.70)
Age, education, African controls	N	Y	N	Y	N	Y	N	Y
N	6876	6876	18	18	5824	5824	18	18

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. Each regression contains a constant (coefficient not shown). In columns 1, 2, 5 and 6, standard errors are Eicker-White, clustered at the province level. Critical values for significance for two-step estimates are taken from the t distribution, see text for details. Columns 1–4 include domestic workers in all waves. Columns 5–8 exclude domestic workers in the September 2002 "cusp" wave. POST = 1 in March 2003, September 2003 and March 2004; otherwise zero. Pre-law wage gap is the province-level difference in the log(4.1) − log(median wage), where 4.1 is the urban fulltime minimum wage introduced in November 2002 and median wage is the median wage of domestic workers in each province across all of the pre-waves (September 2001 and March 2002).

Table 4
Probability of working as a domestic worker: Difference-in-differences.

	All waves				Excluding "cusp" wave			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	−0.004	0.003	−0.012	−0.005	−0.010	−0.002	−0.014	−0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-law wage gap (WG)	0.0409***	−0.014	0.0594**	−0.008	0.0386**	−0.0173**	0.0610**	−0.007
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Pre-law wage gap (WG)*POST	−0.004	0.002	−0.024	−0.017	−0.002	0.004	−0.025	−0.018
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age, education, African controls	N	Y	N	Y	N	Y	N	Y
N	52,739	52,739	18	18	44,005	44,005	18	18

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. Each regression contains a constant (coefficient not shown). In columns 1, 2, 5 and 6, standard errors are Eicker-White standard errors, clustered at the province level. Critical values for significance of two-step estimates are taken from the t-distribution, see text for details. Sample in columns 1–4 include employed and unemployed women aged 20–59 (searching unemployed) who have no more than high school; sample in columns 5–8 excludes all of the individuals in this group who appear in the September 2002 "cusp" wave.

our ability to precisely estimate the effect of the law on hours of work. Nevertheless, we find no strong statistical evidence that employers adjusted labor demand on the intensive margin in order to afford the massive increase in wages that are evident in the data.[41]

There is also no evidence that adjustment occurred on the extensive margin. If domestic workers lost jobs as a result of the law, we should see different probabilities of low-skilled African and colored women being employed as domestic workers *POST*-law. Table 4 presents difference-in-differences results for the binary outcome "Does the individual work as a domestic worker?" The sample includes domestic workers and demographically similar women who are working or searching for work. Defining the sample in this way allows for the possibility that domestic workers may switch to other jobs or lose jobs altogether in the *POST* period. None of the estimated coefficients on the $POST_t$ or $POST_t{}^*WG_j$ variables is large, or statistically significant, under any specification.[42]

4.2. Checking for threats to validity

There are three main concerns with the difference-in-difference strategy that we use to identify the causal impact of the new law on wages and employment. One concern regards accurate inference: with such a small number of 'effective' units driving the main variation in the intensity of the law, we need to be cautious that we are not over-rejecting a null of zero effect. The results from the two-step estimator in the previous section address this concern. The other concerns relate to direct threats to the validity of our causal estimates.

The primary threat to validity is that provinces with different pre-law WG_j measures may also experience differential economic trends in the $POST_t$ period or contemporaneous shocks, which could account for our estimates of α_3. Having more prior years of data generally helps to rule out a differential trends explanation; however, as mentioned before, we are limited in the amount of pre-law data we can use to investigate this. Instead, we tackle this issue by understanding more about where the underlying wage gap variation is coming from, and by implementing a placebo test.

First, it is useful to consider the type of differential trend that could confound the wage and employment results of the previous section. Recall from Fig. 3 that, when ordering provinces on WG_j, the Western Cape, Gauteng, Mpumalanga and KZN emerge as the highest paying provinces prior to the law. Three of these four provinces comprise the "economic centers" of South Africa, the provinces that contain the three largest cities of Johannesburg, Durban and Cape Town. These are areas where labor demand in general is much

[41] Results for the difference-in-differences coefficient in the wage and hours regressions are the same if we also include a full set of province fixed effects to control for level differences in wages across provinces.

[42] Other types of extensive margin adjustments (of type rather than number of domestic workers) may have altered the composition of domestic workers and contributed to observed earnings shifts. For example, in the *POST*-period, employers might try harder to select higher quality workers once the law is in place and as a result pay these higher quality domestic workers more. That these changes drive the results we see is ruled out in our propensity-score reweighting exercise in Appendix **2**.

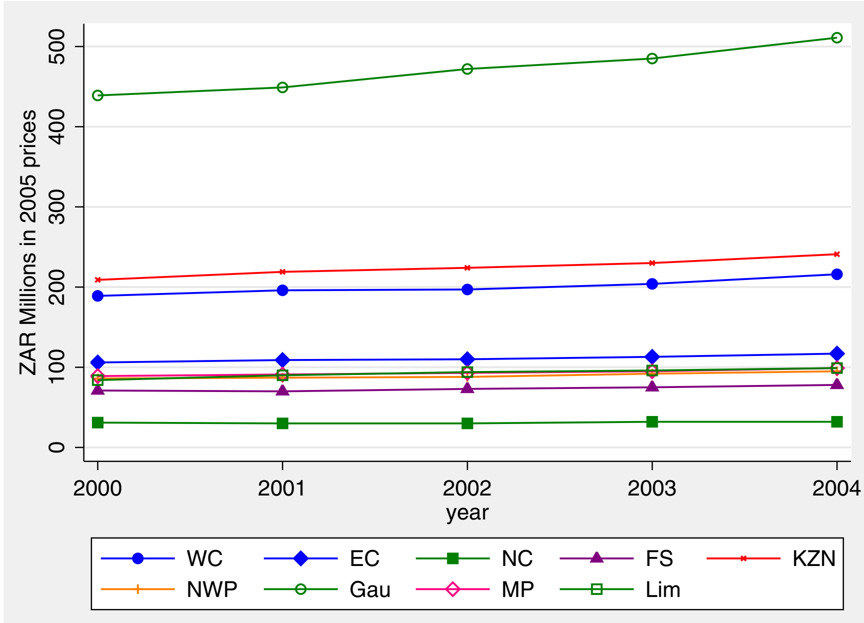

Fig. 4. Real GDP by province and year. Data are from National Treasury. Provinces are: Western Cape (WC), Eastern Cape (EC), Northern Cape (NC), Free State (FS), Kwazulu-Natal (KZN), North West Province (NWP), Gauteng (Gau), Mpumalanga (MP) and Limpopo (LIM).

