8-1-2005

The Effect of Minimum Wages on the Employment and Earnings of South Africa's Domestic Service Workers

Tom Hertz
American University

Upjohn Institute Working Paper No. 05-120

Citation

Hertz, Tom. 2005. "The Effect of Minimum Wages on the Employment and Earnings of South Africa's Domestic Service Workers." Upjohn Institute Working Paper No. 05-120. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. https://doi.org/10.17848/wp05-120

The Effect of Minimum Wages on the Employment and Earnings of South Africa's Domestic Service Workers

Tom Hertz
Department of Economics, American University

Comments welcomed at: hertz@american.edu

First draft July 9, 2004
This version August 14, 2005

Abstract

Minimum wages have been in place for South Africa's one million domestic service workers since November of 2002. Using data from seven waves of the Labour Force Survey, this paper documents that the real wages, average monthly earnings, and total earnings of all employed domestic workers have risen since the regulations came into effect, while hours of work per week and employment have fallen. Each of these outcomes can be linked econometrically to the arrival of the minimum wage regulations. The overall estimated elasticities suggest that the regulations should have reduced poverty somewhat for domestic workers, although this last conclusion is the least robust.

Many thanks to the W.E. Upjohn Institute for Employment Research for financial support of this work; to Nersan Govender of the CCMA for data on referrals over time; and to participants in seminars at the Employment Policy Institute, the American University, and the 2004 Development Policy Research Institute / Trade and Industrial Strategies Project Annual Conference for helpful comments.

1) Introduction and Summary

In September of 2002, South Africa's 1.2 million domestic workers – about one million mostly African and Coloured women who work as housekeepers, cooks and nannies, and another 200,000 men, mostly gardeners – were granted new labor market protections, including the right to a written contract with their employers and the rights to paid leave, severance pay, and notice prior to dismissal (Department of Labour, 2002a). In November of 2002, a schedule of minimum wages including time-and-a-half provisions for overtime work went into effect. The minima were set *above* the median hourly wages that prevailed at the time, and so constitute a major intervention in South Africa's lowest-wage labor market. (The minima were increased in November of 2003 and again in November of 2004.) As of May 2003, employers were also required to register any domestic workers whom they employed for more than 24 hours a week with the Unemployment Insurance Fund (UIF) and to withhold UIF contributions from their paychecks. At that point employed and contributing domestic workers began to earn credits towards future potential UIF benefits, at a rate one day's benefit for every six days worked.

This paper attempts to determine what collective effect these regulations have had on wages, employment levels, hours of work, total earnings, and the conditions of employment of domestic workers, using data from the semi-annual Labour Force Surveys (LFS) of September 2001 through September 2004. After describing the survey data and some of its strengths and weaknesses, I present the results of a simple before-and-after comparison, with no adjustment for other factors that might have affected the market for domestic services. I then present some econometric evidence on the question of whether and to what extent the observed outcomes are in fact causally related to the introduction of the regulations.

I find that the average real hourly wages of domestic workers have indeed risen since the regulations went into effect, by almost 20%: from R3.74 (in September 2001 / February 2002) to R4.45 (in mid-2004, all at September 2004 prices).[1] This reflects a 22% increase in women's wages, and an increase for men of either 6.6 or 12%, depending on the sample definition, as will be explained below. Over this same period, the percentage of workers estimated to be earning less than the applicable hourly minimum fell from 75% to 63%.

These wage increases have been accompanied by a 5% reduction in average hours worked per week for women; for men the decrease was roughly 2%, and was not statistically

[1] The rand was worth US$0.153 in September 2004, meaning the average hourly wage rose from $0.57 to $0.68.

significant at the 10% level.[2] Employment of full and part-time women (not adjusted for full-time equivalency) also fell, by 10 or 12%. Strikingly, male employment *rose* by 14 or 15%.

The combined effect of the changes in wages and hours was that the average real monthly earnings of employed domestic workers increased by about 15% (9% for men, and 16% for women). For men, who benefited from the employment increase, *total* estimated earnings also rose, by about 27%. For women, however, the employment losses offset some of the wage gains, resulting in an estimated net increase in total earnings of 3.5 to 5%, figures that are not statistically different from zero. For men and women combined, total earnings rose significantly, by 8 or 9%.

The results for the non-wage outcomes include a rapid increase in the number of workers reporting that they have a written contract with their employer, and that UIF contributions are being withheld, although the rate of compliance with these two provisions would still appear to be less than 30%. The changing nature of the employment relationship may also be gauged by the number of cases brought before the Commission for Conciliation, Mediation, and Arbitration (CCMA), which has increased by roughly 50% since the regulations went into effect.

The econometric results are based on a cross-regional analysis of before/after changes in wages, hours, and jobs, and they generally support the proposition that the introduction of minimum wages caused average wages to rise, and hours of work and total employment to fall. For women, the minimum wage appeared to have no effect on employment in Year 1, but a significant negative effect in Year 2. For men, whose employment rose over time, there is nonetheless a detectable negative effect of the minimum wage, which also appeared to have been stronger in the second year than the first.

The combination of higher *total* real earnings but fewer people employed means that poverty and ultrapoverty could have risen or fallen, depending on where the winners and losers are located in the income distribution. Although the LFS data are not adequate for a full poverty analysis, I can compare the estimated elasticities of employment derived here to some critical values I estimated from earlier research, using the 1993 PSLSD data (Hertz 2002). This comparison suggests that the cumulative two-year changes in wages and employment brought about by the new regulations should, on balance, have reduced poverty somewhat, particularly for the households of male domestic workers, although this conclusion is necessarily tentative.

[2] Unless otherwise stated, the 10% threshold is assumed.

2) Data Issues

Waves 4 and 5 of the Labour Force Survey were undertaken in September of 2001 and February of 2002, before the domestic worker regulations were promulgated (in September of 2002). In the analyses that follow, these two waves are pooled and designated Year 0. I will present evidence that employers do not appear to have acted in anticipation of the regulations (which had been discussed in the media for some time), so that data from this period constitute a legitimate baseline. The next wave, from September of 2002, coincides with the introduction of the non-wage obligations, but predates the minimum wages, which only came into effect in November. This wave is omitted from the before/after comparisons, as being neither before nor after. Waves 7 and 8, from March and September of 2003 are pooled and treated as Year 1 of the minimum wage regime.[3] Waves 9 and 10, from March and September of 2004, are pooled and designated Year 2; note that higher minima were in effect for this period. In addition to reducing sampling variability, pooling the waves in pairs eliminates any seasonal effects (e.g. for gardeners) by combining a fall and spring survey for each year.

Survey Design

Each survey covers roughly 30,000 households and together Waves 4 through 9 constitute a rotating panel, with an intended replacement rate of 20% per wave.[4] The initial sample consisted of 10 households from each of 3000 clusters, which were drawn from the 1996 census master list of enumerator areas; these were stratified into 18 layers, representing the urban and rural areas of each of the nine provinces. In calculating the standard errors of the descriptive means and totals it is important to allow for contemporaneously correlated outcomes among individuals in the same cluster, as well as between individuals in a given cluster from one wave to the next. The sequential correlation derives both from the fact that 80% of the households from one wave were supposed to be re-interviewed in the next, and from the fact that the

---

[3] Domestic workers on farms initially were exempt, but subsequently were covered by an identical set of provisions in a separate sectoral determination, with an effective date of 1 March 2003, meaning that they too were entitled to minimum wages for the first time in the March 2003 survey (Dept. of Labour 2002b). Note also that the UIF registration requirement, which some have held to be more burdensome than the wage provisions, became effective on 30 April 2003, so its impact should first be noticed in the second half of Year 1.

[4] Personal and family IDs are not invariant across waves, making it difficult to match people over time. As a result, the actual share of people who are the same from one wave to the next is not known with precision, but it appears to be substantially less than the 80% target. To the best of my knowledge, these ID problems have prevented any researchers from exploiting the panel nature of the dataset, myself included.

remaining 20% of households were drawn from the same clusters as the households they were replacing.[5]

For the September 2004 survey (Wave 10), a new master sample was selected. The 3000 new clusters were drawn from the 2001 census list of enumerator areas, and were stratified by the 53 newly demarcated district councils that make up South Africa. This last wave is treated as independent of the first six. All told, for the pooled analysis of Years 0,1, and 2, there are 18+53=71 independent strata and 5984 clusters; half the clusters span waves 4 through 9, and the other half are unique to wave 10. Standard errors calculated under this design are larger than if clustering and stratification are ignored, although the size of the design effect varies considerably with the quantity being estimated. The standard error of total employment in Year 1, for example, rises by 70%; however, the standard error of the *change* in employment from Year 0 to Year 1 is only 8% larger than if the effects of clustering and stratification are ignored. For mean hourly wages, the increase in standard errors is much smaller, on the order of 13% for levels, and just 1% for changes.[6]

Weights

The sampling weights distributed with the 2001 and 2002 rounds of the LFS were based on the 1996 census, and yield a significantly different demographic distribution than that of the more nearly contemporaneous 2001 census. This inconsistency is troublesome, as the estimated domestic worker employment totals are quite sensitive to the demographic weights. The 2001 census, however, is believed to have misrepresented some key demographic proportions, including the proportion of the population who are women of working age, an obvious correlate

[5] As with the US Current Population Survey, it is dwellings that are revisited, not families. Families that move are not followed, but are instead replaced by the family that now occupies their former dwelling. Ashenfelter, Deaton and Solon (1986) note that families that occupy the same dwelling at different times are likely to be similar, so it is plausible that not much precision in measured changes over time is sacrificed by the failure to track down and re-interview the original family.

[6] Although stratification and clustering are acknowledged in the calculation of standard errors, not all of the benefits of the rotating panel design are exploited. Ashenfelter, Deaton and Solon (1986) observe that the most efficient estimator of the mean of a variable in a given period (as well its change over time) is derived from data from *both* periods. The formula requires that we know the proportion of people that are retained from one survey to the next, as well as the population correlation of the variable in question between the two periods. The difficulties in matching IDs across waves, noted above, have so far prevented me from estimating these parameters, and from pursuing this approach.

of domestic worker status.[7] Furthermore, the population growth rate used to extrapolate from 1996 to later years turned out to be almost twice as large as the actual growth rate, due to an initial failure to account for the HIV/AIDS pandemic.

Beginning with the March 2003 survey, the weights were recalculated to mirror the demographic distribution of the 2001 census, and then adjusted for estimated demographic changes between 2001 and 2003, including internal migration (Statistics South Africa 2003). The erroneous population growth rate assumptions, and the problems with the census proportions, were not corrected until the September 2004 survey (Wave 10), when improved mortality data became available (Statistics South Africa 2004, 2005). This resulted in a significant downward revision of the estimated size of the population, and hence of the level of employment. Statistics South Africa (known as StatsSA) has also published revised estimates of key labor market statistics for March 2004, and is working on a full set of revised weights for the earlier waves that will reflect the new mortality information. For the time being, however, 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 that are too large to be ignored.

To address this problem, I treated the most recent weights (those from Wave 10, which incorporate the updated HIV/AIDS mortality effects, and are believed to fix the problems with the census as well) as correct, and recalibrated the weights for Waves 4 through 9 to yield the same age, sex, race, province, and urban/rural distribution. I then applied StatsSA's most recent published estimates of annual population growth from 2001 to 2004 to adjust the total population size of the earlier surveys (Statistics South Africa 2004).[8] This resolves some of the worst inconsistencies in the weights, but at the price of introducing an artificial stability in the demographic distribution. Presuming the Wave 10 weights are in fact correct for September 2004, they are almost certainly incorrect for September 2001, and this could bias the descriptive and econometric results in various ways. However, if this bias is not too severe (i.e. if the net

---

[7] Personal communication with analysts at Statistics South Africa. The census reports that 52.2% of the population are female, while the September 2004 LFS reports 50.7%.

[8] The figure they report is a 5.77% growth rate from 2000 to 2005, which would imply 3.43% over the three study years, from September 2001 to September 2004. This estimate, however, is higher than those of the HSRC and the ASSA, reflecting StatsSA's lower mortality and HIV prevalence assumptions (Statistics South Africa 2004, Table 8).

change in the demographic proportions over three years has not been too great), it may be offset by the increase in precision that results from eliminating one component of sampling variability, namely, the fact that the sample's weighted distribution could vary over time, even if the population's demographic distribution were stable.

The results of these corrections are illustrated in Figure 1, which also introduces the question of sample definition. The bottom lines (solid and hollow circles) compare the originally published estimates of the number of domestic workers for March 2004 with StatsSA's revised estimates for that month. The difference is 166,000 people, or 20% of the revised total, demonstrating that the estimated number of domestic workers is quite sensitive to the choice of weights. The next line up (asterisks) implements the same definition of who is a domestic worker as is used by StatsSA, but applies my own modified weights. These figures coincide with the published estimates for September 2004, by construction, but are larger by about 50,000 in Year 0, and *smaller* by about the same amount in Year 1, for a net reduction of 100,000 workers (about 10% of the total) when calculating the change from Year 0 to Year 1. The revised official estimates produce only a very slightly downward sloping trend in domestic worker employment over three years, but the recalibrated weights result in much larger estimated employment losses, which will be discussed below.

Definition of domestic work

StatsSA's definition of domestic work is based on a single occupation code, and a corresponding, but independently ascertained, industry code. It does not capture all the people who are covered by the new minimum wage regulations, with the principal omission being gardeners (mostly men). Fortunately, the survey contains a host of questions that may be used to identify covered workers. Respondents are first asked a series of yes/no questions, designed to pick up all forms of labor force participation, of which one is: "In the last seven days, did [name] do any work as a domestic worker for a wage, salary, or any payment in kind?" Later, employed respondents are asked their occupations: About 83% of those who report having performed domestic work are coded as "domestic helpers and cleaners" (code 9131, the basis of the StatsSA definition); the other relevant occupations include gardeners (16%, code 6113 in waves 4-6, but code 9211 in waves 7-10), nannies (0.5%, code 5131), and security guards (0.2%, code 5169). Next, a pair of questions is used to establish an industry code, of which domestic work is again one. Another question categorizes the types of employers, and includes the option: "Working for one or more private households as a domestic employee, gardener or security guard." Finally, there are questions about the location of the workplace, and about whether the work was formal or informal, the latter category being stipulated to include domestic work.[9] Note that the task is to identify those for whom domestic work is the *main* job, since the wage and hours data relate to the main job only.

These questions may be combined in various ways to define the set of workers covered by the new regulations, and the choice of definition matters for the measurement of employment trends. Given this problem, I present results based on a narrow and a broad definition of domestic employment. The narrow definition requires that *all* relevant questions be answered in a way that is consistent with domestic employment: in particular, the worker must (a) have performed domestic work in the past seven days or report that they have a job from which they are temporarily absent; (b) meet the StatsSA definition of employment, which allows for recent absence from work for some reasons but not others; (c) report working in a private household; (d) be categorized as working in the domestic industry; (e) report an occupation code of either 9131, 6113, 9211, or 5131 (code 5169, security guards, is disallowed because they are covered by a different sectoral determination); and (f) report a work location that is neither a formal business, shop, market, nor street corner. This (narrow) definition yielded between 2400 and 3100 cases per survey wave, and the corresponding population-weighted employment levels appear in Figure 1.

The broad definition effectively changes "and" to "or" in the above list: workers must *either* (a) report having done domestic work in the past seven days, and not report any non-domestic wage or salary employment; or (b) report working in a private home; or (c) be categorized as working in the domestic industry; or (d) be assigned occupation code 9131. In addition, as with the narrow definition, they must meet the Statistics South Africa definition of employment, not be coded as security guards, and report an appropriate business location. By this definition, there were between 2500 and 3200 full or part-time domestic workers in each wave of the survey, or between 1.09 and 1.24 million in population-weighted terms (upper line, Figure 1; these data also appear in Table 6, discussed below). Notice that the broad and narrow

[9] Despite this stipulation, I worried that the formal/informal distinction might have been variously interpreted, and chose not to use this question in defining the sample.

definitions differ by only about 34,000 in Year 0, but diverge noticeably thereafter. Given that the narrow definition probably undercounts domestic workers (since it disqualifies anyone who makes a mistake, or is miscoded, on any of the many relevant questions) while the broad definition probably overcounts, and yet they start in roughly the same place, it seems likely that the two definitions bracket the true employment trend.

