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A needle in a haystack.
Do increases in the minimum wage
cause employment losses?

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Does increasing the minimum wage lead to employment losses? For many years most economists thought that the answer to this was a straight forward ‘yes’. However, research during the 1990s began to overturn this conventional wisdom and showed that increases in the minimum wage did not automatically lead to employment losses. At present, the literature on this important topic remains divided, both in Europe and the United States. A recent Australian study, by Andrew Leigh, examined the impact of statutory minimum wages in Western Australia and reached conclusions which supported the conventional view. However, close scrutiny of Leigh’s paper shows that it is fundamentally flawed. It suffers from both methodological and empirical weaknesses which are so severe as to make its claims unsustainable. Despite Leigh’s efforts, it remains the case that we simply do not know a great deal about the employment impact of Australia’s system of minimum wages.

Debating the impact of minimum wages

Does increasing the minimum wage lead to employment losses? In recent decades this question has intrigued economists, particularly since the mid-1990s when seminal work by American economists, Card and Krueger, unsettled the conventional wisdom (see the consolidated research in Card and Krueger, 1995). That wisdom was largely based on theoretical considerations which decreed that if the minimum wage was set at a level above the market-clearing wage, then labour demand would drop and employment would fall. In the words of Charles Brown: ‘attempts to raise poorly paid workers’ wages will cost some of them their jobs’ (Brown, 1995, p. 827). This logic has featured prominently in the arguments advanced by employer groups and conservative governments in their opposition to substantial increases in the minimum wage in Australia.¹ What Card and Krueger did was challenge this wisdom by showing empirically that increases in the minimum wage in some American states did not lead to job losses.

The conventional wisdom is, of course, somewhat simplistic. The ‘rise in minimum wages leads to job loss’ logic must be contextualised in at least

¹ Australia does not have a ‘minimum wage’ in the same way that some overseas countries do. Rather, the Australian Industrial Relations Commission (AIRC) adjusts *award* minimum rates of pay. It does this through an annual Safety Net Review decision (also called the Living Wage case). This decision applies to award workers who have not been subject to enterprise based bargaining and who would not be likely to gain an increase in pay without a change in the award rate of pay. The Federal change in minimum rates usually flows through to State award workers as well. Western Australia is one state that does have its own statutory minimum wage, based on the *Minimum Conditions of Employment Act 1993* which applies to non-award, non-federal employees.

two ways, as Lewis (1997) showed clearly. First, the impact of an increase in the minimum wage will have only a small impact on the *average* wage, since most workers already receive wages higher than the minimum wage. Secondly, the labour market for minimum wage workers is essentially one among many labour markets. In the minimum wage labour market, which is essentially a market for low skilled labour, labour substitutability is common and the impact of *relative* wages can be pronounced. In other words, an increase in the minimum wage for one group of low skilled workers can lead to a drop in their employment, as employers substitute other low skilled workers in their place. The net employment effect may be negligible, but the adverse effect on particular sub-groups of workers may be considerable (Lewis, 1997, pp. 204–5).

Actual employment outcomes also depend on assumptions about employer behaviour. If employers choose to substitute capital for labour, in the form of labour-replacing equipment, then the net employment effect may also be adverse. In reality, most minimum wage workers are located in labour intensive industries, such as personal services, hospitality or retail, where there are physical limits to the installation of labour-replacing equipment. Where it does suit an employer to choose new machinery over labour, the time involved in its installation may result in considerable delays before the impact of an increase in minimum wages on employment becomes evident. Indeed, this question of time lags may be critical for whichever form of substitution takes place. As Borland and Woodbridge observed:

the employment response to a change in labour costs is likely to vary with the time-horizon considered . . . The variety of factors that account for the potential difference between short-term and long-term adjustment include time-lags involved in recruiting and training substitute labour for low-wage workers, organizational costs in restructuring production processes, time taken to identify and instal new capital, and the cost of severance payments to low-wage workers who are laid off (1999, p. 95).

Obviously it is difficult to reach a firm theoretical conclusion about the impact of the minimum wage on employment. A great deal depends on numerous assumptions, including the composition of the workforce, the degree of competition in the labour market, and the impact of higher wages on consumer demand. In particular, the size of the minimum wage increase can be critical: below a certain threshold its impact may be insignificant.²

² Ehrenberg (1992, p. 5) observed in summarising the state of play in the minimum wage debate at the beginning of the 1990s: ‘it is significant that none of the studies suggest that at current relative values of the minimum wage, large disemployment effects would result from modest increases in the minimum wage—increases up to, say, 10%. In this sense, all the findings are very consistent.’ Writing in 1997, Dawkins (1997, p. 192)

With so many caveats involved, it is wise to be cautious when it comes to theoretical pronouncements. As Dolado et al. observed:

Predictions of economic theories are almost always sensitive to assumptions. We are surprised by an unconditional claim [that raising the minimum wage automatically leads to unemployment] . . . and sceptical that anyone actually believes it. Yet it pervades the analysis of the minimum wage (1996, pp. 327–28).