stronger than in other parts of the country; they jointly account for close to two-thirds of the country's GDP. We can see this in Fig. 4 which presents annual real GDP in millions of ZAR at the province level for each year from 2000 to 2004. The level of output produced by Gauteng, KZN and the WC clearly dominate the contributions of the other provinces. For our positive estimate of α_3 to be driven by differential labor demand, we would need to see strong improvements in the provincial economies with the largest wage gap values (i.e. Limpopo, the Eastern Cape, the North West Province, the Free State, and the Northern Cape), or strong declines in the provincial economies with the smallest wage gap values. This is not, in fact, what we see in Fig. 4: over time the small wage gap provinces experience some trend growth in annual GDP, while provinces with larger wage gaps show no signs of strong positive trends in GDP growth. Strong labor demand trends that differ across provinces are therefore unlikely to explain the large wage effects we find in Table 2, and the lack of significant negative employment effects in Table 3.

We can more formally rule out the possibility that high wage gap provinces experienced large general shocks to their economies at the same time as the minimum wage law came into effect using a placebo test. We ask: do wages rise (hours of work fall) in high wage gap relative to low wage gap provinces, for workers who are similar to

Table 5
Difference-in-differences for male manufacturing workers.

	Log hourly wages				Weekly hours of work			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Placebo test using manufacturing worker wages/hours and the pre-law domestic worker wage gap								
POST	0.103***	0.0956***	0.089	0.09	−0.913***	−0.880***	−0.755	−0.736
	(0.01)	(0.01)	(0.05)	(0.05)	(0.21)	(0.22)	(0.593)	(0.554)
Pre-law wage gap	−0.263**	−0.240**	−0.242**	−0.203*	2.208**	1.670*	2.461*	2.013
	(0.08)	(0.09)	(0.09)	(0.08)	(0.95)	(0.82)	(1.101)	(0.957)
Domestic wage gap*POST	0.042	0.042	0.002	0.003	1.366**	1.208*	0.795	0.744
	(0.04)	(0.04)	(0.13)	(0.13)	(0.59)	(0.55)	(1.438)	(1.271)
N	3631	3631	18	18	4824	4824	18	18
B: Robustness check for mean reversion using manufacturing worker wages/hours and the pre-law manufacturing worker wage gap								
POST	0.107***	0.100***	0.0888**	0.0894**	−1.050***	−0.999***	−0.684	−0.671
	(0.015)	(0.014)	(0.033)	(0.037)	(0.268)	(0.269)	(0.825)	(0.740)
Pre-law wage gap	−1.038***	−0.883**	−0.981***	−0.811***	3.321	0.029	−1.302	−1.992
	(0.238)	(0.290)	(0.245)	(0.183)	(4.881)	(3.695)	(6.085)	(5.308)
Manufacturing wage gap*POST	−0.097	−0.137	−0.146	−0.159	0.993	−0.275	0.877	0.591
	(0.200)	(0.156)	(0.339)	(0.301)	(2.567)	(2.553)	(8.197)	(7.211)
N	3631	3631	18	18	4824	4824	18	18

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. Each regresssion contains a constant (coefficient not shown). In columns 1, 2, 5 and 6, standard errors are Eicker-White, clustered at the province level. Critical values for significance of between estimates and for two-step estimates are taken from the t-distribution, see text for details. Sample includes all African and colored male workers of relevant age and education level employed in manufacturing, in all waves excluding the cusp wave September 2002. POST = 1 in March 2003, September 2003 and March 2004; otherwise zero.

Panel A: Pre-law wage gap is the province-level difference in the log(4.1) − log(median wage), where 4.1 is the urban full-time minimum wage introduced in November 2002 and median wage is the median wage of domestic workers in each province across all of the pre-waves (September 2001 and March 2002). Panel B: Pre-law wage gap is the province-level difference in the log(4.1) − log(median manufacturing worker wage), where 4.1 is the urban full-time domestic worker minimum wage introduced in November 2002. Median manufacturing worker wage is defined at the province level across all of the pre-waves (September 2001 and March 2002).

domestic workers but who are not affected by this law? If the answer is yes, we might be concerned that our estimate of α_3 is not really picking up the impact of the minimum wage law.

Table 5 Panel A presents the results of this placebo experiment. We implement our main estimating equation in (1) for male manufacturing workers who are employed in urban areas, between age 20 and 59 (inclusive), are African or colored and who have no more than a high school level of education. The first four columns of the table present OLS and two-step estimator results for log hourly wages, and the final four columns present OLS and two-step estimator results for weekly hours of work. For brevity, we only present the results that exclude the September 2002 "cusp" wave.

For this sample of manufacturing workers, wages are 9 to 10% higher in the *POST* period, although this change is not significant once we take into account the grouped nature of the data (columns 3 and 4). Importantly, the change in wages for manufacturing workers in high wage gap areas does not seem to be significantly higher in the *POST* period. The coefficients on $WG_j{}^*POST_t$ are small and statistically insignificant. Hours of work are higher in the post period in high wage gap areas (columns 5 and 6), although these are again, not significantly different from zero once we take into account the grouped nature of the error term in Eq. (1) in the presence of a small number of groups. These estimates together with Fig. 4 provide some evidence that our difference-in-differences estimates of the wage effects of the law for domestic workers in Table 2 do reflect the impact of the law and not simply positive wage trends or contemporaneous positive shocks in high wage gap provinces.