Variable Definitions

The survey contains a detailed set of questions relating to labor force status, money earnings, and the terms of employment. However, no data are available on payments in kind, which can be of considerable importance to domestic workers. Given that the new regulations place lower limits on wages, and upper limits on the degree to which payments in kind may be substituted for wages, we might expect a reshuffling of the compensation package to occur. The inability to track this substitution is one limitation of this analysis.[10]

Hourly wages were calculated by dividing reported regular monthly earnings in one's main job by reported usual weekly hours of work, including overtime, multiplied by an assumed 4.35 weeks per month. Earnings data are sometimes available only as intervals (e.g. R501 to R1000); where this is the case, linear regressions of wages against a host of demographic, regional and occupational indicators were used to impute an earnings value, with the constraint that it must lie within the stated interval. Earnings are imputed in this fashion for roughly 8% of domestic workers. Once these are included, hourly wages are available for 98% of the sample. All earnings figures were converted to September 2004 prices using the urban CPI published by the South African Reserve Bank.

Minimum wages were set at four distinct levels, with higher hourly rates for part-time workers (those regularly working 27 or fewer hours per week), and for those working in "Area A" (defined by a list of 54 of South Africa's 262 newly demarcated municipalities, reproduced in the Appendix.)[11] The first round of minimum wages applied to the period 1 November 2002 through 31 October 2003, covering waves 7 and 8 of the LFS. The sectoral determination also

---

[10] A second weakness of the survey is that the household's non-labor income is not measured, and neither is consumption, so neither income- nor consumption-based estimates of poverty are possible, limiting our understanding of the distributional impact of employment and wage changes.

[11] The 262 municipalities cover the full territory of South Africa: they include the six largest cities (metros), 231 smaller cities and towns, and 25 deep rural district management areas that are extremely sparsely populated.

set wages for a second round, from 1 November 2003 through 31 October 2004, which spans waves 9 and 10; these wage levels are about 8% higher than the first year's. Note that *all* domestic workers, including those above the minimum, were entitled to an 8% wage increase on that date, and again in November of 2004. Using these rules, a minimum hourly wage was assigned to each employed domestic worker in each wave of the LFS survey, taking account of the reported usual hours of work, and including the entitlement to overtime pay, at time-and-a-half rates, for up to 35% of the sample who report working more than 45 hours per week.

The determination of whether workers were entitled to the higher Area A wage was limited in its precision by the lack of data on their employer's location. However, the worker's residential location was known, at least to the level of the magisterial district, 354 of which comprise the whole of South Africa. It was assumed that Area A wages applied if any portion of the magisterial district in which the worker resided was contained in any of the listed municipalities.[12] Municipalities and metro areas are defined fairly broadly, so nearby settlements are included: for example, workers living in Soweto, who likely work in Johannesburg, are coded as belonging in Area A, as are those living in Khayelitsha and working in Cape Town. Moreover, because of the imperfect overlap between magisterial districts and municipalities, an additional portion who do not live within the municipal boundaries, but who live in an overlapping magisterial district, are also coded as being entitled to Area A wages. This errs of the side of assuming that these workers are employed in nearby towns, rather than in their more rural residential areas, which seems appropriate. Workers living in still more remote areas and commuting longer distances to their urban employers may be incorrectly coded as not being entitled to Area A wages. For the geographic analysis, each worker was assigned a magisterial district, as well as one of the 262 new municipality codes, which can in turn be aggregated into the 53 district councils.

[12] This was accomplished using the master file that provides geographic attributes for all enumerator areas associated with the 2001 Census (ea_sa.dbf), including the magisterial district codes that allow one to link back to the 1996 census and to waves 4 through 9 of the LFS. Note that in wave 10, the actual municipality (of residence) is known. However, for consistency with the earlier waves, the determination of Area A eligibility was made by the same algorithm, relying on magisterial districts.

3) Basic Findings

Table 1 reports average real hourly wages for each wave, and their combined averages for Years 0,1, and 2; all percentage changes and significance tests are in relation to Year 0. We see large and significant wage increases for women under either sample definition, amounting to more than 20% over two years. Increases for men are smaller (7or 12%).[13] The addendum to the table reports the percentage changes in nominal wages, which total more than 35% over two years for men and women combined. Table 2 reports the proportion earning less than the applicable hourly minimum, with time-and-a-half for overtime factored in; this share falls significantly for both men and women under either sample definition. We see that in March 2004, when the higher, second year minima been in effect for just four full months, the proportion earning less than the minimum temporarily rose, before falling to its lowest level to date; this is consistent with a delayed response on the part of employers. Together, these results suggest that the minimum wage is having the expected effect on the bottom of the wage distribution, and that this effect is large enough to raise both the nominal and real average wage. Still, it bears emphasizing that, by these estimates, as of September of 2004 about 58% of employed domestic workers were still earning less than the regulations require.[14]

Figure 2 displays the estimated densities of real log hourly wages for men and women, using the broad definition. The dotted lines represent the Year 0 distributions; the dashes are Year 1; and the solid lines are Year 2. The distributions have clearly been shifting to the right, and they display increasingly prominent modes near the minima, indicated by the vertical lines (see notes at bottom of figure). In Year 1 there is evidence of a minor spill-over effect on wages above the minima; in Year 2 this spillover is more pronounced and extends all the way to about 2.5 log rand per hour (R12.20); this is consistent with the fact that in Year 2 the law entitled all domestic workers, not just those at the minimum, to a raise of at least 8%.

Table 3 shows that average hours of work per week have fallen by more than 5% for women (a statistically significant change), but by at most 2% for men (not significant). In the next table we see that there has been no increase in the proportion of domestic workers who

---

[13] Note that the men's Year 1 estimates are not statistically significantly higher than their Year 0 figures, and the same is true for Year 2 under the broad definition.

[14] It is possible that reported hours of work overstate actual hours of work, and certain that the assumption of 4.35 weeks of work per month is incorrect in many cases. Since this is the maximum possible, this assumption biases the hourly wage downwards.

report that they would like to work more hours, suggesting that these reductions in hours are by mutual agreement.

Table 5 shows that estimated average real monthly earnings increased only slightly (and insignificantly) in Year 1, but by Year 2 had risen to 15-16% above their Year 0 values, with larger increases for women (despite their reduced of hours of work). To summarize, for men and women under the broad definition, in the two years since the regulations went into effect we have seen a real wage increase of about 19%, combined with an hours reduction of about 5%, resulting in an increase in average earnings of about 15%. It is noteworthy that it took two years, and an increase in the minimum, before significant real earnings gains were seen for the average employed domestic worker. It should also be remembered that these are so far only descriptive results, with no assertion of a causal relationship to the minimum wage regulations, although such a relationship is of course entirely plausible.

Table 6 demonstrates that full and part-time employment of women has fallen by either 9.6 or 11.8% in two years, depending on the sample. For reasons that will remain largely mysterious, employment of men has *increased* significantly, by 14 to 15%. Combining men and women, the net employment drop stands at 5.5 or 7.6% over two years. As already noted, this result stands in contrast to the official figures published by StatsSA in the statistical releases that accompany the Labour Force Surveys (and their subsequent revisions), which show a much more gradual negative employment trend (see Figure 1), but which are based on an inconsistent series of sampling weights.

In the next table I divide the log changes in total employment by the log changes in the average real wage to yield a crude elasticity, i.e. one that is not adjusted for any other factors that might be influencing employment. These figures are generally positive for men, and vary between -1.55 and -0.28 for women, depending on the sample and the time period. For women it appears that the employment drop was relatively large in relation to the wage change in Year 1, with elasticities of -0.76 or -1.55, whereas in the second year the elasticities are much smaller, at -0.28 and -0.38. Over the two years, the cumulative elasticity for women stood at -0.64 under the narrow definition and -0.51 under the broad.

Table 8 documents the change in total hours worked, the combined effect of the changes in employment and in the length of the work week. For men, the growth in employment dominates, raising total hours by 12 or 13% over two years, a marginally statistically significant increase. For women, both changes are negative, and sum to -14 or -16%.

Table 9 displays the elasticities of total hours with respect to the wage. These are again mostly positive for men, given their increased wages and hours; for women, the two-year figures are either -0.79 (broad definition) or -0.91 (narrow definition), over two years. As with total employment, the elasticities were higher in Year 1 than in Year 2. The cumulative effects over two years are inelastic, implying that both men and women's total earnings have risen.

This is documented in Table 10; the increase in total earnings stands at 8 or 9% for men and women together, which attains statistical significance at the 10% level. This consisted of a 27-29% rise for men, and a 3.5 to 5% increase for women (the latter change was not significantly different from zero). Note that all of women's real gains came in the second year, whereas men made more steady progress.

Number and Status of Unemployed Domestic Workers

The surveys allow us to track the number and situation of those non-working individuals who report that domestic work was their last occupation. In Table 11 we see that this number has risen from a Year 0 average of 1.29 million men and women to a Year 2 figure of 1.40 million, a statistically significant increase of 117,000, or 9.1% over two years. Note that the rate of growth for men was higher, at 30%, despite their simultaneous increase in measured employment, implying an increase in their labor force participation rate, possibly in response to the higher wages. The figure 117,000 also exceeds my largest estimate of the two year loss in domestic employment (86,000, Table 6, narrow definition, men plus women). In Year 2, the majority of the 1.4 million unemployed domestics (70%) reported last working three or more years ago, and so cannot represent the recently disemployed. Among the 596,000 more recently unemployed (less than two years) only 1,600, or 0.27%, report receiving UIF benefits. This is an extremely low take-up rate, and it is not rising. It may reflect a general unawareness of the new benefit, as well as the fact that UIF registration rates still stood at under 30% among those employed in Year 2. It is also noteworthy that the average number of months since last work has not fallen, despite the presumed influx of recently-fired people.

Non-Wage Terms of Employment and Employment Relations

Figure 3 displays the trends in the various non-wage terms of employment that may be tracked using the LFS surveys. Two of these, namely, written contracts, and paid leave were required under the new regulations as of the first of September 2002. The share reporting having a written contract rose from about 8% to between 25 and 30%.[15] Paid leave was more prevalent to begin with, and saw a smaller increase: about 17-18% reported it in Year 0, rising to 21-22% in Year 2. UIF registration was required as of 30 April 2003; it rose from a Year 0 value of 3% to 26-27% in Year 2, at which point it appears to have leveled off.[16] Pension and health insurance benefits are not required of employers, yet pension benefits appear to have risen nonetheless. Health insurance contributions[17] remain at near-zero levels.

Some evidence of changes in labor market norms and institutional roles may be found in the trend in membership in domestic workers unions, the use of labor brokers, and in the use of third-party mediation by domestic workers with grievances. Union density rose from 1.0% in Year 0 to 2.6% in Year 2, an absolutely small but proportionally large change, which was significant at p=0.001. Counter to expectations, there has been no measured increase, and in fact a statistically significant decline, in the share of workers who reported being paid by a labor broker or cleaning agency (lowest line, solid circles)[18]. This figure fell from 1% in Year 0 to one-quarter of one percent in Year 2.

The statistics on mediated grievances come from the records of the Commission for Conciliation, Mediation, and Arbitration. From January of 2001 through August of 2002, the number of domestic worker cases referred this organization, about 80% of which relate to allegations of unfair dismissal, averaged 764 per month (Figure 4). From September 2002 to February 2005 the average stood at 1150, a 50% increase. This probably reflects both a genuine

---

[15] The documentation relating to this question in the September 2004 LFS includes the following comment: "The question is intended to find out if people involved in economic activities have written contracts with their employers. Most domestic workers have written contracts but they may not know it. The interviewers are instructed to probe and make the respondents understand that even a one-page written agreement regarding their work between themselves and the employers qualifies."

[16] UIF registration is not required for the 7% of the sample who report working fewer than 24 hours of work per month. The trend in the UIF rate excluding these few is indistinguishable from the above. The UIF Commissioner reports that 330,000 workers had been registered by 30 April 2003 out of an estimated 800,000, for a rate of 41% (SouthAfrica.Info 2003). This is higher than my estimate for September of 2003 (25%).

[17] The question reads: "Is the organisation/ business/ enterprise/ branch where [name] works providing for membership of, or contributions towards, membership of a medical aid fund or health insurance?"

[18] In the figure, "Paid by agency" means paid by a labour broker, a contractor or agency, or "other", as opposed to being paid by "The establishment / enterprise / individual for which he/she works."

increase in dismissals, fair and otherwise, and an increased awareness on the part of domestic workers as to their legal rights. That awareness has been fostered by education and outreach efforts on the part of the Commission, aimed at both employers and employees. The spike in April and May of 2003 coincides with the highly unpopular introduction of the UIF registration requirement, which appears to have generated considerable friction.[19] However, it is also reported that many informal "consultants" took advantage of domestic workers, offering to take their cases to the Commission for a small and modest fee, and generating many unnecessary referrals in the process.

These non-wage outcomes, while interesting in their own right, also help us with the econometrics to come. In particular, the detailed monthly accounting of referrals to the CCMA allows us to observe the time at which relations between employers and domestics begin to change, which in turn allows us to see whether employers acted in anticipation of the regulations or not. I see no upward trend in complaints prior to September of 2002, the month the non-wage requirements became effective, which suggests that Year 0 (the September of 2001 and February of 2002 surveys) is in principle a valid baseline. In September of 2002, complaints reached an 18-month high of 954. While this might suggest that data from the September 2002 survey should be included in the "after" period, I deemed it safer to drop this survey wave, since some of the regulatory provisions were in effect at that time, but others (namely, minimum wages) were still pending.[20]

Furthermore, like the minimum wage itself, the requirements of UIF enrollment, written contracts, and paid leave also impose costs on employers, and the question arises whether the employment declines we have seen are causally related to these various additional costs, and whether the impact of the wage and non-wage costs can be separately identified.

---

[19] Initial media reports emphasized the inconvenience of registration, and of a requirement that the employers' UIF withholdings be forwarded to the Fund monthly, even though they amount to very small sums of money. Some of the bureaucratic problems have since been fixed.

[20] This decision is not without consequence: September 2002 was a particularly bad month for wages, monthly earnings, and employment, as may be seen from Tables 1, 5, and 6. Classifying it either before or after makes a real difference to some observed trends.

4) Econometric Evidence

Many studies of minimum wages examine the effects of a relatively long series of small changes in nationwide or statewide minima that apply to all, or most, occupations and employers. In the present case, we have a short time series during which an occupation-specific minimum is introduced *de novo*, set above the median wage for that occupation, and then raised once, two surveys later. Despite the short period of observation, the size of the initial intervention suggests that any effects on wages, hours, and employment should be readily identifiable, and the descriptive data certainly seem to bear this out, more clearly for women than men. One way to test for a causal link between the regulations and the employment change is to compare the evolution of employment for domestic workers to that of a control group: but which one? Higher-wage domestic workers are not directly affected by the introduction of the Year 1 minimum, but appear, from the evidence in Figure 2, to have been affected by the across-the-board wage hikes required in Year 2. Other so-called unskilled occupations are also not directly affected, but this comparison yields meaningful results only if their observed employment trend is in fact a good proxy for what would have happened to domestic workers absent the regulatory intervention. But why should we believe that the employment trends of, say, farm workers, truck drivers, retail trade workers, or miners, whose industries are subject to countless idiosyncratic influences (including their own sector-specific minimum wages) represent the right counterfactual?

The approach I adopt resembles one of Card and Krueger's cross-state analyses (1995, pp. 127-137) insofar as it focuses on regional variation in the impact of the minimum wage; the definition of region is discussed below. This variation is both absolute (the Area A minima are higher than elsewhere) and relative (the share who fall below any given minimum, or the relation between the minimum and the average wage, varies by region). The first step is to ask whether regions in which the initial share earning less than the minimum was largest also saw the largest wage increases.[21] If so, we treat this as evidence that the regulations raised wages as intended. Once this is established, we ask whether these regions also saw the largest decreases in either

---

[21] The fraction below the minimum is analogous to Card and Krueger's (1995) "fraction affected" since this is defined as the share who fall between the old minimum (in this case, 0) and the new. Brown (1999, p. 2130) notes that these measures of the degree of impact of minimum wage changes are "conceptually cleaner" than relative wage measures such as the Kaitz index.

hours per week or the domestic worker employment-to-population ratio,[22] and if the answer is yes we have evidence of a negative effect of the minimum wage on hours or jobs. Analogously, we may ask whether regions in which the shares reporting UIF withholdings, written contracts, and paid leave are lowest (in other words, the regions where non-compliance with the pending regulations was highest) were also the regions in which employment fell fastest (or grew most slowly) once the regulations went into effect. If so, we have indirect evidence that the cost of complying with the non-wage provisions has led some employers to lay off their domestic workers. Finally, we may include both the wage and non-wage factors in the same equation to see if separate effects can be identified.