It is clear that detailed empirical studies are more useful for shedding light on the relationship between minimum wages and employment. As Borland and Woodbridge (1999, p. 96) observed ‘the question of how employment of low-wage workers will be affected by an increase in wages must be resolved empirically’. Similarly, Card and Krueger (1995, p. 7) argued that elucidating this relationship requires ‘systematic empirical study’ rather than the ‘abstract theoretical reasoning’ which has characterised the field.

Unfortunately, much of the empirical literature is far from systematic or rigorous. Prior to the 1980s, it often entailed repeating the same economic modeling exercise against similar data. For example, an early overview on the impact of minimum wages (Brown et al., 1982) concluded that a 10 per cent increase in the minimum wage would have an adverse impact on employment of between 1 and 3 per cent. The authors of this study conceded, however, that their conclusion was not based on a large body of research because ‘one could argue that there really are not 25 independent studies’ (1982, p. 502). Their use of the same labour force data and the similarity of their methodologies meant that most of the studies simply replicated each other.

More sophisticated analyses began to emerge during the late 1980s, as researchers began to make use of longitudinal data. Detailed studies of firms were also conducted, which allowed researchers to examine the impact of increases in minimum wages on the employment patterns within those firms (Katz and Krueger, 1992; Card and Krueger, 1994). The best known of these studies was Card and Krueger’s investigation of the 1992 increase in statutory minimum wages in New Jersey. Termed a ‘natural experiments’ approach, Card and Krueger compared the impact of a rise in the minimum wage (from \$4.25 to \$5.05) on employment in fast-food restaurants in New Jersey with similar restaurants in Pennsylvania where no such wage increase had occurred.³ The firms affected by the wage increase were regarded as

offered a similar view: ‘while a small increase in minimum wages may not have much of an effect on employment, a large increase might have a significant negative effect. It seems that this is becoming the conventional wisdom.’

³ Card had adopted a similar approach, using Current Population Survey data (that is, individual-level rather than firm-level data) to study the impact of increases in statutory minimum wages in California during the late 1980s (Card, 1992).

a ‘treatment group’ and the firms where no wage increases occurred were regarded as a ‘control group’. A comparison between the employment outcomes for the two groups resembled an *experiment*, whilst the real world setting, with its policy-prescribed changes, provided the *natural* dimension. (Other researchers, including Andrew Leigh, have used the term ‘quasi-experiment’ in a similar vein.)

What made Card and Krueger’s research so important was its controversial findings alongside its methodological rigour. It reversed the conventional wisdom by showing that increasing statutory minimum wages had no deleterious effects on employment, and it did so in a way which survived critical scrutiny. Subsequent criticism (for example, Welch, 1995; Neumark and Wascher, 2000) was answered by means of a reanalysis of the New Jersey wage increase using payroll data (Card and Krueger, 1998, 2000). This study confirmed Card and Krueger’s earlier survey-based findings.

It may be the case, as Machin and Manning (1997, p. 735) pointed out, that increasing the minimum wage has less impact on employment in the United States because the minimum wage is so low in that country. It might be argued that in countries where the ratio of minimum wages to average wages is much higher—such as in Europe and Australia—the adverse effect on employment of increasing minimum wages will be much more pronounced. It is important, therefore, to examine what recent empirical research in Europe and Australia has shown.

Not surprisingly, recent European research has produced contentious results. Several studies of the impact of the minimum wage in France, the Netherlands, Spain and the United Kingdom failed to find strong evidence showing that increasing minimum wages invariably led to employment losses (Dolado et al., 1996; Dickens et al., 1999). In a summary of a number of earlier studies Machin and Manning, p. 739 concluded:

our study of the experience of minimum wages in four European countries finds very little evidence of important employment effects associated with minimum wages. Effects are typically small and in some cases go the opposite way in terms of predictions of the orthodox model of the labour market. On the whole our results seem in line with the recent US work that fails to find any evidence of job loss associated with minimum wages. The emphasis seems to have shifted from ‘how negative are the employment effects?’ to ‘is there an employment effect?’ and, in some circles, ‘what potential is there for a positive effect?’ (1997, p. 739).