A final concern posing a threat to the validity of our estimates of α_3 relates to mean reversion in wages: wages might rise in "more intensely treated" province simply because of mean reverting measurement error. To some extent, using province-level aggregate data insulates us from the worst forms of this—the problem would be more severe if our wage gap measure was computed at the level of a smaller geographic entity with fewer observations per unit, and worse still if we were using individual level data to define this treatment measure. We can, however, provide some evidence against mean reversion explaining our results even at the provincial level, using the sample of male manufacturing workers. If wages are strongly mean-reverting, then this should show up as low paid workers (based on a measure of manufacturing wages) being paid more after the law, even for workers who are unaffected by the law.

To implement this test, we regress the wages of male manufacturing workers on a $POST_t$ indicator, a different wage gap measure that captures the difference between the domestic worker minimum in 2002 (an arbitrary benchmark) and the median manufacturing worker's wage at the province level before 2002, and their interaction. Interestingly, the correlation between this wage gap measure and our original wage gap measure for domestic workers is relatively low at 0.4. Because of this, the mean reversion test is quite a different test than our placebo experiment.

The results of testing the mean reversion hypothesis for log male manufacturing worker wages are shown in Panel B of Table 5. Focussing on the coefficients on the interaction terms, there is no clear indication of mean reversion in the data. None of the coefficients are close to statistically significant at conventional levels. We take this as supportive evidence that mean reversion in wages (or hours of work) for domestic workers cannot account for the main wage results.

4.3. Testing for partial compliance

In this section, we provide suggestive evidence that some of the wage increases that occur after the law are only in partial compliance with the minimum. This is important to show, because it gives us some sense of how this labor market operates. It suggests that the effects of the law are not simply driven by a subset of employer-types who want to abide by the letter of the law; rather some employers are voluntarily choosing whether and by how much to comply with the law.

Table 6
Testing for partial compliance in response to the law.

Change in predicted probability of domestic worker reporting nominal wages in the near-compliant region	Controlling only for POST $\delta_1 = 0.9*\text{wmin}$		Controlling for POST, WG and POST*WG $\delta_1 = 0.9*\text{wmin}$	
	No controls	All controls	No controls	All controls
	(1)	(2)	(3)	(4)
$\Pr(w^*=1\|POST) - \Pr(w^*=1\|PRE)$	0.007***	0.007***	0.012***	−0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
$\Pr(w^*=1\|POST^*WG) - \Pr(w^*=1\|PRE^*WG)$		0.047***	0.002***	
		(0.004)	(0.000)	
N workers	5205	5205	5205	5205

*10%, **5%, and ***1% significance level. Coefficients are predicted changes in the probability of earning a wage in the "near compliant" bin where the predictions are generated from estimated coefficients from an ordered probit model (using Stata's non-linear prediction command). The three groups in the ordered probit are: non-compliant (w = 0), near-compliant (w = 1) and compliant (w = 2) and we defined near compliant as having a wage 10% below the minimum or less. In columns (3) and (4), we compute the predictions taking into account the impact of an interacted variable in a non-linear model. Sample excludes the cusp wave (September 2002). Each specification includes an indicator for whether the observation is captured PRE or POST law. Columns (2) and (4) present results from models which control for age, race, years of education. Columns (3) and (4) present results from the specification that controls for a POST indicator, a measure of the log wage gap in the province before the law was in effect, and the interaction of POST with wage gap (WG) (i.e. the difference in differences specification).

Table 6 presents results from the ordered probit model we estimate for the outcome variable that classifies a worker's wage into a range below, near or above the minimum in each wave, where near is defined as being paid 90–99% of the minimum wage.[43] Recall from the discussion above (and Appendix 1), the idea of the test is that if the probability of a worker reporting a wage in the non-compliant bin falls by more than the probability of a worker earning a wage in the compliant bin rises, then some workers are earning more than they were before, but not enough more to bring them into compliance with the law. This is equivalent to testing whether the probability of a worker being in the "near compliant" bin rises after the law, and the test is informative as long as there are no large employment reductions in response to the law, which we showed in Tables 3 and 4.

Since there are a range of coefficients we could report from the ordered probit, we isolate the marginal effects for the $POST_t$ indicator (all columns) and the interaction term (last two columns) on the probability of being classified in the "near compliant" bin. In the first two columns, our specification excludes the $POST_t * WG_j$ control; the interaction term is included in the specification underlying the estimates in the second columns. We compute these marginal effects using non-linear prediction methods.

To interpret results, consider the coefficients in column (1). In the *POST* period, the probability of a worker reporting hourly wages in the "near compliant" range increases by a significant 0.7 percentage points: more workers are squeezing into the narrow band near the minimum. This result looks across all workers and compares the pre- to the post-period. Columns (3) and (4) show that this shift in the direction of the wage floor is more pronounced in higher wage gap provinces, after

[43] Results are similar whether we choose the cut-off for being "near" the minimum as 20% or less than the minimum.

the law. Without demographic controls, there is a 4.7 percentage point increase in the probability of a worker earning a wage close to the minimum; this change falls to 0.2 percentage points when we add demographic controls, yet is still statistically different from zero.

We interpret these results as indicating partial compliance with the law at the lower parts of the wage distribution. The evidence on increasing dispersion (variance) in wages and earnings in Table 1 and the graphical evidence on increases in mean log hourly wages for domestic workers even in the Western Cape (Fig. 3)—the only province where the mean wage was initially above the minimum—point toward an employer response at levels much higher in the distribution. Together, these results suggest that some employers are responding to the law in ways different to those predicted by conventional compliance models. In the next section, we present a final test that tries to rule out the possibility that employer responses are driven by a desire to avoid penalties associated with non-compliance.