In the wage equations reported in Tables 11 and 12, the primary explanatory variable is the fraction in a given region who, in Year 0, are estimated to fall below the hourly minimum wage that becomes law in Year 1. The outcome to be explained is the change in mean log wages, by region. The entire analysis is stratified by gender, primarily because the observed employment changes for men and women were of opposite sign. The models are then run for Year 0 versus Year 2, to estimate a cumulative effect, and a third set compare Years 1 and 2, to see if the increase in the minimum made any difference.[23] A significant positive coefficient for the share below the minimum is expected. The identifying assumption is that there are no *other* reasons that low-wage regions should experience faster domestic worker wage growth than high-wage regions. To make this assumption more palatable, two further variables are included as general measures of changes in labor market conditions that might be correlated, whether causally or spuriously, with the realized sample's regional shares below the minimum. These are the rate of growth of real hourly wages of similar workers in other occupations, and the change in their employment-to-population ratio.[24] Both of these are assumed to measure the growth of

---

[22] The log change in the level of domestic employment is an alternative to the employment-to-population ratio, but the latter is preferred for two reasons: it controls for actual changes in population over time, which are presumably not dictated by the minimum wage, and it reduces the effects of sampling error if the sample's employment-to-population ratios are less variable than its employment levels, as seems reasonable.

[23] For the Year 1/Year 2 model, the share below the minimum in Year 1 is calculated in relation to the higher minimum that came into effect in Year 2; in other words, it quantifies the fraction to be affected by the Year 2 minimum. This is a cosmetic change, as the two measures are correlated at 0.93 to 0.98. (Note that this departs from Card and Krueger, who define the faction affected as the share falling *between* the old and new minima. But this is in order to allow for non-covered workers in the U.S. setting, and does not apply here.)

[24] The "similar" workers are employed African and Coloured men and women between the ages of 15 and 65 with less than a high school education who were not employed as domestic workers under either definition.

overall labor demand for similar people, and are expected to display positive coefficients in the wage equation.

A problem that immediately arises is the fact that a region might have a low measured average hourly wage because of an unusually large number of negative random errors in the measurement of individual earnings (or positive errors in the measurement of hours). If these errors are independent over time and have an expected value of zero, then in the next period that seemingly low-wage region should no longer display a net negative measurement error, creating the impression that its wages are rising faster than those of other regions, and generating an upwardly biased estimate of the effect of the minimum wage on the average wage (see Appendix 1). This problem may be acute if samples or regions are small, so that the average measurement error across workers need not be near zero. The solution is to create regions that are large enough to reduce this bias to acceptable levels. However, as we group a given number of individual observations into fewer, larger regions, we reduce the sample size of the regression just described, and also reduce the variance of the explanatory variable, both of which reduce the precision of our estimates.

A second potential source of bias lies in the possibility that sub-minimum wage employers in high-wage regions might react differently when the minimum wage is introduced than do employers in low-wage regions. Suppose it were the case that sub-minimum employers who live in urban areas are more likely to raise wages to comply with the new regulations than those in lower-wage regions, either because of differences in social norms, or in the level of awareness of their workers, or in the level of enforcement, or in the perceived difficulty of "finding good help these days." Put differently, suppose non-compliant employers in lower-wage rural areas are more likely to thumb their noses at the Minister of Labour. Such behavior would tend to bias the coefficient on the share below the minimum downwards, since it leads to more widespread increases in compliance, and hence in average wages, in regions with *lower* initial shares below the minimum, offsetting the expected positive association. This offset is undesirable, because the actions of the more compliant employers *are* still attributable to the minimum wage.

One way to test this hypothesis is to use the initial shares reporting UIF registration, written contracts, paid leave, and pensions, none of which were required prior to the regulations, and all of which are positively correlated with regional wage levels, as predictors of this heterogeneity in employer behavior. We then ask: (a) whether any of these variables display a significant positive association with subsequent wage increases, conditional on the share below the minimum; and (b) whether their inclusion serves to raise the coefficient on the share below the minimum. If both answers are yes, I would argue that the non-wage variables belong in the equation.[25]

In the second step, the share of the region's workers who fall below the minimum is used to explain the subsequent change in the average weekly hours worked by domestics, and in the ratio of the number of African and Coloured women (or men) who are domestic workers to the overall number of adult (15 or older) women (or men) in the African and Coloured population. Together the two steps amount to an instrumental variables approach, where the share below the minimum serves as an instrument for the log wage change. The rationale for adopting this approach is that if we were instead simply to regress employment changes against wage changes, we would run the risk of observing a positive correlation that is driven by omitted factors – a classic case of the supply/demand identification problem. For example, an increase in middle-class incomes in a given region could lead to an increase in demand for domestic help, and a demand-driven increase in their wage. This would tend to obscure any negative employment effects of the minimum wage. Using the initial share below the minimum as an instrument will solve this problem, provided there is no correlation between that share and factors other than the wage that affect domestic employment, but are omitted from the analysis (factors such as subsequent changes in middle-class incomes, for example). Otherwise put, the identifying assumption in the employment equation is that, absent the regulatory intervention in the wage, low-wage and high-wage regions would experience the same absolute changes in the domestic worker employment-to-population ratio. Here we may again include our two measures of changes in the non-domestic labor market as controls.

In the employment regression, unlike the wage equation, we need not worry about mean-reverting measurement errors in wages causing an upward bias; instead, we should be concerned about attenuation bias if the share who fall below the minimum is poorly measured and our regions are small. To explore this issue, I considered three levels of regional aggregation. The

[25] Note that if the non-wage measures have no independent impact on the wage, and are merely providing some additional information about the (poorly measured) share below the minimum, then they should enter positively in the equation but their inclusion should *reduce* the coefficient on the share below the minimum.

finest is at the level of the 353 magisterial districts; of these, roughly 300 contain at least one female domestic worker in each time period, but only 220 or so have any men. The next level is that of the 262 municipalities, roughly 200 of which are usable (150 for men), and the most aggregated is at the level of the 53 district councils.

To test the robustness of the results, I estimate a host of plausible specifications for men and women, at each of the three levels of aggregation, and each of the three time period comparisons. These include: using least squares (OLS) versus least absolute deviations (LAD), in order to explore the influence of outliers; including or excluding regional weights, which measure the number of domestic workers at the start of the period;[26] using both the broad and the narrow definitions, which display different crude wage-employment elasticities; and including or excluding the four non-wage variables and the two general labor market controls. All told, I report summary results for 432 equations for each outcome (wages, hours, employment).

The OLS employment equations may also be recast explicitly as instrumental variables estimates, with the share below the minimum serving as an instrument for the log wage change. In this framework, we may then allow the non-wage variables to enter as additional instruments for the log wage change. This would be justified if we find evidence that the non-wage variables do indeed have significant independent effects on the wage (as hypothesized), and if they appear to affect employment only through their effect on the wage.

[26] If large and small regions display similar wage/employment relationships, the weights should not matter in large samples, but in smaller samples, such as these, the weighted and unweighted results may diverge considerably. Moreover, if large and small regions respond *differently*, the weights will matter. Unfortunately, in this case, *neither* the weighted nor the unweighted results are consistent estimators of the population-weighted average of the response coefficients for different-sized regions (see, for example, Deaton 1997). I treat them all as plausible, if imperfect, estimators, and assess robustness by comparing their results. Note that the weights should reduce the effects of measurement error by down-weighting the smaller regions, in which noise is likely to be larger in relation to signal. A final problem is that employment/population ratios should be weighted by total population, while wages and hours should be weighted by the number of domestic workers; neither set of weights will preserve the sample means of *both* the dependent and the independent variables once the data are broken into regions.

Results: Wages

Table 12 reports the results of 18 representative wage regressions, run at the district council level in hopes of minimizing measurement error bias. The equations are estimated via ordinary least squares in the narrowly defined sample; results under the full set of specifications are summarized in the following table. In the first column of the upper panel (women), the coefficient for the share below the minimum is 0.445 and clearly significant, implying that if we compare two districts whose initial shares below the minimum differ by 0.10, we would expect to see a difference in their subsequent wage growth rates of 4.45 percentage points; for men (lower panel) the effect appears stronger, at 8.56 points, although it is estimated with less precision.[27]

The next column adds the controls for the rate of growth of real hourly wages for similar workers in other occupations, and the change in their employment-to-population ratios. The wage variable's coefficient is positive in all 12 equations in which it appears (as expected) but significant at the 10% level in just two (men, last two columns). The employment variable enters with insignificant positive and negative signs for women; it is positive and significant in three of the six equations for men. The estimated effect of the share below the minimum is generally robust to the inclusion of these controls, for both men and women.

The next columns add the four non-wage variables. For women, these raise the estimated effect of the minimum wage (by 0.10 or more), and also add between 0.09 and 0.16 to the values of R^2. The share reporting UIF deductions has a significant positive effect in two equations, the shares with contracts and pensions each contribute one positive significant term, and there are no significant negative terms, lending some support to the hypothesis of heterogeneity in the employers' responses. For men, however, the non-wage variables make little difference.

Table 13 summarizes the results of the 432 different specifications, and several points emerge. First, for women, the response of the average wage to the minimum is weaker in the Year 1/Year 2 comparisons than in the Year 0/Year 1 equations; for men, however, the coefficients appear, if anything, to rise over time. Second, for women the coefficients increase as we move to finer levels of disaggregation, which is consistent with, but not diagnostic of, an increasing effect of mean-reverting measurement error bias. For men, this pattern is less clear. Third, for both men and women the weighted estimates are generally smaller than the

[27] Recall that women outnumber men in the domestic worker sample by about 5:1.

unweighted figures. This may be because the weights reduce the influence of the smaller regions, for which average measurement errors are likely to be largest, but it may also be the case that smaller regions display a different wage response than larger ones.

The final columns categorize the parameter estimates for the share below the minimum according to sign and statistical significance at the 10% level. Overall, 398 out of 432 equations yield a significant coefficient of the expected sign, and just four yielded negative estimates, of which none was significant. This leaves little room for doubt that minimum wages raised average wages from Year 0 to Year 1, and also from Year 0 to Year 2. For men, the effect from Year 1 to Year 2 is equally strong, while for women the effect becomes weaker. I would argue, however, that there is still evidence of an effect for women from Year 1 to Year 2, and that the average coefficient at the district council level (0.11) is a reasonable estimate of its magnitude. Although the coefficient was significant in only 9 of the 24 equations it may be that with just 53 observations we lack the power reliably to detect an effect of this size. The results from the more disaggregated datasets, although upwardly biased to an unknown degree, also support the claim that minimum wages mattered in Year 2.

Table 14 summarizes the results for the other explanatory variables that appear in the wage equation. The change in the non-domestic wage displays a modest effect on domestic wages for women, having a positive and significant coefficient in 48 of 144 equations, with an average cross-elasticity of wages of 0.11. For men the effect is weaker, with an average coefficient of just 0.03. By contrast, the non-domestic employment variable accomplishes little for women, but has a generally positive effect on male employment. Finally, the four non-wage terms of employment have a generally positive effect for women, but display net negative effects in three of four cases for men. These variables may be of some limited use as additional instruments for the change in wages in an employment equation. The F-test of their joint effect is significant in just 26 of the 72 cases for women, and 25 of 72 for men, but this includes cases in which some of the individual terms are significant but with the wrong (i.e. negative) sign. Moreover, just 17 of these 51 significant estimates came from the 72 OLS versions of the equation, which we will later want to extend to two-stage least squares estimates. This implies that adding these instruments might not substantially alter our estimates.

Results: Hours

Table 15 reports a representative subset of regressions for hours per week, this time at the magisterial district level; Table 16 summarizes the full set of 432. These unambiguously support the proposition that minimum wages have reduced hours worked, for both men and women, with 340 negative and significant estimates, another 91 negative but insignificant results, and just one positive estimate, which did not attain statistical significance at the 10% level. Taking the overall average estimates for men and for women from Table 16 we may infer that a low-wage district whose sub-minimum share was 0.1 above the national mean would see a reduction in hours worked of about three-quarters of an hour for women, and 0.9 hours for men, on top of the change experienced by the average district.

In Table 17, the change in the wage of similar workers in other occupations appears to exert a negative effect on the length of the domestic work week (the effect is significant in 74 of 288 equations) while the employment control variable yields estimates that are all over the map. Most of the non-wage variables, which I have interpreted as measures of employer generosity of sorts, display predominantly negative effects on hours of work. This could mean that more conscientious employers are moving more quickly to conform to the 45 hour maximum regular work week. Or it may be that the ability of these variables to add information about changes in log wages, document above, also enables them to predict the consequent changes in hours.

Results: Jobs

Table 18 reports a representative set of employment regressions, again using unweighted OLS at the magisterial district level. The first column in each block uses the share below the minimum as the sole explanatory variable of the change in the domestic service employment-to-population ratio; this term ought to enter negatively, as it predicts a rise in wages. The second column adds changes in both non-domestic wages and the non-domestic employment-to-population ratio. The non-domestic wage could display a negative sign if higher alternative wages serve to raise the (imperfectly measured) domestic wage, and so reduce employment; or they could display a positive sign if they signal an increase in general labor demand, including demand for domestic help.[28] The expected sign of the non-domestic employment variable is also not obvious: a rise in alternative employment might tempt domestics away from their jobs (again subject to the footnoted proviso), or it might also signal a general increase in income, and hence in demand for domestic work (a more plausible argument in a labor surplus economy).

The third column in each block presents the full model, with added controls for the non-wage requirements, namely, the initial shares reporting UIF registration, written contracts, and paid leave. If indeed these variables have significant independent positive effects on wages, then they ought also to have independent negative effects on employment. On the other hand, they could have *positive* signs if compliance with these requirements is costly (and enforcement is serious), since we would expect the smallest employment declines (or largest increases) in regions where *ex ante* rates of compliance were highest. The pension variable should enter negatively for the first reason, and is not subject to the second argument since it is not a required term of employment.

No significant effect of the minimum wage is found in the comparison of Year 0 and Year 1, in any of the six specifications shown. However, the cumulative effect from Year 0 to Year 2 (middle three columns) is negative and significant at the 10% level or better in all three of the women's equations, but much smaller, and insignificant, for the men. For women, this suggests that there was a lag in wage and employment adjustments on the part of employers. The Year 1/Year 2 parameters (final three columns) are significant for both men and women, and are twice as large for women as men. The significant Year 2 results for men are remarkable: they imply that their modest employment gains in Year 2 occurred *despite* the negative effects of the minimum wage. The descriptive results appear to be confounded by the operation of other forces that were working to raise male domestic worker employment even as the minimum wage worked to reduce it. Some of these forces are captured by the other covariates, but these had only modest effects for men; the main factors responsible for men's job growth remain buried in the intercepts, which are uniformly positive and significant.

The breakdown of the 432 estimates appears in Table 19. All told, significant negative minimum wage effects outnumber significant positive effects by 121 to 3; of the remaining 308

---

[28] The link between domestic and non-domestic wages may be weak given South Africa's extraordinarily high rates of open unemployment, which stood at 36% for African women in September of 2004, by the official definition, and at 56% by the expanded definition (which counts the non-searching, discouraged unemployed as economically active).

results, 196 are negative and 112 positive. There is no clear evidence of an effect in Year 1 over Year 0 for women, but 10 of 72 equations for men yield significant negative coefficients in that period. For Year 2 versus either Year 0 or Year 1, between 1/3rd and not quite half of the wage coefficients are negative and significant for men and women.[29] The average cumulative two-year effect for women was -0.017, compared to -0.013 for men. For women, whose employment fell, the coefficient means that an increase (across regions) in the share below the minimum of 0.1 corresponds to an additional reduction in the employment to population ratio of 0.0017. Given that the observed regional average change in the employment ratio was on the order of -0.0100, this would add entail a 17% greater reduction in employment in the lower-wage region than in its higher-wage neighbor. For men, the mean region saw a *gain* of about 0.0250 in the employment ratio. A region that was 0.1 above the average in its sub-minimum share, however, would be predicted to see a gain of just 0.0237, or a 5% smaller gain. Overall, the weighted and unweighted specifications produced comparable results; the male results are somewhat more one-sided than the female (despite their observed employment gains); and the OLS and LAD results are comparable, on average, but may differ considerably on a case by case basis (not shown in table.)