4.4. Testing for compliance related to probability of audit

To learn more about how much employer behavior may be driven by the threat of external sanctions, we investigate the responsiveness of wages to the presence of a local LC. While this is not the only aspect of a local labor market that could increase the likelihood of being caught for non-compliance, it is a plausible feature that distinguishes markets and it is feasible to obtain data on the location of these offices. Since dealing with worker complaints is one of the three main responsibilities of these LCs, living nearby an LC should capture a lower cost of complaint for domestic workers, regardless of whether or not these workers actually use these labor centers in equilibrium.[44] Hence, having an LC nearby should increase the actual probability of audit as well as employer beliefs about their likelihood of being monitored.

We tracked down the physical addresses of these LCs and matched each one to a unique magisterial district in the LFS, a geographic unit that is more disaggregated than the province. Since the LCs were primarily established to serve formal sector workers in the rest of the economy, it is unlikely that their location is endogenous to the prevalence of domestic worker employers or to the presence of non-compliant employers of domestic workers; however, the LCs are likely to be over-represented in areas with more economic activity in the formal sector. Across all waves, 75% of domestic workers live in magisterial districts where there is at least one LC.

In Table 7, we estimate wage regressions of the form in Table 2, now including a control for whether the domestic worker lives in a magisterial district with an LC, the interaction of this indicator with $POST_t$, with WG_j and the triple interaction $POST_t{}^*WG_j{}^*LC_{ijt}$. We estimate Eq. (1) again using OLS (and clustered standard errors) as well as using the two-step estimator and present results for the sample excluding September 2002, for brevity. Interestingly, the OLS results indicate that domestic worker wages are about 12% higher in areas that have an LC (in columns (1) and (2)) and that LCs tend to offset the effect of working in a province where the median wage in the pre-period is below the minimum (coefficient on LC^*WG). However, neither of these differences are evident once we estimate the model using the two-step procedure. Furthermore, there seems to be no indication that having an LC in one's local labor market increases the impact of the minimum wage for domestic workers; employers who raise wages are not doing so differentially in areas where the likelihood of being caught for non-compliance is higher. Although it is never possible to precisely estimate the coefficient on LC^*WG^*POST, the coefficient on this triple-difference term is negative, pointing in the wrong direction for the mechanism that has employers responding more in areas with a higher probability of audit.

[44] LCs also pay out UIF claims and deal with general enquiries, see http://www.labour.gov.za/contacts/Labour%20Centres/labour-centres-gauteng-south for details. The Labor Centers are also, practically, where labor inspectors actually work. These inspectors are responsible for enforcing sectoral determinations across all industries (there were 57 inspectors per 1 million workers in 2007 (Bhorat et al., 2010)) and so are stretched a bit thin, reducing the overall audit probability for any one industry. However, as noted by Bhorat et al. (2010), inspections by these labor centers are generally "triggered by clients" and investigated on a case-by-case basis.

Table 7
Wage responses in areas with high versus low costs of complaint: Difference-in-differences.

	OLS		Two-step estimator	
	(1)	(2)	(3)	(4)
POST	0.258***	0.257***	−0.155	−0.164
	(0.02)	(0.02)	(0.23)	(0.23)
Pre-law wage gap (WG)	−1.008***	−0.979***	−0.483	−0.422
	(0.04)	(0.06)	(0.30)	(0.30)
Pre-law wage gap (WG)*POST	0.180**	0.196***	0.804	0.812
	(0.06)	(0.05)	(0.61)	(0.64)
Labor Center	0.122***	0.121***	−0.12	−0.058
	(0.02)	(0.02)	(0.20)	(0.20)
Labor Center*POST	−0.0568	−0.0716**	0.527	0.52
	(0.03)	(0.03)	(0.32)	(0.32)
Labor Center*WG	0.175***	0.193***	−0.647	−0.672
	(0.04)	(0.05)	(0.48)	(0.48)
Labor Center*WG*POST	−0.079	−0.0884	−0.807	−0.798
	(0.07)	(0.07)	(0.89)	(0.93)
Controls for age, education, African?	N	Y	N	Y
N	5205	5205	36	36

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. Each regression contains a constant (coefficient not shown). In columns 1 and 2 standard errors are Eicker-White, clustered at the province level. Critical values for significance for two-step estimates are taken from the t-distribution, see text for details. Sample includes domestic workers in all waves excluding the September 2002 "cusp" wave. POST = 1 in March 2003, September 2003 and March 2004; otherwise zero. Labor Center = 1 if the domestic workers in a magisterial district that contains at least one Labor Center (LC).
Pre-law wage gap is the province-level difference in the log(4.1) − log(median wage), where 4.1 is the urban full-time minimum wage introduced in November 2002 and median wage is the median wage of domestic workers in each province across all of the pre-waves (September 2001 and March 2002).

4.5. Contract coverage and related employment benefits

Since the sectoral determination for domestic workers mandated formal labor contracts and some additional rights and benefits, it is likely that the law also impacts the conditions of work for domestic workers, over and above any direct effects on wages. We already see strong evidence of this in Table 1, with the large increases in contract coverage, UI and pension benefits after November 2002. In this section, we investigate whether workers originally paid below the minimum wage are more likely to also see improvements in their employment benefits and legal rights (in terms of formal contract coverage).

In Table 8, we present difference-in-differences comparisons for whether a worker has a formal written contract of employment, whether the employer contributes to an unemployment insurance fund, and whether the employer contributes to a pension fund. The coefficient on the *POST* indicator reflects the large differences in pre-post means that were seen in Table 1. The fraction of workers with a formal job contract rises between 17 and 20 percentage points, the fraction of workers with UI coverage rises by 18 to 20 percentage points and the fraction enjoying pension contributions rises by 6–7 percentage points. Notice that workers in high wage gap provinces are not likely to experience larger improvements in these conditions of work relative to low wage gap provinces after the law. Although the interaction terms for the UI outcomes are negative in the OLS regressions, none of the interaction terms is statistically significant in any of the two-step estimation results.

While it may not be surprising that improvements in employment benefits and rights are unrelated to initial levels of non-compliance with the wage part of the minimum wage law, it is striking that

Table 8
Improvements in domestic worker employment rights and benefits: Difference-indifferences.