Table 20 summarizes the results for the other explanatory variables. For women, the change in the average wage for non-domestic workers enters negatively and significantly in 46 out of 144 equations, and is positive and significant in just two cases. For men, however, this variable leans towards the positive. The non-domestic employment control variable's estimates are positive and significant in 36 of 144 case for women, with just two significant negative results. For men, with their smaller sample sizes, these estimates are again less stable across specifications.

The three non-wage requirement variables (UIF, Contract, Paid Leave), generate more negative than positive estimates. Recall that a negative effect would be expected if these variables have an independent positive effect on wages, while a positive coefficient would be consistent with the proposition that increasing compliance is costly, so that lower rates of *ex-ante* compliance (which produce higher increases in compliance) lead to lower rates of subsequent

[29] An earlier version of this paper (Hertz, 2004), which was presented at the 2004 DPRU/TIPS conference, and which received some attention in the media, was based on data from Year 0 and Year 1 only, (the Year 2 data not yet being available). Using methods similar but inferior to the above, and pooling men and women, it found no evidence of a disemployment effect, which is consistent with the present finding for that time period.

employment growth. The fact that these figures tend to be negative thus provides little evidence for a disemployment effect stemming from the non-wage provisions *per se*, and does lend some support to their use as instruments for the wage change. The possible exceptions are the generally positive effects of UIF and paid leave for men.

The instrumental variables estimates appear in the final set of tables, 21a for women and 21b for men. The first column uses the share below the minimum as the sole instrument for the change in log wages, with no other covariates; the second column adds the labor market controls; the third adds the four non-wage variables; and the fourth uses the non-wage variables as additional instruments for the wage, as discussed above. The first three equations do not tell us anything more than we already knew, although they facilitate the calculation of the elasticity of the employment-to-population ratio with respect to the average (not the minimum) wage. We repeat the finding of no significant effect of the minimum wage in the Year 0 / Year 1 comparison, and note that adding the new instruments does not alter this conclusion. In the Year 0 / Year 2 columns, where the wage effect is negative and significant, the addition of the four new instruments appears to reduce the absolute value of the wage coefficient somewhat (and its corresponding elasticity), as it does in the Year 1 / Year 2 comparisons. Note that Hansen's test of the validity of the instruments raises no objections in any of these six specifications, although it did reject some of the models not shown.

Elasticities are calculated by dividing the wage coefficient by the mean level of the domestic worker employment-to-population ratio across regions, reported in the third line from the bottom.[30] These are summarized in Table 22 by averaging across 16 specifications (the four models from the table above, with and without weights, broad and narrow definitions) for each row. For women, the results are positive (but not significantly so) in Year 1, but then became roughly unit elastic in Year 2; their cumulative employment elasticity with respect to the average wage, from Year 0 to Year 2 (figures in bold), had an average value of -0.42, and a median result of -0.46. For men, the results are more consistent across time, but less consistent across specifications, with an average cumulative effect of -0.48, and a median of -0.33.

---

[30] The predicted change in the employment-to-population ratio given a 1% change in the wage is 0.01 times the coefficient associated with the difference in log wages. Divide this by the mean employment ratio and multiply by 100 to express the result as a percentage to yield the elasticity. For the mean employment ratio I used the arithmetic mean across regions of the logarithmic mean of the domestic employment-to-population ratios in the two periods, where the log mean is defined by $(e_2 - e_1)/\log(e_2/e_1)$.

Figure 5 shows a pair of a representative employment equations. The left figure plots the results of the unweighted OLS regression reported in the last column of Table 18, upper panel (slope -0.043), with the 297 magisterial districts as the units of analysis. The right is the weighted LAD equivalent (slope 0.005; not significant; regression not reported in Table), with the size of the circles indicating the number of domestic workers in the district. The data are the residuals after removing the predicted effects of non-domestic wages and employment, and of the four non-wage variables. The sensitivity of the slope estimates to the choice of estimator, and the considerable degree of unexplained cross-sectional variation, are both clear. It was the contemplation of one too many graphs like this that drove me to run so many different versions of each regression. As it happened, the specification-hunting was done with only the women's data at hand, and the resulting package of models was then applied in its entirety to the male sample.

5) Concluding Remarks

Domestic service workers were, and remain, the lowest paid broad category of labor in South Africa, earning just over half the hourly wage of the next group up, the farmworkers. Raising their pay, and reforming their relationships with their employers, have long been goals of the ANC-led government, and the new regulations represent a major step in this direction. The results just discussed make it plain that, despite substantial apparent non-compliance with both the wage and non-wage provisions, average hourly wages, average monthly earnings, and the *total* monthly earnings of employed domestics are indeed rising. Progress is also being made in UIF registration (although not, apparently, in the payment of UIF benefits to unemployed domestic workers, at least, not yet); in the provision of written contracts; and, to a lesser extent, in the provision of paid leave; and even pensions, which are not mandated. It also appears that the regulations have reduced the length of the work week somewhat, without provoking any outcry.

The bad news is that there is also evidence of the microeconomically anticipatable loss in employment, for both women (whose observed employment levels fell) and for men (whose employment rose, despite the negative effect of the minimum wage). The mere fact of a disemployment effect, however, does not condemn the policy. As I argued in an earlier work (Hertz 2002), what matters are the relative numbers and sizes of the income gains and losses, and their location within the broader distribution of household income.[31] Are the disemployed domestics by and large falling below the poverty or ultrapoverty lines? Are those who got a raise climbing out of poverty? In that earlier paper, I tried to answer the question prospectively, by simulating the distributional impact of the soon-to-be-implemented minimum wage, under a series of simplifying assumptions, some of which have since been shown to be inappropriate, namely, full compliance with the law, no spillover to wages above the minimum, and no reduction in hours of work for those who remain employed. Using the 1993 PSLSD data, the last national survey for which income-based household poverty estimates are readily calculated, I lifted all sub-minimum wage domestic workers to the minimum, and then fired varying numbers

[31] A South African friend with deep roots in the anti-apartheid struggle has remarked that empirical cost-benefit analyses of this kind, no matter how distributionally sensitive, fail to address the profound moral argument for higher wages and better working conditions, a point which is well taken. I have focussed on trying to provide decent empirical information on the question, in hopes that it will inform the policy debate, of which the moral arguments are likewise an important part.

of them, with a firing propensity that was higher for those who had received larger raises. I then recalculated their household incomes, holding all other income components constant, and generated new national estimates of poverty, ultrapoverty, and of mean log household income, a highly progressive income measure.

Last, I plotted the results from many runs (varying the number of people fired and the random seed that governs the firing process), with the poverty measures on the vertical axis and the realized elasticity of domestic worker employment to their average wage on the horizontal. For each poverty or welfare measure this allowed me to locate a critical elasticity, namely, the elasticity which generated no change in the outcome. I found that any elasticity less than about one in absolute value was low enough to allow the poverty and ultrapoverty rates to improve, despite the loss of jobs. At higher elasticities, the job losses outweighed the income gains, and poverty or ultrapoverty rose.[32] For gains in mean log welfare, the elasticity must smaller (less negative) than about -0.60.

With a moderate leap of faith, across ten years and all the limitations of both the simulation method and the econometrics just witnessed, we may compare these critical values to the averages and medians of our estimated cumulative two-year employment elasticities in Table 22. These range between -0.33 and -0.48, and would thus appear to be low enough for the policy to have been poverty-and ultrapoverty-alleviating, by a fairly comfortable margin.[33] As for mean log welfare, whose critical value is -0.60, the figures appear to fall below this line as well, but the margin is much smaller. The point estimates suggest that, by this progressive welfare measure, the minimum wage has been beneficial, but the confidence interval likely includes negative outcomes.

If we also take into account the reduction in hours worked, the relevant elasticities are higher. Using the results from Tables 13 and 16, we may derive an estimated elasticity of hours per week to the real wage of -0.47 for women and -0.28 for men.[34] Adding these elasticities to the (average) employment elastiticies yields a wage elasticity of total hours of -0.99 for women

---

[32] I can think of no good reason that unity should be the critical value: that fact is a finding of the quasi-experimental approach, based on the data at hand, not a theoretical given.

[33] I have not yet been able to provide standard errors for these aggregate estimates, as each is an average over a dozen models whose outputs cannot be treated as independent.

[34] I take the average effect of the share below the minimum on hours per week, across all specifications that compare Year 0 to Year 2, expressed as a percentage change in hours, and divide this by the average percentage effect on the wage (at the district council level, to avoid the upward bias at finer levels of disaggregation). For women the figures are (-6.89 / 41.5) / 0.356 = -0.47. For men: (-8.62 / 39.6) / 0.771 = -0.28.

and -0.76 for men. These figures imply that the minimum wage has left the total real monthly earnings of employed women unchanged, while slightly the total earnings for men. Comparing these figures to the critical values described above, we can conclude that, for women, neither poverty nor ultrapoverty have changed by very much as a result of the minimum wage, while for men they should have improved somewhat.

Once we take account of hours of work, both the male and female figures are higher than the critical value for mean log welfare (-0.60), suggesting that the negative welfare effects of the job losses may have outweighed the welfare gains associated with rising monthly earnings. However, it could also be argued that the poverty and welfare calculations should *not* make any adjustment for the reduction in the length of the work week, since this is immaterial to those who lost their jobs, and, according to the evidence in Table 4, unobjectionable to those who are still employed. If this latter argument is accepted, we may again conclude that the minimum wage policy has been beneficial, but must admit that this conclusion is tentative, given that the critical elasticity values were derived from outdated survey data, and that the simulations do not exactly mimic the patterns of change in wages and employment that we have witnessed since the minimum was introduced.[35]

Finally, I should again remind the reader of the sensitivity of the descriptive results, and, to a lesser extent, the econometrics, to the choice of weights. I have calibrated the weights as best I can, but Statistics South Africa's revision may produce a better final product, and this could alter my findings.

[35] It should also be noted that the elasticity for women appears to be getting more negative as time goes by. The two year average was -0.42, but in the second year the figure was -1.04. (This is the opposite of what we found using the crude elasticities in Table 6, where the more recent figures were the smaller ones.) If this is a sign of things to come, it would mean larger employment losses as the minimum is raised and enforcement is stepped up. But it is even harder to predict future elasticities than to estimate past ones.

References

Ashenfelter, Orley, Angus Deaton and Gary Solon (1986). "Collecting Panel Data in Developing Countries: Does it Make Sense?". LSMS Working Paper No. 23, World Bank, January.

Brown, Charles (1999). "Minimum Wages, Employment and the Distribution of Income" in Handbook of Labor Economics, Vol. 3B, David Card and Orley Ashenfelter, eds. Amsterdam: Elsevier.

Card, David, and Alan B. Krueger (1995). Myth and Measurement: The New Economics of the Minimum Wage, Princeton: Princeton University Press.

Deaton, Angus (1997). *The Analysis of Household Surveys : A Microeconomic Approach to Development Policy*. Washington: World Bank.

Department of Labour, Republic of South Africa (2002a). "Basic Conditions of Employment Act (75/1997): Sectoral Determination 7: Domestic Worker Sector, South Africa." Government Notice No. R. 1068, 15 August.

Department of Labour, Republic of South Africa (2002b). "Basic Conditions of Employment Act (75/1997): Sectoral Determination 8: Farm Worker Sector, South Africa." Government Notice No. R. 1499, 2 December.

Hertz, Tom (2002). "Forecasting the Effects of Pending Minimum Wage Legislation On Poverty in South Africa." Unpublished, April. Available from author.

Hertz, Tom (2004) "Have Minimum Wages Benefited South Africa's Domestic Service Workers." Unpublished, October. Available from author.

SouthAfrica.Info (2003) "UIF for domestic workers" 7 May 2003 http://www.southafrica.info/public_services/citizens/services_gov/uif-domestic.htm

Statistics South Africa (various years): Labour Force Surveys, Waves 4 (September 2001) through 10 (September 2004). Public use microdata, available for purchase via http://www.statssa.gov.za/

Statistics South Africa (2004). "Mid-year population estimates, South Africa 2004." Statistics South Africa , Statistical Release P0302, Pretoria, 27 July.

Statistics South Africa (2005). "Mortality and causes of death in South Africa, 1997–2003: Findings from death notification." Statistics South Africa , Statistical Release P0309.34, Pretoria, 18 February.

Table 1
Average Real Hourly Wages
(Rand, at Sept. 2004 Prices)

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	4.05	3.80	3.84	4.44	3.81	3.90
Feb. 2002	Year 0	3.79	3.54	3.58	3.82	3.54	3.59
Sept. 2002	(Omit)	3.74	3.43	3.49	3.67	3.44	3.48
Mar. 2003	Year 1	3.91	3.80	3.83	3.95	3.79	3.83
Sept. 2003	Year 1	4.30	3.95	4.01	4.23	4.07	4.10
Mar. 2004	Year 2	4.09	4.18	4.17	4.11	4.18	4.16
Sept. 2004	Year 2	4.65	4.77	4.74	4.59	4.81	4.76
Averages (weighted by employment)	Year 0	3.90	3.67	3.70	4.09	3.67	3.74
	Year 1	4.09	3.88	3.92	4.07	3.93	3.96
	Year 2	4.38	4.46	4.44	4.36	4.47	4.45
Pcnt. change from Year 0	Year 1	4.9%	5.7%	5.9%	-0.5%	7.1%	5.9%
	Year 2	12.3%	21.5%	20.0%	6.6%	21.8%	19.0%
Significance of changes (p-value)	Year 1	0.375	0.027	0.015	0.938	0.008	0.016
	Year 2	0.026	0.000	0.000	0.241	0.000	0.000

Addendum: Change in Average Nominal Hourly Wages

Pcnt. change from Year 0	Year 1	18.5%	19.7%	19.5%	12.3%	20.9%	19.6%
	Year 2	28.2%	39.2%	36.8%	21.6%	39.1%	35.9%
Significance of changes (p-value)	Year 1	0.002	0.000	0.000	0.034	0.000	0.000
	Year 2	0.000	0.000	0.000	0.000	0.000	0.000

Table 2
Proportion Earning Less than the Applicable Minimum Hourly Wage

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	0.71	0.76	0.75	0.70	0.76	0.75
Feb. 2002	Year 0	0.71	0.76	0.75	0.70	0.76	0.75
Sept. 2002	(Omit)	0.72	0.75	0.75	0.72	0.75	0.75
Mar. 2003	Year 1	0.65	0.70	0.69	0.65	0.70	0.69
Sept. 2003	Year 1	0.60	0.64	0.63	0.60	0.63	0.63
Mar. 2004	Year 2	0.64	0.67	0.66	0.63	0.67	0.66
Sept. 2004	Year 2	0.56	0.59	0.58	0.57	0.59	0.58
Averages (weighted by employment)	Year 0	0.71	0.76	0.75	0.70	0.76	0.75
	Year 1	0.62	0.67	0.66	0.63	0.67	0.66
	Year 2	0.60	0.63	0.63	0.60	0.63	0.63
Pcnt. change from Year 0	Year 1	-12.0%	-12.1%	-12.3%	-10.1%	-12.1%	-11.9%
	Year 2	-16.0%	-16.6%	-16.7%	-14.9%	-16.7%	-16.6%
Significance of changes (p-value)	Year 1	0.004	0.000	0.000	0.009	0.000	0.000
	Year 2	0.001	0.000	0.000	0.002	0.000	0.000

Table 3
Average Number of Hours Worked Per Week

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	40.2	42.5	42.1	40.6	42.6	42.3
Feb. 2002	Year 0	39.6	43.5	42.8	39.8	43.5	42.8
Sept. 2002	(Omit)	40.4	42.2	41.9	40.8	42.2	41.9
Mar. 2003	Year 1	39.3	41.7	41.2	39.6	41.6	41.2
Sept. 2003	Year 1	38.8	41.0	40.6	38.8	41.0	40.6
Mar. 2004	Year 2	40.3	41.5	41.3	40.6	41.5	41.3
Sept. 2004	Year 2	38.0	39.9	39.5	38.6	39.8	39.5
Averages (weighted by employment)	Year 0	39.9	43.0	42.5	40.1	43.0	42.5
	Year 1	39.1	41.3	40.9	39.2	41.3	40.9
	Year 2	39.1	40.8	40.4	39.5	40.7	40.4
Pcnt. change from Year 0	Year 1	-2.0%	-3.8%	-3.7%	-2.2%	-4.0%	-3.9%
	Year 2	-2.0%	-5.1%	-4.8%	-1.5%	-5.5%	-4.9%
Significance of changes (p-value)	Year 1	0.406	0.000	0.000	0.326	0.000	0.000
	Year 2	0.418	0.000	0.000	0.527	0.000	0.000

Table 4
Proportion Indicating a Desire to Work Longer Hours

		Narrow Definition			Broad Definition		
	Period	Men	Women	All	Men	Women	All
Averages (weighted by employment)	Year 0	0.30	0.17	0.19	0.30	0.17	0.20
	Year 1	0.27	0.18	0.19	0.27	0.17	0.19
	Year 2	0.29	0.17	0.19	0.29	0.17	0.19
Pcnt. change from Year 0	Year 1	-10.9%	1.8%	-0.2%	-12.2%	0.2%	-1.7%
	Year 2	-2.9%	-4.2%	-1.4%	-4.2%	-3.9%	-1.6%
Significance of changes (p-value)	Year 1	0.201	0.753	0.977	0.131	0.971	0.725
	Year 2	0.764	0.498	0.814	0.668	0.522	0.775

Table 5
Average Real Monthly Earnings at September 2004 Prices

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	621	595	599	653	600	608
Feb. 2002	Year 0	581	567	570	586	568	571
Sept. 2002	(Omit)	523	519	519	519	518	518
Mar. 2003	Year 1	629	571	582	620	570	581
Sept. 2003	Year 1	626	613	615	619	625	624
Mar. 2004	Year 2	648	656	655	657	654	655
Sept. 2004	Year 2	684	704	700	682	706	701
Averages (weighted by employment)	Year 0	598	581	583	615	583	589
	Year 1	628	592	599	619	597	602
	Year 2	667	679	677	670	679	677
Pcnt. change from Year 0	Year 1	5.0%	1.9%	2.6%	0.7%	2.4%	2.2%
	Year 2	11.5%	17.0%	16.0%	9.0%	16.4%	15.0%
Significance of changes (p-value)	Year 1	0.371	0.289	0.154	0.891	0.212	0.240
	Year 2	0.027	0.000	0.000	0.087	0.000	0.000

Monthly earnings calculated from reported regular weekly wages, assuming 4.35 weeks of work per month.
In September of 2004 the rand was worth $0.153

Table 6
Full and Part-Time Employment
(Thousands)

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	155	920	1075	170	944	1114
Feb. 2002	Year 0	217	995	1212	228	1010	1238
Sept. 2002	(Omit)	190	871	1061	200	887	1087
Mar. 2003	Year 1	218	871	1089	252	934	1186
Sept. 2003	Year 1	191	885	1076	205	920	1125
Mar. 2004	Year 2	204	884	1088	216	920	1135
Sept. 2004	Year 2	220	805	1025	242	845	1087
Averages	Year 0	186	957	1143	199	977	1176
	Year 1	204	878	1082	229	927	1156
	Year 2	212	845	1057	229	883	1111
Pcnt. change from Year 0	Year 1	9.8%	-8.3%	-5.3%	14.8%	-5.1%	-1.7%
	Year 2	14.1%	-11.8%	-7.6%	15.0%	-9.6%	-5.5%
Significance of changes (p-value)	Year 1	0.137	0.001	0.025	0.018	0.033	0.460
	Year 2	0.074	0.000	0.012	0.047	0.001	0.063

Table 7
Estimated Crude Elasticity of Employment with Respect to Average Wages

	Narrow Definition			Broad Definition		
	Men	Women	All	Men	Women	All
Years 0→ 1	1.96	-1.55	-0.95	-28.11	-0.76	-0.30
Years 0→ 2	1.13	-0.64	-0.43	2.18	-0.51	-0.32
Years 1→ 2	0.56	-0.28	-0.19	0.02	-0.38	-0.33

Elasticities are calculated as the ratio of log changes, not arithmetic percentage differences; the former have the advantage of symmetry and additivity; the latter may be calculated by dividing the percentages reported here by those in Table 1.

Table 8
Total Hours Worked Per Week
(Millions)

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	6.2	38.9	45.1	6.9	39.9	46.8
Feb. 2002	Year 0	8.6	43.2	51.8	9.1	43.9	52.9
Sept. 2002	(Omit)	7.7	36.6	44.3	8.1	37.3	45.4
Mar. 2003	Year 1	8.6	36.3	44.9	10.0	38.9	48.9
Sept. 2003	Year 1	7.4	36.3	43.7	7.9	37.7	45.6
Mar. 2004	Year 2	8.2	36.7	44.9	8.8	38.2	46.9
Sept. 2004	Year 2	8.3	32.1	40.4	9.3	33.5	42.9
Averages	Year 0	7.4	41.0	48.4	8.0	41.9	49.9
	Year 1	8.0	36.3	44.3	9.0	38.3	47.2
	Year 2	8.3	34.4	42.7	9.0	35.8	44.9
Pcnt. change from Year 0	Year 1	7.8%	-11.6%	-8.6%	12.4%	-8.6%	-5.3%
	Year 2	12.0%	-16.2%	-11.9%	13.4%	-14.4%	-10.0%
Significance of changes (p-value)	Year 1	0.262	0.000	0.000	0.061	0.000	0.028
	Year 2	0.130	0.000	0.000	0.078	0.000	0.001

Table 9
Estimated Crude Elasticity of Total Hours with Respect to Average Wages

	Narrow Definition			Broad Definition		
	Men	Women	All	Men	Women	All
Years 0→ 1	1.57	-2.21	-1.56	-23.77	-1.32	-0.95
Years 0→ 2	0.97	-0.91	-0.69	1.97	-0.79	-0.61
Years 1→ 2	0.56	-0.39	-0.29	0.14	-0.51	-0.44

Elasticities are calculated as the ratio of log changes, not arithmetic percentage differences; the former have the advantage of symmetry and additivity; the latter may be calculated by dividing the percentages reported here by those in Table 1.

Table 10
Total Real Monthly Earnings
(Millions of Rand at September 2004 Prices)

		Narrow Definition			Broad Definition		
Survey Date	Period	Men	Women	All	Men	Women	All
Sept. 2001	Year 0	96	536	631	110	553	663
Feb. 2002	Year 0	122	551	673	129	560	689
Sept. 2002	(Omit)	98	444	542	102	452	554
Mar. 2003	Year 1	136	487	623	154	523	677
Sept. 2003	Year 1	115	532	647	121	565	686
Mar. 2004	Year 2	130	574	704	140	594	734
Sept. 2004	Year 2	149	552	700	163	575	738
Averages	Year 0	109	544	652	119	557	676
	Year 1	125	510	635	138	544	682
	Year 2	140	563	703	151	585	736
Pcnt. change from Year 0	Year 1	15.3%	-6.3%	-2.6%	15.3%	-2.3%	0.8%
	Year 2	28.5%	3.5%	7.7%	26.7%	5.0%	8.9%
Significance of changes (p-value)	Year 1	0.116	0.045	0.395	0.094	0.461	0.800
	Year 2	0.009	0.432	0.082	0.012	0.256	0.042

Table 11
Number of Unemployed Domestic Workers, Percentage Receiving UIF Benefits, and Months Since Last Worked

		Men			Women			Total		
Survey Date	Period	Number (1000s)	Receiving UIF pmts.	Months since last worked	Number (1000s)	Receiving UIF pmts.	Months since last worked	Number (1000s)	Receiving UIF pmts.	Months since last worked
Sept. 2001	Year 0	91	0.00%	48.0	1242	0.24%	56.3	1333	0.22%	55.7
Feb. 2002	Year 0	94	0.68%	42.9	1147	0.23%	55.7	1241	0.27%	54.7
Sept. 2002	(Omit)	101	0.00%	46.2	1201	0.12%	57.4	1302	0.11%	56.5
Mar. 2003	Year 1	109	0.50%	43.5	1255	0.12%	57.0	1364	0.15%	56.0
Sept. 2003	Year 1	104	0.43%	48.7	1235	0.12%	57.3	1339	0.15%	56.6
Mar. 2004	Year 2	100	0.00%	49.9	1265	0.14%	57.0	1365	0.13%	56.5
Sept. 2004	Year 2	140	0.08%	46.6	1303	0.34%	56.5	1443	0.32%	55.6
Averages	Year 0	93	0.34%	45.5	1195	0.24%	56.0	1287	0.24%	55.3
	Year 1	106	0.46%	46.1	1245	0.12%	57.2	1351	0.15%	56.3
	Year 2	120	0.05%	48.1	1284	0.24%	56.7	1404	0.23%	56.0
Pcnt. change from Year 0	Year 1	15.1%	34.4%	1.4%	4.2%	-47.8%	2.1%	5.0%	-38.6%	1.9%
	Year 2	30.1%	-86.0%	5.8%	7.5%	2.8%	1.3%	9.1%	-7.3%	1.4%
p-value (changes)	Year 1	0.060	0.764	0.786	0.043	0.193	0.037	0.015	0.276	0.065
	Year 2	0.002	0.181	0.244	0.004	0.959	0.220	0.000	0.881	0.214

Unemployed domestics selected via industry code for last job held; the universe is all household members aged 15 and above who did not work and were not absent from work in the last seven days but did work at some point in their lives. The months since last worked were coded as 1-6, then six months to less than one year (which I recoded as 12); then 1 year to less than 2 years (recoded as 24); then 2 years to less than 3 years (recoded as 36); then 3 years of more (recoded as 72).

Table 12
Least Squares Regression of Change in Mean Log Real Hourly Wage
Against Initial Share Below Minimum (Narrow Definition, District Councils, No Weights)

Women	Change in mean log wage Year 0→1			Change in mean log wage Year 0→2			Change in mean log wage Year 1→2		
Share initially below minimum	0.445‡ (0.128)	0.405‡ (0.132)	0.503‡ (0.136)	0.357† (0.140)	0.355† (0.139)	0.501‡ (0.149)	0.158 (0.140)	0.138 (0.131)	0.298‡ (0.144)
Change in log non-dom. wage		0.263 (0.186)	0.199 (0.193)		0.070 (0.115)	0.066 (0.125)		0.162 (0.130)	0.175 (0.140)
Change in non-dom. emp./pop.		-0.404 (0.698)	-0.971 (0.698)		0.394 (0.502)	0.221 (0.476)		-0.135 (0.460)	-0.214 (0.470)
Share initially having UIF			1.396* (0.822)			-0.863 (0.597)			0.416* (0.245)
Share initially having Contract			0.163 (0.517)			0.913† (0.438)			-0.001 (0.167)
Share initially having Paid Leave			-0.317 (0.272)			0.285 (0.214)			-0.008 (0.171)
Share initially having Pension			0.816 (0.765)			-0.427 (0.635)			0.612* (0.345)
Intercept	-0.257* (0.104)	-0.232* (0.107)	-0.351* (0.135)	-0.067 (0.112)	-0.065 (0.109)	-0.262* (0.139)	0.000 (0.112)	0.004 (0.106)	-0.219 (0.140)
R^2	0.16	0.20	0.31	0.14	0.15	0.31	0.04	0.07	0.22
N	53	53	53	53	53	53	53	53	53
Men									
Share initially below minimum	0.856* (0.445)	0.884† (0.433)	0.866* (0.509)	0.850‡ (0.192)	0.885‡ (0.200)	0.904‡ (0.222)	0.668‡ (0.188)	0.576‡ (0.186)	0.637‡ (0.211)
Change in log non-dom. wage		0.358 (0.480)	0.298 (0.501)		0.031 (0.295)	0.035 (0.322)		0.611† (0.277)	0.649† (0.312)
Change in non-dom. emp./pop.		2.326 (1.581)	2.035 (1.714)		1.775* (0.987)	2.026* (1.053)		2.614 (1.606)	3.176* (1.604)
Share initially having UIF			0.255 (0.475)			-0.499 (0.434)			0.007 (0.610)
Share initially having Contract			-0.248 (0.440)			-0.210 (0.388)			-0.090 (0.458)
Share initially having Paid Leave			0.242 (0.391)			0.604 (0.418)			0.084 (0.376)
Share initially having Pension			-0.582 (1.418)			-0.287 (1.058)			0.397 (0.551)
Intercept	-0.640* (0.361)	-0.609* (0.357)	-0.613 (0.449)	-0.524‡ (0.135)	-0.531‡ (0.144)	-0.560‡ (0.167)	-0.363‡ (0.125)	-0.369‡ (0.138)	-0.436‡ (0.161)
R^2	0.22	0.26	0.27	0.28	0.33	0.36	0.26	0.36	0.37
N	51	51	51	52	52	52	51	51	51

Robust standard errors in parentheses. * significant at 10%; † significant at 5%; ‡ significant at 1%

Table 13
Regression of Change in Mean Log Real Wage Against Initial Share Below Minimum:
Summary of Parameter Estimates for Share Below Minimum Under 432 Different Specifications

Women	Years	Mean estimate	Mean estimate: no weights	Mean estimate: weights	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	0.429	0.482	0.376	0	0	3	21
	0→2	0.356	0.406	0.305	0	0	3	21
	1→2	0.110	0.168	0.052	0	4	11	9
Municipalities	0→1	0.565	0.693	0.436	0	0	0	24
	0→2	0.411	0.540	0.283	0	0	0	24
	1→2	0.239	0.370	0.108	0	0	6	18
Magisterial Districts	0→1	0.591	0.681	0.501	0	0	0	24
	0→2	0.486	0.533	0.439	0	0	0	24
	1→2	0.296	0.420	0.171	0	0	0	24
Averages /Totals	0→1	0.528	0.619	0.438	0	0	3	69
	0→2	0.418	0.493	0.343	0	0	3	69
	1→2	0.215	0.319	0.110	0	4	17	51
	All	0.387	0.477	0.297	0	4	23	189

Men	Years	Mean estimate	Mean estimate: no weights	Mean estimate: weights	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	0.578	0.659	0.498	0	0	4	20
	0→2	0.771	0.760	0.782	0	0	0	24
	1→2	0.757	0.767	0.747	0	0	0	24
Municipalities	0→1	0.662	0.883	0.441	0	0	3	21
	0→2	0.642	0.654	0.630	0	0	0	24
	1→2	0.728	0.737	0.718	0	0	0	24
Magisterial Districts	0→1	0.766	0.898	0.635	0	0	0	24
	0→2	0.810	0.782	0.839	0	0	0	24
	1→2	0.786	0.865	0.707	0	0	0	24
Averages /Totals	0→1	0.669	0.813	0.525	0	0	7	65
	0→2	0.741	0.732	0.750	0	0	0	72
	1→2	0.757	0.790	0.724	0	0	0	72
	All	0.722	0.778	0.666	0	0	7	209

Estimated under least squares and least absolute deviations; with and without weights; using narrow and broad definitions. Then add controls for change in mean log wage for non-domestic workers and change in non-domestic employment to population ratio; then add controls for initial share in compliance with three non-wage provisions (UIF, contract, paid leave) and share offering pension. Significance is defined at the 10% level.

Table 14
Regression of Change in Mean Log Real Wage Against Initial Share Below Minimum:
Summary of Results For Other Explanatory Variables Under Different Specifications

Women	Average coefficient	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant	Total
Change in log non-dom. hourly wage	0.110	7	15	74	48	144
Change in log non-dom. emp/pop ratio	-0.050	20	61	50	13	144
Share initially having UIF	0.069	0	27	32	13	72
Share initially having Contract	0.282	4	7	37	24	72
Share initially having Paid Leave	0.058	4	21	35	12	72
Share initially having Pension	0.186	6	17	40	9	72
Men						
Change in log non-dom. hourly wage	0.033	10	53	64	17	144
Change in log non-dom. emp/pop ratio	0.851	3	15	86	40	144
Share initially having UIF	-0.034	14	28	21	9	72
Share initially having Contract	-0.124	10	35	23	4	72
Share initially having Paid Leave	0.140	3	16	40	13	72
Share initially having Pension	-0.183	10	29	23	10	72

See notes to Table 13.