	OLS		Two-step estimator	
	(1)	(2)	(3)	(4)
A: Worker has a formal job contract with employer				
POST	0.173***	0.170***	0.200***	0.196***
	(0.02)	(0.02)	(0.03)	(0.03)
Pre-law wage gap (WG)	−0.101**	−0.073**	−0.100**	−0.075**
	(0.03)	(0.03)	(0.03)	(0.02)
Pre-law wage gap WG*POST	−0.07	−0.06	−0.07	−0.07
	(0.05)	(0.05)	(0.07)	(0.06)
Age, education, African controls	N	Y	N	Y
N	5743	5743	18	18
B: Employer makes UIF contributions on behalf of worker				
POST	0.184***	0.181***	0.205***	0.202***
	(0.01)	(0.01)	(0.02)	(0.02)
Pre-law wage gap (WG)	−0.0372**	−0.017	−0.0387***	−0.0217*
	(0.01)	(0.01)	(0.01)	(0.01)
Pre-law wage gap WG*POST	−0.0731*	−0.0674*	−0.054	−0.052
	(0.03)	(0.03)	(0.05)	(0.04)
Age, education, African controls	N	Y	N	Y
N	5817	5817	18	18
C: Employer contributes to a pension fund				
POST	0.0653***	0.0634***	0.0707***	0.0680***
	(0.01)	(0.01)	(0.01)	(0.01)
Pre-law wage gap (WG)	−0.0439***	−0.0500**	−0.0424***	−0.508**
	(0.01)	(0.02)	(0.01)	(0.01)
Pre-law wage gap WG*POST	−0.039	−0.040	−0.037	−0.037
	(0.03)	(0.03)	(0.03)	(0.03)
Age, education, African controls	N	Y	N	Y
N	5815	5815	18	18

*10%, **5%, and ***1% significance level. Robust standard errors presented in each column and all regressions are weighted. All outcomes are binary, and each regression contains a constant (coefficient not shown). In columns 1 and 2, standard errors are Eicker-White, clustered at the province level. Critical values for significance for two-step estimates are taken from the t-distribution, see text for details. Sample excludes domestic workers in the September 2002 "cusp" wave. POST = 1 in March 2003, September 2003 and March 2004; otherwise zero. Pre-law wage gap is the province-level difference in the log(4.1) − log(median wage), where 4.1 is the urban full-time minimum wage introduced in November 2002 and median wage is the median wage of domestic workers in each province across all of the pre-waves (September 2001 and March 2002).

after the new Sectoral Determination comes into place, workers across the board are more likely to have a formal job contract, UI and pension benefits. Considering the low baseline contract coverage rates (10%), UI coverage (2%) and pension coverage (3%), this more than doubling of contract and pension coverage rates, and tripling of UI coverage, in the year after the law is enacted is remarkable. It is unlikely that anything other than the introduction of the minimum wage law could have had such immediate impacts on the conditions of work for these workers. Importantly, nothing in the Sectoral Determination made pension contributions mandatory for domestic worker employers. Although pension contributions are stipulated in the general Basic Conditions of Employment Act covering all workers, as we noted earlier, the BCEA was seldom adhered to in the informal sector. The large increases in the fraction of domestic workers enjoying pension contributions after the law suggests that the introduction of the minimum wage law may have been a catalyst for employers to start adhering to other aspects of general labor legislation.

We view this last set of results as particularly important, because they indicate that with the introduction of sector-specific protections for a previously informal industry, there is a shift in the employment relationship towards the more formal. Workers gain legal protections and real benefits previously denied, and so the new regulation appears to have initiated the formalization of the industry. This process could have far-reaching consequences for the nature of domestic work in South Africa.

5. Discussion and conclusions

The introduction of a new minimum wage law for an informal market presents a unique opportunity to examine important issues around responses to legal wage floors. It also allows us a window onto how informal labor markets operate and the conditions under which they might become more formal.

Although conditions in and characteristics of the domestic worker industry in South Africa were stable before the introduction of a minimum wage, the difference-in-differences results clearly indicate that domestic workers who worked in areas where the pre-law median wage was below the minimum (i.e. where the new law had more "bite") experienced large increases in wages in the *POST*-period: on average, wages rose 13 to 15% for workers in provinces with the mean wage gap. Despite the absence of full compliance and a sharp wage spike at the minimum, we find evidence of a strong wage response to the law, and little statistical evidence of work reductions on the intensive or extensive margins. The main purpose of our paper has been to document these changes and to provide evidence that labor market regulation in an informal sector of considerable importance can have a real and immediate impact, even with very limited monitoring and enforcement. The results points to a highly inelastic demand for domestic workers among private households, at least in the short run. Our final set of results also point to significant improvements in the conditions of work across the entire domestic work sector. They are particularly useful in showing that real formalization of the industry is a likely consequence of the new laws.

Given that issues of compliance are not often at the center of empirical work on minimum wages, it is worthwhile considering how our results should be interpreted. One possible reason that the law had an effect is that some fraction of employers erroneously believed that the government was going to enforce the new minimum and penalize non-compliers. A difficulty with this explanation is that it cannot account for wage or earnings increases for domestic workers already paid more than the minimum before the law. Although we do not have a panel data set of workers to test this directly, the fact that the variance of the wage and earnings distribution increases rather than compresses after the law suggests that some employers increased wages in excess of the minimum, despite already being compliant with the law.

A second reason that a new and largely unenforced law may have been effective relates to models of fairness in wage-setting. A theoretical literature in labor economics posits that the notion of a fair wage is important in incentivizing workers to provide high effort in tasks for which effort is unobservable (Akerlof, 1982, 1984 and Akerlof and Yellen, 1988). These models are difficult to test empirically, since defining and measuring a fair wage, or a reference wage, is tricky in practice.[45] Experimental studies have separately established the importance of gift exchange and fairness intentions in employer–employee relationships.[46] In the case of the domestic worker industry in South Africa, it is plausible that the announcement of a wage floor

[45] Mas (2006) is a notable exception.

[46] See for example Brandts and Charness (2004), who show that a minimum wage may undermine the efficiencies that gift-exchange can achieve, Falk et al (2000) discuss how fairness intentions matter for behavior and Fehr et al (1997) model how reciprocity in response to fair behavior expands the set of enforceable contracts that can be sustained in an economy. Konow (2003) reviews the large body of experimental evidence on fairness. Falk et al. (2005) provide a model and experimental evidence that the announcement of a minimum wage raises workers' reservation wages in a persistent way. Reservation wages remain high even after the wage floor is removed, suggesting that policy interventions may have direct and indirect effects on behavior by altering the meaning of a fair transaction and by creating 'entitlement' effects.

defined, or re-defined, what the fair wage was and set in motion voluntary employer responses, even though the two traditional channels for encouraging compliance—enforcement and penalties—were largely closed off.[47] What we take from our analysis is that in the initial stages of labor market formalization, governments may need to accept partial compliance with new legislation in order to bring about real changes in outcomes without significant disemployment.[48] Even with very limited enforcement, such sector-specific legislation can move the market towards a more formal setting, if, for example, it increases contract coverage, as in the South African case.

We emphasize that our conclusions are valid for the domestic worker industry, which is one example of an informal sector. The dynamics of the employment relationship between a single employer and a single employee no doubt condition the response to the law and so may only be relevant in some types of informal enterprises. However, this characterization of the informal sector may not be too far from a description of the typical small-scale firm that generates much informal sector employment in developing countries.[49] Finally, our analysis is only relevant for the short-run effects of a new minimum wage policy. While we find clear effects in the 16 months after the law, as employers face annual increases in this minimum (often above inflation in the case of South Africa) and as workers sort across space in response to these new protections, the wage, earnings and employment effects of the policy may change as the sector itself becomes more formal over time.

Appendix 1. Testing for evidence of incomplete wage adjustment

Consider Fig. A.1 below which shows the wage distribution F(.) of domestic worker wages in the period before and after the law. F_{After} is shifted to the right of F_{Before} and the solid black vertical line at δ_2 represents the minimum wage level. Define δ_1 as 90% of the minimum wage. Then, classify all worker wages into one of three groups: wages below δ_1 are non-compliant, wages between δ_1 and δ_2 are nearly compliant, and wages at or above δ_2 are compliant.

We would like to know whether the shift in the wage distribution in response to the law is consistent with incomplete adjustment up to compliant levels. That is, are wages shifting up out of the bottom part of the distribution, but not enough to get most workers up to or over the minimum threshold? Some shifts in the distribution are consistent with this idea and some are not. In example 1 Appendix 1 Table 1, the first set of changes in the fraction of workers in each region would be consistent with complete adjustment for some workers up to compliance: 10% of workers in each of the non-compliant and near-compliant regions leave these regions; and 20% of workers join the compliant region. The second example is, however, not consistent with full compliance for workers who experience wage changes: 20% of workers leave the non-compliant region, and the near-compliant region grows by 10% of workers as does the compliant region. While we cannot say which exact workers are getting wage increase, as long as jobs are not being lost as a result of the law change, then such shifts in the overall distribution reflect partial adjustments in response to the law.

Intuitively, we would like to know whether or not the fall in the fraction of domestic workers in the non-compliant region is fully offset by the increase in fraction of workers in the compliant region. We do this by implementing an ordered probit model and testing a specific hypothesis. To describe the appropriate hypothesis, consider the following linear model of the log of the individual worker's wage (w_i) (ignoring covariates for now) where $\epsilon_i \sim N(0,1)$: $w_i = \beta_0 + t\beta_1 + \epsilon_i$, where $t = 1$ if the worker is observed in the after period and $t = 0$ if observed in the before period. We define an ordered categorical variable, w_i^* that captures the region that each wage falls into:

Appendix 1 Table 1

	Example 1: Fraction of distribution in each region		
	Non-compliant region	Near compliant	Compliant
Before	0.3	0.3	0.4
After	0.2	0.2	0.6
	Example 2: Fraction of distribution in each region		
	Non-compliant region	Near compliant	Compliant
Before	0.3	0.3	0.4
After	0.1	0.4	0.5

$$w_i^* = 0 \qquad if \qquad w_i \le \delta_1 \tag{1}$$

$$w_i^* = 1 \qquad if \qquad \delta_1 < w_i \le \delta_2 \tag{2}$$

A test for partial compliance

$$w_i^* = 2 \qquad if \qquad w_i > \delta_2 \tag{3}$$

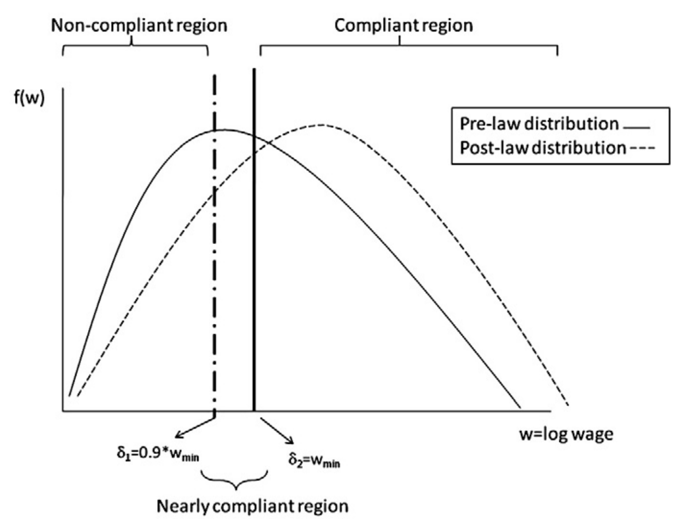

Fig. A.1: A test for partial compliance.