Table 15
Least Absolute Deviations Regressions of Change in Average Hours of Work per Week Against Initial Share Below Minimum (Narrow Definition, Magisterial Districts, No Weights)

Women	Change in hours/week Year 0→1			Change in hours/week Year 0→2			Change in hours/week Year 1→2		
Share initially below minimum	-10.75‡ (3.93)	-10.50† (4.15)	-13.06‡ (3.44)	-6.21‡ (2.28)	-5.69† (2.56)	-8.44† (3.40)	-5.31† (2.56)	-5.63† (2.58)	-7.21† (3.18)
Change in log non-dom. wage		-2.71 (2.85)	-2.66 (2.27)		-1.59 (1.53)	-3.41* (1.96)		-6.42‡ (2.21)	-5.81† (2.59)
Change in non-dom. emp./pop.		12.23 (10.78)	14.59* (8.45)		9.31 (5.77)	9.77 (7.41)		-12.55 (7.95)	-8.76 (9.42)
Share initially having UIF			8.01 (7.68)			4.14 (8.28)			1.08 (5.92)
Share initially having Contract			-10.13 (6.64)			-0.80 (5.86)			-0.20 (4.22)
Share initially with Paid Leave			3.33 (4.73)			-3.51 (4.54)			-3.97 (4.18)
Share initially having Pension			-23.67 (15.06)			-22.91 (13.90)			-12.37 (9.36)
Intercept	7.33† (3.32)	7.63† (3.50)	10.28‡ (3.14)	2.42 (1.93)	2.27 (2.13)	6.01* (3.06)	3.07 (2.07)	3.50 (2.06)	6.00 (2.90)
Pseudo-R^2	0.035	0.044	0.064	0.012	0.017	0.029	0.012	0.029	0.044
N	300	299	299	300	299	299	293	293	293
Men									
Share initially below minimum	-8.20† (3.77)	-8.24† (3.56)	-7.17* (3.79)	-5.99* (3.54)	-4.88 (3.98)	-5.09 (3.91)	-8.05† (3.19)	-7.59† (3.46)	-7.99† (3.87)
Change in log non-dom. wage		-6.53 (4.42)	-8.57* (4.55)		-3.85 (4.04)	-4.17 (3.82)		-3.69 (4.31)	-5.96 (4.67)
Change in non-dom. emp./pop.		-6.83 (14.07)	2.83 (14.62)		17.27 (14.64)	17.07 (15.36)		-15.56 (15.17)	-23.44 (17.18)
Share initially having UIF			-6.21 (8.06)			-1.23 (8.75)			-5.30 (5.67)
Share initially having Contract			-1.08 (7.27)			6.63 (8.23)			-0.84 (4.45)
Share initially with Paid Leave			2.84 (5.01)			-9.32 (6.12)			3.86 (5.45)
Share initially having Pension			3.99 (15.55)			-9.27 (15.21)			-3.71 (7.86)
Intercept	4.96 (3.14)	4.84 (2.99)	4.57 (3.27)	2.93 (2.95)	3.39 (3.28)	4.83 (3.30)	6.20† (2.58)	5.98† (2.81)	7.61† (3.26)
Pseudo-R^2	0.023	0.037	0.048	0.009	0.017	0.027	0.021	0.023	0.028
N	168	168	165	182	182	178	176	176	175

Robust standard errors in parentheses. * significant at 10%; † significant at 5%; ‡ significant at 1%.

Table 16
Regression of Change in Average Hours Per Week Against Initial Share Below Minimum:
Summary of Parameter Estimates for Share Below Minimum Under 432 Different Specifications

Women	Years	Mean estimate	Mean estimate: no weights	Mean estimate: weights	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	-7.70	-7.30	-8.09	16	8	0	0
	0→2	-7.05	-5.75	-8.35	17	7	0	0
	1→2	-4.26	-4.06	-4.47	11	12	1	0
Municipalities	0→1	-8.76	-11.91	-5.60	20	4	0	0
	0→2	-5.10	-2.26	-7.94	12	12	0	0
	1→2	-7.01	-7.61	-6.40	24	0	0	0
Magisterial Districts	0→1	-10.95	-13.81	-8.09	24	0	0	0
	0→2	-8.54	-7.19	-9.89	24	0	0	0
	1→2	-8.01	-7.21	-8.80	24	0	0	0
Averages /Totals	0→1	-9.13	-11.01	-7.26	60	12	0	0
	0→2	-6.89	-5.06	-8.73	53	19	0	0
	1→2	-6.43	-6.29	-6.56	59	12	1	0
	All	-7.48	-7.45	-7.51	172	43	1	0

Men	Years	Mean estimate	Mean estimate: no weights	Mean estimate: weights	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	-9.54	-11.33	-7.76	14	10	0	0
	0→2	-11.79	-12.94	-10.63	15	9	0	0
	1→2	-13.22	-14.48	-11.96	23	1	0	0
Municipalities	0→1	-11.33	-12.05	-10.61	24	0	0	0
	0→2	-7.47	-6.71	-8.22	19	5	0	0
	1→2	-11.70	-12.47	-10.93	20	4	0	0
Magisterial Districts	0→1	-6.89	-7.19	-6.59	17	7	0	0
	0→2	-6.41	-5.81	-7.00	12	12	0	0
	1→2	-10.76	-12.19	-9.34	24	0	0	0
Averages /Totals	0→1	-8.32	-10.19	-9.25	55	17	0	0
	0→2	-8.62	-8.49	-8.56	46	26	0	0
	1→2	-10.74	-13.05	-11.89	67	5	0	0
	All	-9.23	-10.58	-9.90	168	48	0	0

Estimated under least squares and least absolute deviations; with and without weights; using narrow and broad definitions. Then add controls for change in mean log wage for non-domestic workers and change in non-domestic employment to population ratio; then add controls for initial share in compliance with three non-wage provisions (UIF, contract, paid leave) and share offering pension. Significance is defined at the 10% level.

Table 17
Regression of Change in Average Hours Per Week Against Initial Share Below Minimum: Summary of Results For Other Explanatory Variables Under Different Specifications

Women	Average coefficient	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant	Total
Change in log non-dom. hourly wage	-2.20	48	64	29	3	144
Change in log non-dom. emp/pop ratio	2.32	28	40	53	23	144
Share initially having UIF	3.50	0	16	42	14	72
Share initially having Contract	-5.52	15	43	14	0	72
Share initially having Paid Leave	-1.59	13	32	24	3	72
Share initially having Pension	-12.88	32	27	9	4	72
Men						
Change in log non-dom. hourly wage	-3.30	26	84	33	1	144
Change in log non-dom. emp/pop ratio	-13.80	31	51	43	19	144
Share initially having UIF	-0.29	4	39	19	10	72
Share initially having Contract	-5.22	22	32	11	7	72
Share initially having Paid Leave	-8.61	36	28	8	0	72
Share initially having Pension	-12.90	23	39	8	2	72

See notes to Table 16.

Table 18
Least Squares Regressions of Change in Domestic Employment to Population Ratio Against Initial Share Below Minimum (Broad Definition, Magisterial Districts, No Weights):

Women	Change in domestic emp/pop Year 0→1			Change in domestic emp/pop Year 0→2			Change in domestic emp/pop Year 1→2		
Share initially below minimum	0.012 (0.014)	0.017 (0.013)	0.012 (0.016)	-0.042† (0.019)	-0.034* (0.019)	-0.047† (0.021)	-0.043‡ (0.014)	-0.039‡ (0.014)	-0.043‡ (0.016)
Change in log non-dom. wage		-0.029† (0.012)	-0.026† (0.012)		-0.028† (0.012)	-0.031† (0.013)		-0.014 (0.009)	-0.013 (0.009)
Change in non-dom. emp./pop.		0.068 (0.064)	0.064 (0.061)		0.049 (0.047)	0.046 (0.044)		0.132‡ (0.050)	0.132† (0.052)
Share initially having UIF			0.003 (0.053)			0.014 (0.057)			-0.001 (0.026)
Share initially having Contract			-0.095† (0.043)			-0.065 (0.045)			-0.016 (0.016)
Share initially with Paid Leave			0.008 (0.021)			-0.029 (0.030)			-0.011 (0.022)
Share initially having Pension			0.006 (0.106)			-0.147* (0.088)			0.012 (0.042)
Intercept	-0.020* (0.010)	-0.022† (0.011)	-0.012 (0.016)	0.013 (0.016)	0.010 (0.016)	0.034* (0.020)	0.021* (0.011)	0.020* (0.012)	0.028* (0.015)
R^2	0.00	0.04	0.07	0.02	0.06	0.11	0.04	0.09	0.10
N	304	303	303	301	300	300	297	297	297
Men									
Share initially below minimum	-0.006 (0.016)	-0.013 (0.017)	-0.011 (0.017)	-0.012 (0.014)	-0.018 (0.014)	-0.019 (0.014)	-0.021† (0.010)	-0.020† (0.010)	-0.022† (0.010)
Change in log non-dom. wage		0.039* (0.022)	0.041* (0.023)		0.030 (0.025)	0.030 (0.025)		-0.024 (0.018)	-0.025 (0.019)
Change in non-dom. emp./pop.		-0.016 (0.091)	-0.014 (0.094)		-0.036 (0.053)	-0.041 (0.055)		0.054 (0.054)	0.041 (0.055)
Share initially having UIF			-0.007 (0.040)			0.030 (0.020)			-0.014 (0.017)
Share initially having Contract			0.005 (0.040)			-0.024 (0.025)			0.013 (0.019)
Share initially with Paid Leave			0.022 (0.033)			0.006 (0.017)			-0.003 (0.018)
Share initially having Pension			-0.019 (0.096)			-0.036* (0.022)			-0.011 (0.021)
Intercept	0.066‡ (0.014)	0.070‡ (0.015)	0.067‡ (0.014)	0.058‡ (0.011)	0.058‡ (0.011)	0.059‡ (0.012)	0.062‡ (0.008)	0.064‡ (0.009)	0.065‡ (0.009)
Pseudo-R^2	0.00	0.03	0.03	0.00	0.03	0.04	0.02	0.04	0.05
N	228	228	226	227	227	225	223	223	221

Robust standard errors in parentheses. * significant at 10%; † significant at 5%; ‡ significant at 1%.

Table 19

Regression of Change in Domestic Employment/Population on Initial Share Below Minimum: Summary of Parameter Estimates for Share Below Minimum Under 432 Different Specifications

Women	Years	Mean estimate	Mean estimate: no weights	Mean estimate: weights	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	0.009	0.014	0.004	1	2	21	0
	0→2	-0.012	-0.010	-0.014	4	9	11	0
	1→2	-0.017	-0.018	-0.016	9	15	0	0
Municipalities	0→1	0.013	0.023	0.002	1	4	17	2
	0→2	-0.017	-0.020	-0.015	7	17	0	0
	1→2	-0.018	-0.021	-0.016	10	14	0	0
Magisterial Districts	0→1	0.007	0.010	0.004	0	3	21	0
	0→2	-0.023	-0.033	-0.013	14	10	0	0
	1→2	-0.018	-0.029	-0.006	12	8	4	0
Averages /Totals	0→1	0.010	0.015	0.004	2	9	59	2
	0→2	-0.017	-0.021	-0.014	25	36	11	0
	1→2	-0.018	-0.023	-0.013	31	37	4	0
	All	-0.008	-0.009	-0.008	58	82	74	2
Men								
District Councils	0→1	-0.008	-0.009	-0.007	3	15	6	0
	0→2	-0.013	-0.015	-0.010	6	12	6	0
	1→2	-0.021	-0.023	-0.018	12	3	8	1
Municipalities	0→1	-0.016	-0.017	-0.016	6	17	1	0
	0→2	-0.018	-0.015	-0.021	13	7	4	0
	1→2	-0.022	-0.021	-0.024	15	9	0	0
Magisterial Districts	0→1	-0.004	-0.006	-0.003	1	18	5	0
	0→2	-0.008	-0.010	-0.006	1	19	4	0
	1→2	-0.009	-0.010	-0.008	6	14	4	0
Averages /Totals	0→1	-0.009	-0.010	-0.008	10	50	12	0
	0→2	-0.013	-0.013	-0.013	20	38	14	0
	1→2	-0.017	-0.018	-0.017	33	26	12	1
	All	-0.013	-0.014	-0.013	63	114	38	1

Estimated under least squares and least absolute deviations; with and without weights; using narrow and broad definitions. Then add controls for change in mean log wage for non-domestic workers and change in non-domestic employment to population ratio; then add controls for initial share in compliance with three non-wage provisions (UIF, contract, paid leave) and share offering pension. Significance is defined at the 10% level.

Table 20
Regression of Change in Domestic Employment/Population On Initial Share Below Minimum:
Summary of Results For Other Explanatory Variables Under Different Specifications

Women	Average coefficient	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant	Total
Change in log non-dom. hourly wage	-0.016	46	85	11	2	144
Change in log non-dom. emp/pop ratio	0.040	2	36	70	36	144
Share initially having UIF	-0.054	13	50	9	0	72
Share initially having Contract	-0.022	24	26	20	2	72
Share initially having Paid Leave	-0.018	17	31	20	4	72
Share initially having Pension	-0.016	7	31	30	4	72
Men						
Change in log non-dom. hourly wage	0.004	9	63	50	22	144
Change in log non-dom. emp/pop ratio	0.030	10	37	82	15	144
Share initially having UIF	0.009	4	22	31	15	72
Share initially having Contract	-0.011	15	31	17	9	72
Share initially having Paid Leave	0.005	11	18	26	17	72
Share initially having Pension	-0.035	24	29	15	4	72

See notes to Table 19.

Table 21a

Two Stage Least Squares Regressions of Change in Domestic Employment to Population Ratio
Against Change in Mean Log Hourly Wages (Women, Broad Definition, Magisterial Districts, No Weights)

Women	Change in domestic emp/pop Year 0→1				Change in domestic emp/pop Year 0→2				Change in domestic emp/pop Year 1→2			
Change in mean log wage	0.016 (0.018)	0.022 (0.017)	0.015 (0.020)	0.017 (0.020)	-0.069† (0.033)	-0.056* (0.032)	-0.077† (0.037)	-0.063* (0.033)	-0.084† (0.034)	-0.085† (0.038)	-0.082† (0.038)	-0.079† (0.037)
Wage instruments	a	a	a	b	a	a	a	b	a	a	a	b
Change in log non-dom. wage		-0.030† (0.012)	-0.027† (0.012)	-0.029† (0.012)		-0.021 (0.013)	-0.021 (0.014)	-0.020 (0.014)		0.011 (0.015)	0.011 (0.015)	0.009 (0.014)
Change in non-dom. emp./pop.		0.080 (0.063)	0.072 (0.058)	0.076 (0.061)		0.063 (0.052)	0.065 (0.052)	0.063 (0.053)		0.126† (0.052)	0.124† (0.055)	0.126† (0.052)
Share initially having UIF			0.006 (0.051)				0.011 (0.059)				-0.009 (0.029)	
Share initially having Contract			-0.096† (0.042)				-0.063 (0.047)				-0.010 (0.019)	
Share initially with Paid Leave			0.007 (0.020)				-0.032 (0.033)				0.001 (0.022)	
Share initially having Pension			-0.001 (0.101)				-0.117 (0.094)				0.048 (0.043)	
Intercept	-0.011‡ (0.003)	-0.010‡ (0.003)	-0.003‡ (0.005)	-0.009‡ (0.003)	-0.007‡ (0.007)	-0.006‡ (0.007)	0.010† (0.011)	-0.005 (0.007)	-0.002 (0.005)	-0.002 (0.005)	-0.002 (0.007)	-0.003 (0.005)
Test Hansen's J=0 (p-value)				0.16				0.16				0.83
Mean emp./pop.	0.0899	0.0901	0.0901	0.0901	0.0843	0.0846	0.0846	0.0846	0.0798	0.0798	0.0798	0.0798
Elasticity of emp./pop. to wage	0.17	0.25	0.17	0.18	-0.82	-0.66	-0.92	-0.75	-1.05	-1.06	-1.03	-0.99
N	304	303	303	303	301	300	300	300	297	297	297	297

Robust standard errors in parentheses. * significant at 10%; † significant at 5%; ‡ significant at 1%.
Wage Instruments: (a) Share below minimum; (b) Share below minimum and the four non-wage variables

Table 21b
Two Stage Least Squares Regressions of Change in Domestic Employment to Population Ratio
Against Change in Mean Log Hourly Wages (Men, Broad Definition, Magisterial Districts, No Weights)

Men	Change in domestic emp/pop Year 0→1				Change in domestic emp/pop Year 0→2				Change in domestic emp/pop Year 1→2			
Change in mean log wage	-0.007 (0.019)	-0.016 (0.021)	-0.014 (0.020)	-0.013 (0.020)	-0.015 (0.018)	-0.023 (0.018)	-0.024 (0.018)	-0.020 (0.016)	-0.021† (0.010)	-0.020† (0.010)	-0.021† (0.010)	-0.020† (0.010)
Wage instruments	a	a	a	b	a	a	a	b	a	a	a	b
Change in log non-dom. wage		0.039* (0.022)	0.042* (0.023)	0.039* (0.022)		0.030 (0.025)	0.031 (0.025)	0.030 (0.025)		-0.021 (0.017)	-0.023 (0.018)	-0.022 (0.017)
Change in non-dom. emp./pop.		-0.021 (0.091)	-0.018 (0.093)	-0.021 (0.092)		-0.025 (0.054)	-0.030 (0.054)	-0.027 (0.054)		0.064 (0.056)	0.053 (0.056)	0.057 (0.056)
Share initially having UIF			-0.008 (0.040)				0.023 (0.021)				-0.018 (0.016)	
Share initially having Contract			0.003 (0.040)				-0.020 (0.026)				0.012 (0.018)	
Share initially with Paid Leave			0.022 (0.032)				0.006 (0.016)				0.003 (0.017)	
Share initially having Pension			-0.030 (0.101)				-0.053* (0.032)				-0.009 (0.019)	
Intercept	0.062‡ (0.004)	0.060‡ (0.004)	0.059‡ (0.005)	0.060‡ (0.004)	0.051‡ (0.005)	0.048‡ (0.004)	0.049‡ (0.005)	0.047‡ (0.004)	0.049‡ (0.004)	0.051‡ (0.004)	0.051‡ (0.004)	0.051‡ (0.004)
Test Hansen's J=0 (p-value)				0.94				0.50				0.64
Mean emp./pop.	0.0524	0.0524	0.0527	0.0527	0.0483	0.0483	0.0486	0.0486	0.0491	0.0491	0.0492	0.0492
Elasticity of emp./pop. to wage	-0.14	-0.30	-0.26	-0.25	-0.32	-0.47	-0.49	-0.41	-0.43	-0.41	-0.44	-0.42
N	228	228	226	226	227	227	225	225	223	223	221	221

Robust standard errors in parentheses. * significant at 10%; † significant at 5%; ‡ significant at 1%.
Wage Instruments: (a) Share below minimum; (b) Share below minimum and the four non-wage variables.