Then, the probability that any worker's wage falls into a particular bin can be described as:

$$\begin{aligned} Pr(w_i^* = 0) &= Pr(w_i \le \delta_1) \\ &= Pr(\beta_0 + t\beta_1 + \epsilon_i \le \delta_1) = \Phi(\delta_1 - \beta_0 - t\beta_1) \end{aligned} \tag{4}$$

$$Pr(w_i^* = 1) = Pr(\delta_1 < \beta_0 + t\beta_1 + \epsilon_i \le \delta_2) = \Phi(\delta_2 - \beta_0 - t\beta_1) - \Phi(\delta_1 - \beta_0 - t\beta_1) \tag{5}$$

$$\begin{aligned} Pr(w_i^* = 2) &= Pr(\beta_0 + t\beta_1 + \epsilon_i \ge \delta_2) = 1 - Pr(\beta_0 + t\beta_1 + \epsilon_i \le \delta_2) \\ &= 1 - \Phi(\delta_2 - \beta_0 - t\beta_1) \end{aligned} \tag{6}$$

[47] Rebitzer and Taylor (1995) present a theoretical model of a labor market in which employment increases when the minimum wage increases, as a result of an efficiency wage. However, they assume compliance with the law in their model. They focus on showing how an efficiency wage model can predict an increase in labor demand following a minimum wage change.

[48] See Basu et al. (2007) for a theoretical discussion of why non-compliance with a minimum wage could be acceptable.

[49] Banerjee and Duflo (2007) show that small-scale firms and entrepreneurial activities are an important source of income for the poor.

The hypothesis we would like to test is the following:

$$H_0: \frac{\partial Pr(w_i^*=2)}{\partial t}+\frac{\partial Pr(w_i^*=0)}{\partial t}\geq 0 \tag{7}$$

$$H_A: \frac{\partial Pr(w_i^*=2)}{\partial t}+\frac{\partial Pr(w_i^*=0)}{\partial t}<0 \tag{8}$$

Let the parameter combination described in the hypothesis statement be γ. If we reject $\gamma\geq 0$, this provides evidence consistent with some domestic workers getting wage increases that are up to a level less than the minimum, i.e. up to a new but still non-compliant level in region $\delta_2-\delta_1$.

Implementing this test is straightforward. To see this, note that we can write each of the partial derivatives in this way:

$$\frac{\partial Pr(w_i^*=2)}{\partial t}=\beta_1\phi(\delta_2-\beta_0-t\beta_1) \tag{9}$$

$$\frac{\partial Pr(w_i^*=0)}{\partial t}=-\beta_1\phi(\delta_1-\beta_0-t\beta_1) \tag{10}$$

This allows us to redefine γ as:

$$\gamma=\frac{\partial Pr(w_i^*=2)}{\partial t}+\frac{\partial Pr(w_i^*=0)}{\partial t} \\ =\beta_1[\phi(\delta_2-\beta_0-t\beta_1)-\phi(\delta_1-\beta_0-t\beta_1)] \tag{11}$$

Finally, note that:

$$\frac{\partial Pr(w_i^*=1)}{\partial t}=-\beta_1[\phi(\delta_2-\beta_0-t\beta_1)-\phi(\delta_1-\beta_0-t\beta_1)]=-\gamma \tag{12}$$

We use this last partial derivative to re-state the null and alternative hypotheses:

$$H_0: -\gamma\leq 0 \tag{13}$$

$$H_A: -\gamma>0 \tag{14}$$

Appendix 2

In words, we want to know whether the probability of being in the near compliant region rose in the after period, relative to before the law was in place.

We can also extend the test to a framework that includes a measure of the intensity of the law. Suppose that wages are given by $w_i=\beta_0+t\beta_1+WG_j\beta_2+WG_jt\beta_3+\epsilon_i$, where WG_j is the median wage gap for domestic workers in the local labor market during the period before the law. This implies that we can write the partial derivative with respect to t as:

$$\frac{\partial Pr(w_i^*=1)}{\partial t}=\left(-\beta_1-WG_jt\beta_3\right)[\phi(\delta_2-\beta_0-t\beta_1)-\phi(\delta_1-\beta_0-t\beta_1)] \tag{15}$$

Our test will involve examining the signs and significance of the marginal effect of t, at the mean level of the pre-law wage gap. We compute the marginal effects and associated standard errors of the *POST* and $POST_t{}^*WG_j$ measures using non-linear prediction methods in Stata.

Appendix 2. Checking whether composition effects explain the shift in wage distributions

We use a simple propensity score re-weighting technique (as in DiNardo et al. (1996)) to show that the shift in the distribution of observable characteristics (Appendix 2, Fig. 1) for workers accounts for a small fraction of the actual shift in wages. We check whether the distribution of observable characteristics of domestic workers changes significantly over the period by estimating a probit model of the probability of being a domestic worker in the *PRE*-period ($y_i=0$) or the *POST* period ($y_i=1$) and plotting the distribution of predicted probabilities for each period in Appendix 2 Fig. 1. There is substantial overlap in the propensity scores in the two periods but also a noticeable rightwards shift in the distribution of scores *POST*-law. To check whether these changes can account for the large shifts in earnings we see after the law, we apply a propensity score weight to the earnings data of observations in the pre-period and graph three kernel density plots of the distribution of earnings reported by workers: the pre-law distribution, the *POST* distribution and the *POST* distribution re-weighted for the distribution of characteristics observed in the pre-period (as in DiNardo et al., 1996). Appendix 2 Fig. 2 shows that re-weighting in this way does not eliminate the shift in wages from the *PRE*— to the *POST*-period.

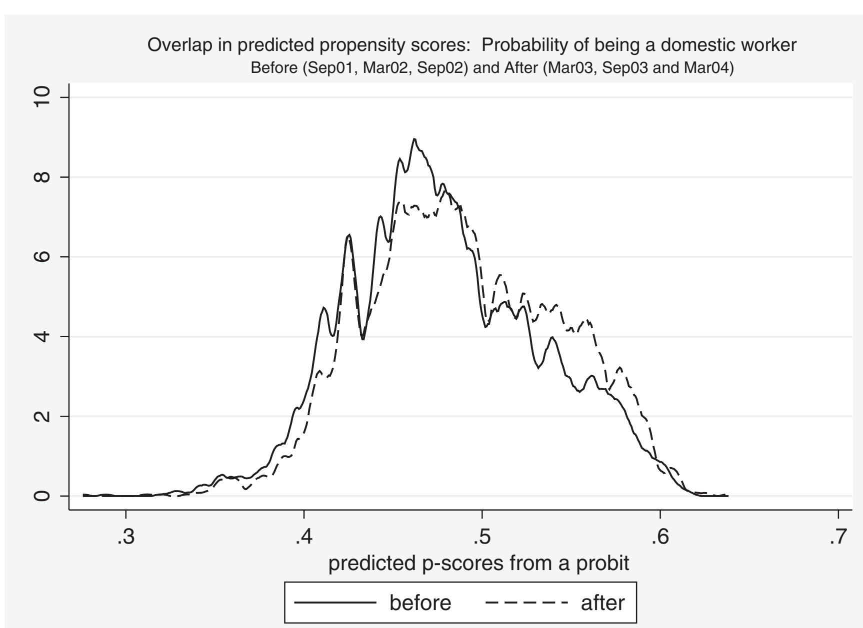

Fig. 1. Shift in observable characteristics of urban domestic workers.

Appendix 2

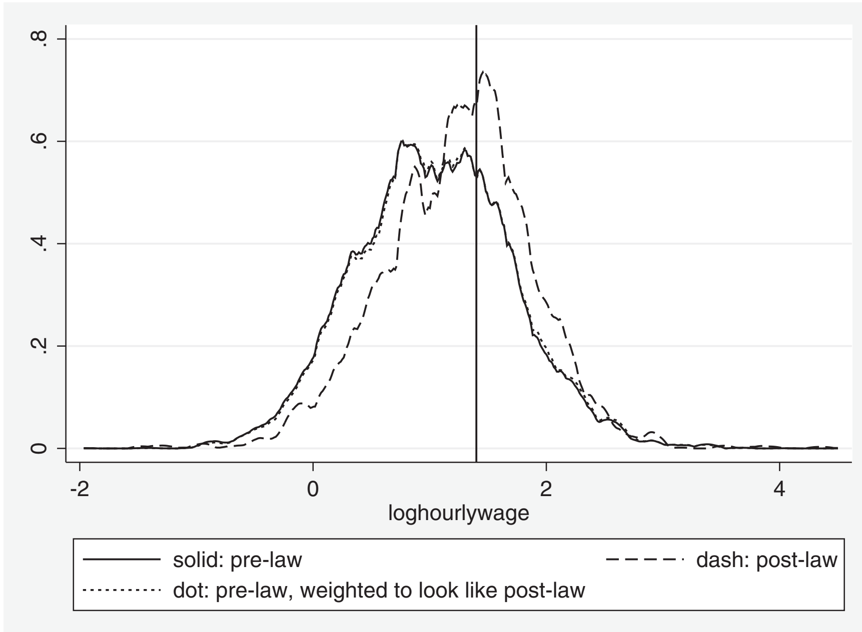

Fig. 2. Reweighted wage distributions.

Appendix 3

Appendix 3 Table 1: Log hourly wages of domestic workers: Difference-in-differences, unweighted.

	Two-step estimator				Excluding "cusp" wave			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	0.198***	0.188***	0.194***	0.183***	0.209***	0.197***	0.209***	0.197***
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
	[0.015]	*[0.017]*			*[0.022]*	*[0.039]*		
Pre-law wage gap (WG)	−0.829***	−0.808***	−0.808***	−0.787***	−0.874***	−0.850***	−0.848***	−0.822***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)
	[0.167]	*[0.118]*			*[0.149]*	*[0.148]*		
Pre-law wage gap (WG)*POST	0.109***	0.111***	0.102	0.103*	0.154***	0.157***	0.142**	0.147**
	(0.03)	(0.03)	(0.07)	(0.06)	(0.03)	(0.03)	(0.06)	(0.06)
	[0.052]	*[0.066]*			*[0.086]*	[0.129]		
Controls for age, education, African?	N	Y	N	Y	N	Y	N	N
N	6154	6154	18	18	5205	5205	18	18

Appendix 3 Table 2: Usual weekly hours of work of domestic workers: Difference-in-differences, unweighted.

	All waves				Excluding "cusp" wave			
	OLS		Two-step estimator		OLS		Two-step estimator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	−0.512	−0.446	−0.557	−0.471	−0.798	−0.664	−0.622	−0.516
	(0.62)	(0.59)	(1.73)	(1.54)	(0.88)	(0.82)	(1.75)	(1.54)
	[0.743]	[0.709]			[1.151]	[1.072]		
Pre-law wage gap (WG)	5.208	4.084	5.990**	5.219*	5.641	4.426	6.810**	5.910**
	(3.03)	(2.46)	(2.76)	(2.44)	(3.52)	(2.77)	(3.06)	(2.66)
	[5.794]	[4.452]			[6.234]	[4.818]		
Pre-law wage gap (WG)*POST	−2.695	−3.064*	−3.509	−3.636	−3.129	−3.536	−4.329	−4.42
	(1.58)	(1.59)	(4.10)	(3.77)	(2.25)	(2.15)	(4.31)	(3.91)
	[2.689]	[2.577]			[3.931]	[3.633]		
Controls for age, education, African?	N	Y	N	Y	N	Y	N	Y
N	6876	6876	6876	6876	5824	5824	18	18

Robust standard errors presented in each column. In columns 1, 2, 5 and 6, standard errors are Eicker-White, clustered at the province level. Significance at p<0.001***, p<0.05** or p<0.01* level. Critical values for significance for two-step estimates are taken from the t-distribution (9-4 = 5 degrees of freedom). See text for details. Standard errors in square brackets are block bootstrapped, treating the province as the block. Sample in columns 1–4 include domestic workers in all waves; sample in columns 5–8 exclude domestic workers in the September 2002 "cusp" wave.

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