Table 22
IV Estimates of Change in Domestic Employment/Population:
Summary of Parameter Estimates for Share Below Minimum Under 288 Different Specifications

Women	Years	Average Elasticity	Negative & significant	Negative & not significant	Positive & not significant	Positive & significant
District Councils	0→1	0.44	0	1	15	0
	0→2	-0.23	2	6	8	0
	1→2	-0.98	4	7	4	1
Municipalities	0→1	0.31	0	1	13	2
	0→2	-0.42	0	16	0	0
	1→2	-1.27	4	11	0	1
Magisterial Districts	0→1	0.10	0	2	14	0
	0→2	-0.62	10	6	0	0
	1→2	-0.89	8	8	0	0
Averages /Totals	0→1	0.28	0	4	42	2
	0→2	**-0.42**	12	28	8	0
	1→2	-1.04	16	26	4	2
Median	**0→2**	**-0.46**				
Men						
District Councils	0→1	-0.34	0	13	3	0
	0→2	-0.47	1	11	4	0
	1→2	-0.54	8	1	7	0
Municipalities	0→1	-0.61	5	9	2	0
	0→2	-0.69	8	7	1	0
	1→2	-0.72	8	8	0	0
Magisterial Districts	0→1	-0.11	0	11	5	0
	0→2	-0.27	0	16	0	0
	1→2	-0.23	4	10	2	0
Averages /Totals	0→1	-0.35	5	33	10	0
	0→2	**-0.48**	9	34	5	0
	1→2	-0.50	20	19	9	0
Median	**0→2**	**-0.33**				

The women's average elasticity is calculated omitting two outliers with elasticities of +70 and -6. These affect the rows for Years 1→2 at the district council and municipal level only. Significance tests are of the underlying coefficient from the 2SLS regression.

Figure 1
Domestic Worker Employment Levels Using Revised Population Weights,
Under Broad and Narrow Sample Definitions,
Compared to Statistics South Africa's Published Estimates

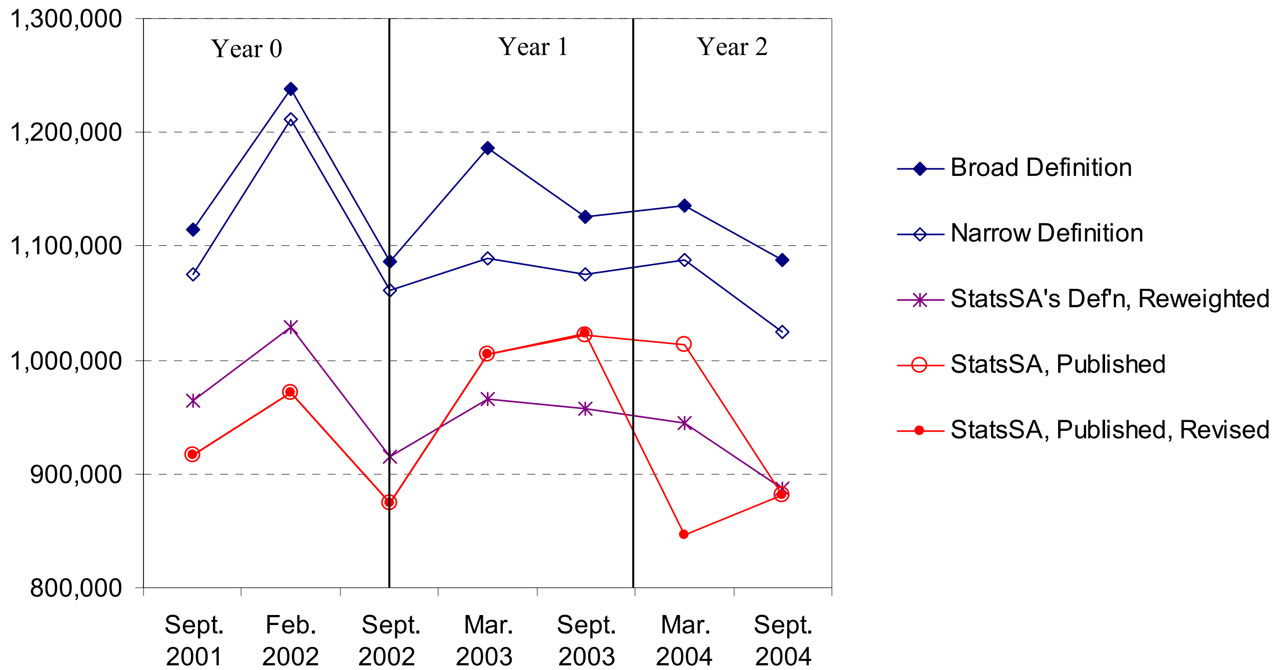
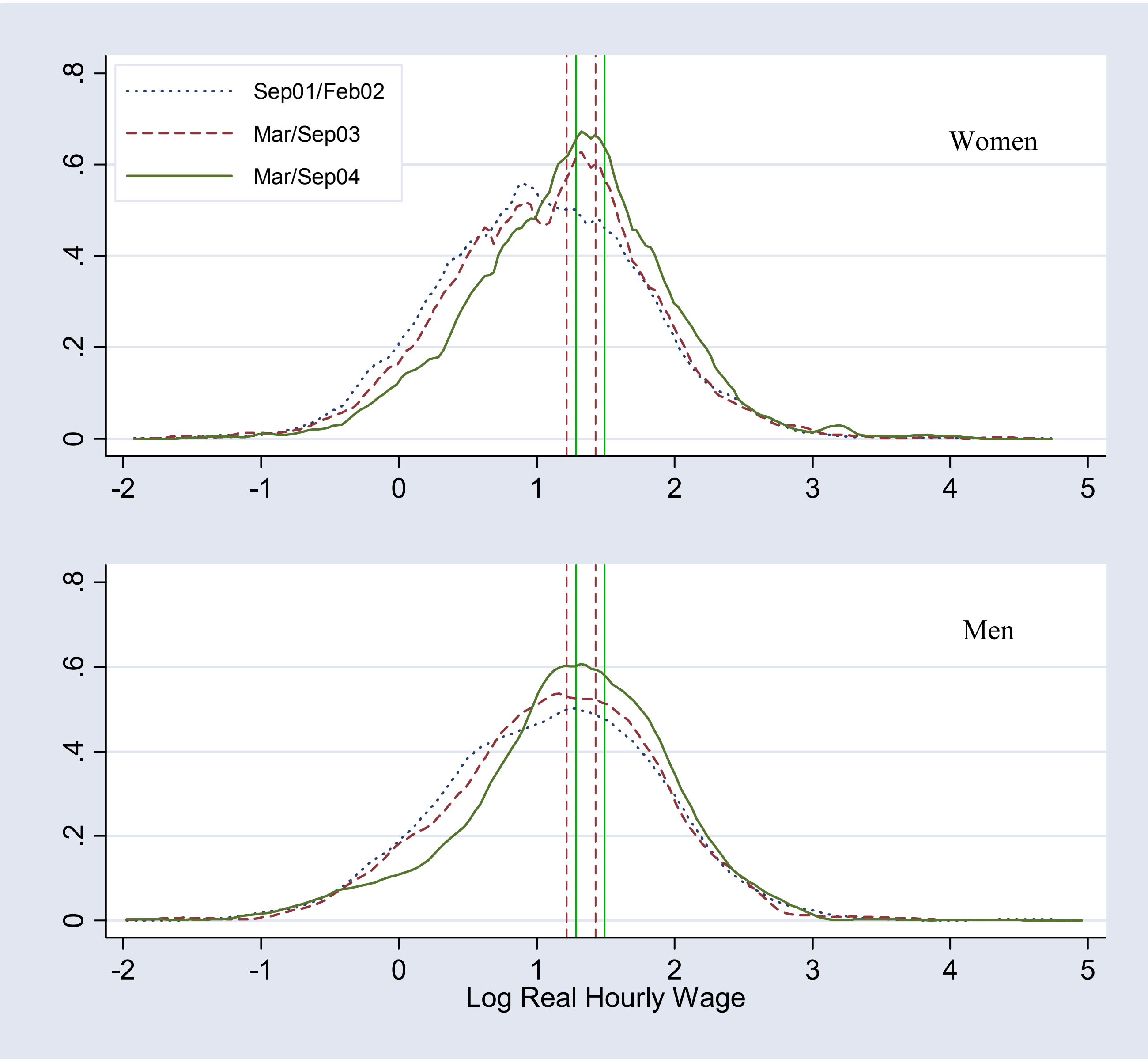
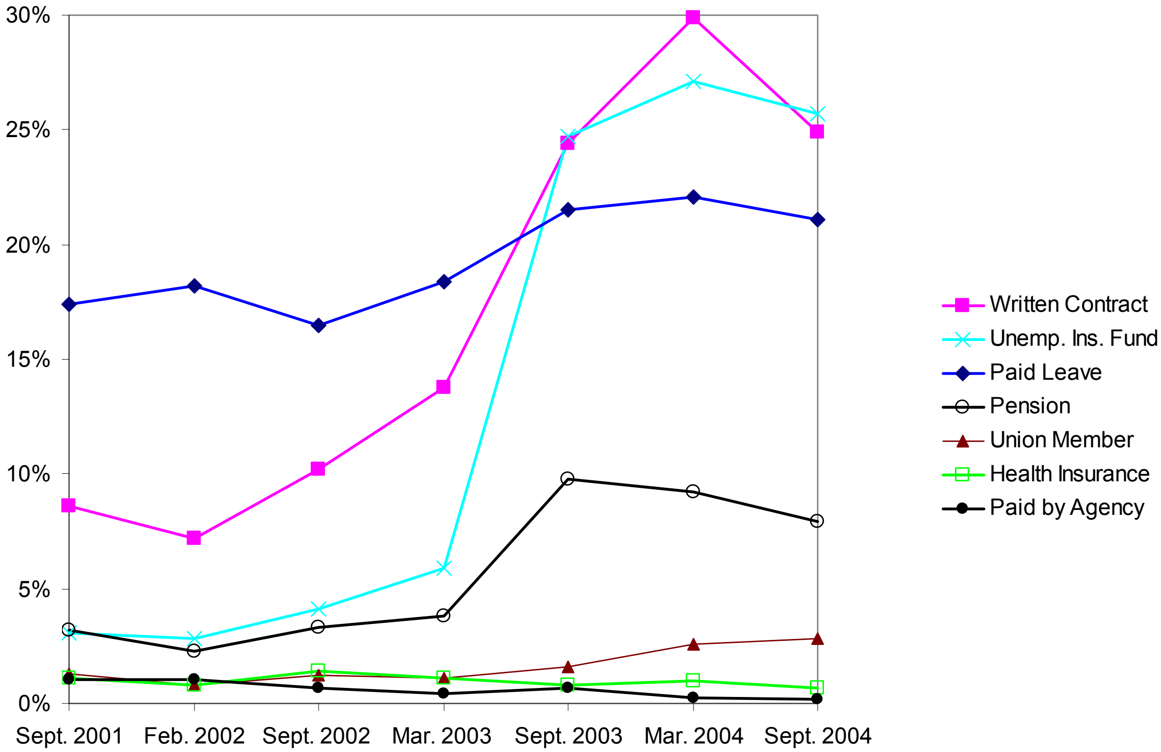


Figure 2
Kernal Density Plots of Log Real Hourly Wages, Men and Women, Broad Definition



Vertical lines represent the (logs of the) real values of the stipulated minima for full time workers, adjusted to September 2004 prices. Full time workers make up roughly 80% of the sample. For March+September 2003 (Year 1, dashed lines) the nominal hourly figures were R3.33 in lower-wage areas, and R4.10 in Area A. These rise to R3.59 and R4.42 for March+September 2004 (Year 2, solid lines). Note that the actual effective hourly minima will be higher for the 35% of the sample who report working more than 45 regular hours per week, since they are entitled to time-and-a-half overtime payments.

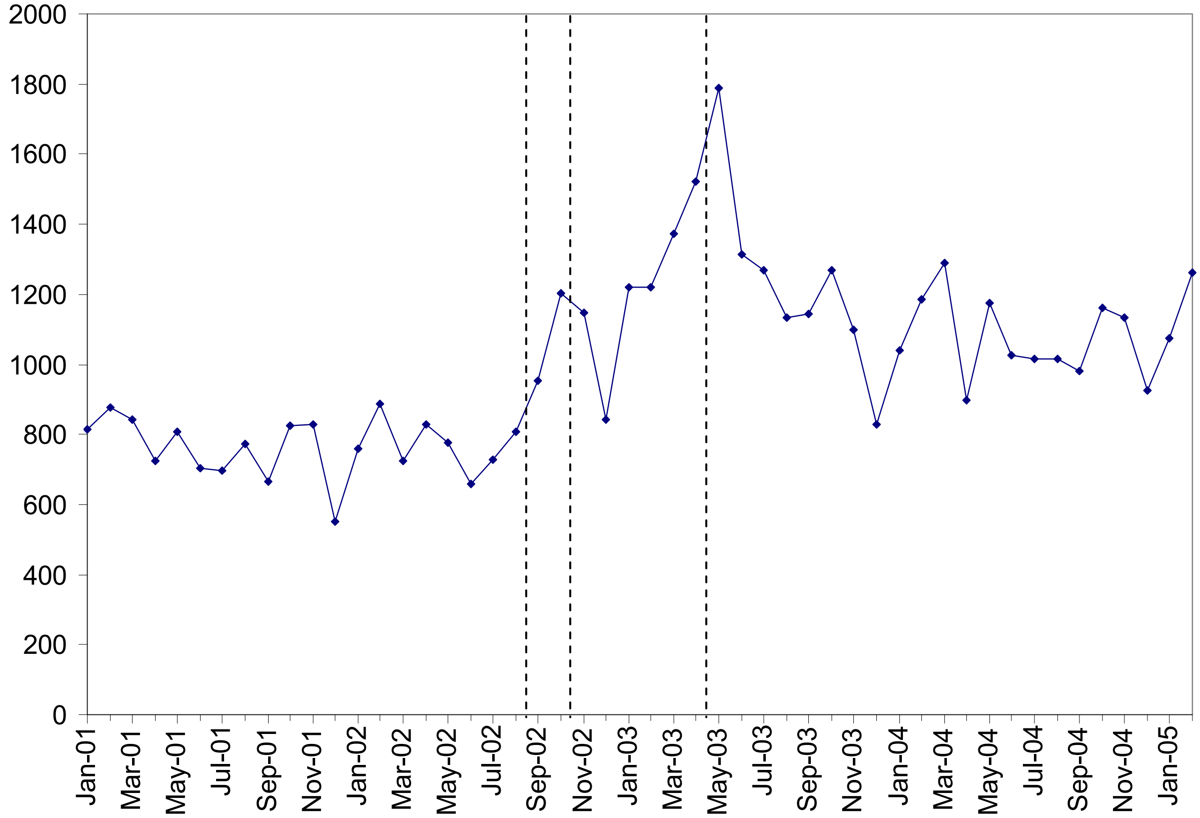
Figure 3
Reported Non-Wage Terms of Employment



Written contracts and paid leave were required as of 1 September 2002. UIF registration was required by 30 April 2003. Pensions, union membership, and health insurance are not required. The narrow sample definition is used; results under the broad definition are virtually identical.

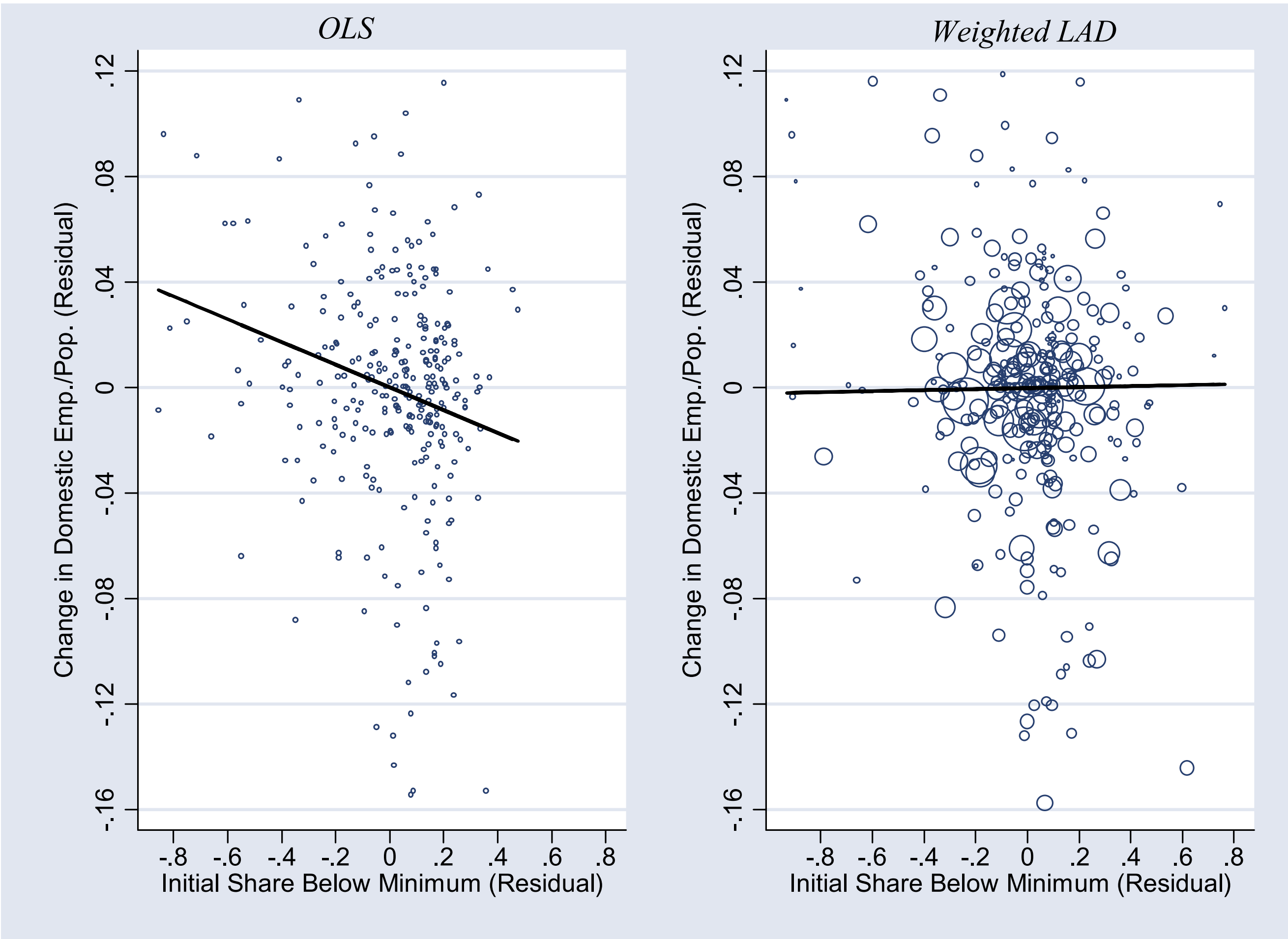
Figure 4

Domestic Worker Cases Referred to the Commission for Conciliation, Mediation and Arbitration



Non-wage provisions (such as written contracts and paid leave) were required as of the first of September 2002. Minimum wages were required as of 1 November 2002. UIF registration was required by 30 April 2003. The clear drop in complaints each December is comforting.

Figure 5
Partial Regression Plot of Unweighted OLS and Weighted LAD Estimates of the Change in Domestic Employment/Population Ratio (Years 1→2) Against Initial Share Below Minimum (Women, Broad Definition, Magisterial Districts)



The left figure plots the results of the unweighted OLS regression reported in the last column of Table 18, upper panel (slope -0.043), with the 297 magisterial districts as the units of analysis. The right is the weighted LAD equivalent (slope 0.005; not significant; regression not reported in Table), with the size of the circles indicating the number of domestic workers in the district. The data are the residuals after removing the predicted effects of non-domestic wages and employment and the four non-wage variables. Six outliers (with y-values below -0.16 or above 0.12) are omitted for clarity, but included in the regressions.

Appendix 1
Direction of Measurement Error Biases in the Wage Regressions

Let $\bar{s}_0$ be the regional average share below the minimum in Year 0, for region *i* (*i*-subscripts omitted), and let the measurement error associated with $\bar{s}_0$ be denoted $\bar{\varepsilon}_0$, which we will assume behaves as additive iid error term in the regional regressions. Let $\bar{w}_0 + \bar{e}_0$ and $\bar{w}_1 + \bar{e}_1$ be the regional average log hourly wages, and their (additive, iid) average measurement error terms, in Years 0 and 1. The dependent variable in the wage equation is $\bar{w}_1 + \bar{e}_1 - \bar{w}_0 - \bar{e}_0$, and the expression for the OLS regression of this against $\bar{s}_0 + \bar{\varepsilon}_0$ is:

$$\beta^{OLS} = \frac{Cov(\bar{w}_1 + \bar{e}_1 - \bar{w}_0 - \bar{e}_0, \bar{s}_0 + \bar{\varepsilon}_0)}{Var(\bar{s}_0 + \bar{\varepsilon}_0)}$$

In expanding and rearranging this, we may drop terms in (*w, e*), (*w, ε*), (e_1, e_0), and (e_1, ε_0) whose covariances are assumed zero, yielding:

$$\text{plim}\,\beta^{OLS} = \frac{Cov(\bar{w}_1 - \bar{w}_0, \bar{s}_0) - Cov(\bar{e}_0, \bar{\varepsilon}_0)}{Var(\bar{s}_0) + Var(\bar{\varepsilon}_0)}$$

Define $R = \dfrac{Var(\bar{s}_0)}{Var(\bar{s}_0) + Var(\bar{\varepsilon}_0)}$, the reliability with which the regional share is measured. As the number of observations in the regions increases, $Var(\bar{s}_0)$ will converge to the population variance across regions (of regional average shares below the minimum), while $Var(\bar{\varepsilon}_0)$ will converge to zero, as measurement errors wash out. As a result, *R* rises towards 1. Now define the desired estimator that would obtain absent measurement error: $\hat{\beta} = \dfrac{Cov(\bar{w}_1 - \bar{w}_0, \bar{s}_0)}{Var(\bar{s}_0)}$.

Substitution yields:

$$\text{plim}\,\beta^{OLS} = R\left(\hat{\beta} - \frac{Cov(\bar{e}_0, \bar{\varepsilon}_0)}{Var(\bar{s}_0)}\right).$$

Because $\bar{\varepsilon}_0$ is the error in measuring the Year 0 average share *below* the minimum, it will be *negatively* correlated with $\bar{e}_0$, the error in measuring the average wage, so that $Cov(\bar{e}_0, \bar{\varepsilon}_0) < 0$. Since it enters with a minus sign, above, this term works to bias the (positive) estimator upwards, but the bias is partially offset by the attenuation term, *R*. As the number of people in each region grows, the average errors within the covariance term should tend toward zero, and they should display a vanishing amount of covariance. Simultaneously, R should converge to 1 and the OLS estimator to its unbiased value.

Assuming $\hat{\beta} > 0$, we can be sure that the net effect of the two biases is positive by noting that the condition: $R\left(\hat{\beta} - \frac{Cov(\bar{e}_0, \bar{\varepsilon}_0)}{Var(\bar{s}_0)}\right) > \hat{\beta}$ is implied by $\frac{\hat{\beta}Var(\bar{s}_0)}{Cov(\bar{e}_0, \bar{\varepsilon}_0)} > \frac{R}{R-1} = \frac{-Var(\bar{s}_0)}{Var(\bar{\varepsilon}_0)}$, or $\hat{\beta} > \frac{Cov(\bar{e}_0, \bar{\varepsilon}_0)}{Var(\bar{\varepsilon}_0)}$, which is true because the right hand term is negative.

If the regression compared the change in average wages to the initial average wage itself, rather than the initial share below the minimum, then $\bar{e}_0 = \bar{\varepsilon}_0$ and the expression simplifies to: $\beta^{OLS:wages} = R\tilde{\beta} - (1-R)$. Now the true negative effect ($\tilde{\beta}$) is made more negative by the term *(1-R)*, but also pulled back toward zero by the attenuation factor *R*. There will be remain a net negative bias, pulling the observed value below the true (negative) $\tilde{\beta}$ provided $0 > \tilde{\beta} > -1$ which it must be.

Appendix 2
Reproduction of Tables 1 and 2 from Sectoral Determination (Department of Labour 2002a)

Table 1: Minimum wages for domestic workers who work more than 27 ordinary hours per week *

AREA A					
Bergrivier Local Municipality, Breederivier Local Municipality, Buffalo City Local Municipality, Cape Agulhas Local Municipality, Cederberg Local Municipality, City of Cape Town, City of Johannesburg Metropolitan Municipality, City of Tshwane Metropolitan Municipality, Drakenstein Local Municipality, Ekurhulen Metropolitan Municipality, Emalahleni Local Municipality, Emfuleni Local Municipality, Ethekwini Metropolitan Unicity, Gamagara Local Municipality, George Local Municipality, Hibiscus Coast Local Municipality, Karoo Hoogland Local Municipality, Kgatelopele Local Municipality, Khara Hais Local Municipality, Knysna Local Municipality, Kungwini Local Municipality, Kouga Local Municipality, Langeberg Local Municipality, Lesedi Local Municipality, Makana Local Municipality, Mangaung Local Municipality, Matzikama Local Municipality, Metsimaholo Local Municipality, Middelburg Local Municipality, Midvaal Local Municipality, Mngeni Local Municipality, Mogale Local Municipality, Mosselbaai Local Municipality, Msunduzi Local Municipality, Mtubatu Local Municipality, Nama Khoi Local Municipality, Nelson Mandela, Nokeng tsa Taemane Local Municipality, Oudtshoorn Local Municipality, Overstrand Local Municipality, Plettenbergbaai Local Municipality, Potchefstroom Local Municipality, Randfontein Local Municipality, Richtersveld Local Municipality, Saldanha Bay Local Municipality, Sol Plaatjie Local Municipality, Stellenbosch Local Municipality, Swartland Local Municipality, Swellendam Local Municipality, Theewaterskloof Local Municipality, Umdoni Local Municipality, uMhlathuze Local Municipality and Witzenberg Local Municipality.					
Minimum rates for the period 1 November 2002 to 31 October 2003		**Minimum rates for the period 1 November 2003 to 31 October 2004**		**Minimum rates for the period 1 November 2004 to 31 October 2005**	
Hourly rate (R)	4,10	Hourly rate (R)	4,42	Hourly rate (R)	4,77
Weekly rate (R)	184,62	Weekly rate (R)	198,90	Weekly rate (R)	214,65
Monthly rate (R)	800,00	Monthly rate (R)	861,90	Monthly rate (R)	930,15
AREA B					
AREAS NOT MENTIONED IN AREA A					
Minimum rates for the period 1 November 2002 to 31 October 2003		**Minimum rates for the period 1 November 2003 to 31 October 2004**		**Minimum rates for the period 1 November 2004 to 31 October 2005**	
Hourly rate (R)	3,33	Hourly rate (R)	3,59	Hourly rate (R)	3,87
Weekly rate (R)	150,00	Weekly rate (R)	161,55	Weekly rate (R)	174,15
Monthly rate (R)	650,00	Monthly rate (R)	700,05	Monthly rate (R)	754,65

Table 2: Minimum wages for part time domestic workers who work 27 ordinary hours per week or less*

AREA A					
Bergrivier Local Municipality, Breederivier Local Municipality, Buffalo City Local Municipality, Cape Agulhas Local Municipality, Cederberg Local Municipality, City of Cape Town, City of Johannesburg Metropolitan Municipality, City of Tshwane Metropolitan Municipality, Drakenstein Local Municipality, Ekurhulen Metropolitan Municipality, Emalahleni Local Municipality, Emfuleni Local Municipality, Ethekwini Metropolitan Unicity, Gamagara Local Municipality, George Local Municipality, Hibiscus Coast Local Municipality, Karoo Hoogland Local Municipality, Kgatelopele Local Municipality, Khara Hais Local Municipality, Knysna Local Municipality, Kungwini Local Municipality, Kouga Local Municipality, Langeberg Local Municipality, Lesedi Local Municipality, Makana Local Municipality, Mangaung Local Municipality, Matzikama Local Municipality, Metsimaholo Local Municipality, Middelburg Local Municipality, Midvaal Local Municipality, Mngeni Local Municipality, Mogale Local Municipality, Mosselbaai Local Municipality, Msunduzi Local Municipality, Mtubatu Local Municipality, Nama Khoi Local Municipality, Nelson Mandela, Nokeng tsa Taemane Local Municipality, Oudtshoorn Local Municipality, Overstrand Local Municipality, Plettenbergbaai Local Municipality, Potchefstroom Local Municipality, Randfontein Local Municipality, Richtersveld Local Municipality, Saldanha Bay Local Municipality, Sol Plaatjie Local Municipality, Stellenbosch Local Municipality, Swartland Local Municipality, Swellendam Local Municipality, Theewaterskloof Local Municipality, Umdoni Local Municipality, uMhlathuze Local Municipality and Witzenberg Local Municipality.					
Minimum rates for the period 1 November 2002 to 31 October 2003		**Minimum rates for the period 1 November 2003 to 31 October 2004**		**Minimum rates for the period 1 November 2004 to 31 October 2005**	
Hourly rate (R)	4,51	Hourly rate (R)	4,87	Hourly rate (R)	5,25
Weekly rate (R)	212,77	Weekly rate (R)	131,49	Weekly rate (R)	141,75
Monthly rate (R)	527,67	Monthly rate (R)	569,79	Monthly rate (R)	614,25
AREA B					
AREAS NOT MENTIONED IN AREA A					
Minimum rates for the period 1 November 2002 to 31 October 2003		**Minimum rates for the period 1 November 2003 to 31 October 2004**		**Minimum rates for the period 1 November 2004 to 31 October 2005**	
Hourly rate (R)	3,66	Hourly rate (R)	3,95	Hourly rate (R)	4,26
Weekly rate (R)	98,82	Weekly rate (R)	106,65	Weekly rate (R)	115,02
Monthly rate (R)	428,22	Monthly rate (R)	462,15	Monthly rate (R)	498,42