At the same time, other research on French minimum wages has found a stronger relationship between increases in minimum wages and employment losses (Abowd et al., 1999), particularly for young workers (Bazen and

Skourias, 1997). This is consistent with the conclusions of the OECD summary of the literature. They found that over 20 major studies had been carried out during the 1990s examining the impact of the minimum wage on employment (OECD, 1998). The results of these studies varied considerably, and there were no clear, unambiguous findings. The OECD concluded tentatively:

young workers may be most vulnerable to job losses at a high level of the minimum wage. There is less evidence available on the employment effects, if any, for other groups such as women and part-time workers (OECD, 1998, p. 31).

There have been few Australian studies during the last decade which have explored in any detail the relationship between minimum wages and employment outcomes. Some research, such as Borland and Woodbridge (1999), looked closely at this issue, but did not advance any new empirical findings. Other studies, which did include empirical findings, were focussed more generally on wages and unemployment. For example, the research by Debelle and Vickery (1998)—which suggested that a one per cent drop in unemployment might follow a two per cent reduction in the growth of real wages—was based entirely on macro-economic modeling of aggregate wages and employment data.

One recent study which did look specifically at minimum wages and employment using more detailed data was that by Mangan and Johnston (1999). While their concerns were posed in the context of proposals to introduce training wages for young people, they located their study within the broader debate about the employment effects of minimum wages. Their findings regarding this relationship were equivocal. Their first model—which examined the employment effects for young people of changes in the relativity between youth and adult wages—produced estimates which were not statistically significant (at 95 per cent level or above) (1999, p. 423). Their second model examined different labour market outcomes for young people contingent on the youth-adult wage relativity. This time their findings were statistically significant, but the size of the estimates were small, leading them to conclude:

the results indicate that the inverse relationship between wages and employment in the youth labour market is small . . . Taken overall, wages do not appear to be a major factor in determining youth employment numbers (1999, pp. 426–427).

An important study of the youth labour market by Junankar et al. (2000) argued that much of the econometrics behind the youth minimum wage debate was methodologically flawed. They argued that many researchers often

ignored the importance of part-time work and the impact of school retention rates on employment, both of which are critical issues for modelling the youth labour market accurately. In their own modelling of data disaggregated by industry, Junankar et al. (2000, p. 184) found that the estimated wage elasticities were ‘almost always incorrectly signed [in terms of the conventional wisdom’s expectation] or statistically insignificant’. They concluded: ‘we have still not found the elusive Australian elasticities, and we don’t think anyone else has beaten us to it’ (2000, p. 184).

Some of the most convincing evidence to date has come from across the Tasman. New Zealander researchers, Hyslop and Stillman (2004), examined very large increases in youth wages in the period following 2001 and found evidence of *positive* employment responses. They studied reforms to the minimum wage system which brought about wage increases for 18 and 19 year-olds in the order of 69 per cent, and increases for 16 and 17 year-olds of about 41 per cent. As Hyslop and Stillman (2004, p. 1) noted, the large wage increases studied by Card and Krueger were in the range of 19 to 27 per cent. If there was to be an adverse impact on employment, then, clearly, wage increases of this magnitude should have made it evident. Instead, Hyslop and Stillman (2004, p. 2) found:

positive employment responses to the changes for both groups of teenagers, and that 16–17 year-olds increased their hours worked by 10–15 percent following the minimum wage changes.

In summary, the theoretical arguments about wages and unemployment hinge on what assumptions are made, while the empirical research remains strongly divided. One of the main problems with the minimum wages debate is the tendency to pursue a generally acceptable and universally applicable answer to the relationship between wages and employment. Such a goal is elusive, if not utopian. There are so many unique factors in a local, or even national economies, that universally applicable findings are impossible to obtain. This does not stop economists trying to find the ‘true’ elasticity of demand for low wage labour (that is, the responsiveness in the demand for labour following a change in wages). In some cases, economists review the current literature and then average out the various elasticities to arrive at an estimate of what the ‘real’ elasticity might be: ‘Taking all the studies for the many countries together, a “best guess” for the long-run constant-output labour-demand elasticity based on this literature is -0.30’ (meaning that a 1 per cent rise in wages results in a 0.3 per cent fall in employment) (Hamermesh, 1999). This kind of logic led Kennan to critically note:

There seems to be an implicit belief that an average of the estimates from many such studies must mean something. But in fact if there is one impeccable study in the set, and if the results of this study are inconclusive, what is gained by tossing in

the results of the other studies and taking an average? What if all of the studies are impeccable, and they are all inconclusive? (Kennan, 1995, p. 1955)

A recent Australian study: Andrew Leigh's quasi-experiment

The most recent Australian study to explore the impact of minimum wages on employment has been written by an Australian, Andrew Leigh, currently working at Harvard University. Writing in the December 2003 issue of the *Australian Economic Review*, Leigh posed the question: 'Does raising the minimum wage cost jobs?' (2003, p. 361). Leigh's answer consisted of a 'quasi-experiment' which looked at the situation in Western Australia between 1994 and 2001, when six statutory minimum wage increases were handed down. By comparing employment rates before and after the increases, he sought to show that minimum wages did cost jobs, and he made use of a control group (the rest of Australia) to validate his before-and-after results.⁴

Leigh located his analysis in the tradition of natural experiments (see, for example, Meyer, 1995; Kennan, 1995), the framework behind the Card and Krueger studies mentioned above (Card and Krueger, 1994, 1995). As noted earlier, this approach tests the impact of policy changes, such as increases in statutory minimum wages, by comparing a treatment group (a group subject to the change) and a control group (a group not subject to the change).

The techniques for assessing the differences between the treatment group and the control group vary. Some studies have used fixed effects models, where regressions are fitted to the data, using controls for state fixed effects and year effects (see overview in Besley and Case, 2000, pp. F681–F685). Other studies have used 'difference-in-difference' estimators, where the observations from both groups are pooled and then analysed in one of two ways. Either simple differences across states and across time are calculated, or regressions are run against the data. It is important to note that difference-in-difference estimators have been subject to considerable criticism in recent years (see, for example, Bertrand et al., 2002; Johansson and Selén, 2002).

Leigh's approach made use of difference-in-difference estimators, using Australian Bureau of Statistics time-series data for seasonally-adjusted, full-time equivalent employment to population ratios. He calculated the before-and-after difference in these employment rates for Western Australia by subtracting the employment rate three months after a minimum wage in-

⁴ Leigh made some mistakes with the data in his original article and these have since been corrected in an erratum, see Leigh (2004).

crease from the employment rate which prevailed three months before. Leigh did this for six occasions when minimum wage increases occurred in Western Australia between 1994 and 2001. His control group was the rest of Australia, so he repeated the before-and-after differencing for this control group. He then calculated the difference-in-difference between the Western Australia figure and the rest of Australia figure. For example, in the case of August 1994 he found that the employment rate in Western Australia rose by 0.0006, but the employment rate rose even greater in the rest of Australia (by 0.003) so the relative change in Western Australia was a fall of 0.002. Taking March 2001, as another example, Leigh found that the employment rate in Western Australia fell by 0.037, whilst the employment rate for the rest of Australia also fell, but by a smaller amount (0.020). Thus, the relative change in Western Australia was again a fall, this time by 0.017.⁵

Leigh also used regression analysis, but not in the fashion pursued by most natural experiments. The more usual approach is to fit a regression model to the employment data, with a reasonable number of controls, and to then scrutinise the interaction effect between the treatment group (those who got the wage increase) and the post-intervention period (the period after the wage increase). The size and direction of this coefficient provides evidence for whether the intervention has led to a change in the outcome under investigation. Leigh made no use of statistical controls except for his control group (a point to which I will return later) and did not make use of an interaction term. Rather, he fitted his regression to the difference-in-difference estimates themselves, rather than to the employment rates. The procedure he followed was to pool all the data for the period 1981 to 2002 (broken down into six month blocks) to provide 247 observations and then ran regressions on these data. The independent variable in Leigh's model was a modified dummy variable which took account of whether an increase in the statutory minimum wage had occurred in Western Australia in that six month block. Rather than setting the dummy to a value of 1, he used the size of the percentage increase in the minimum wage. This allowed him to interpret his coefficient for this variable as an elasticity of labour demand.⁶ Essentially Leigh was posing the question: do these six difference-in-difference estimates—when minimum wage increases took place—stand out from the rest of the estimates? His overall finding was that they did stand out, and that the coefficient suggested an elasticity of labour demand of -0.149 (Leigh, 2004). This can be interpreted as saying that for every 10 per cent increase in wages, there is a 1.5 per cent reduction in employment.

Leigh reported a second set of regressions based on breaking down the

⁵ Figures taken from Leigh (2004), not Leigh (2003).

⁶ Though Junankar (2004, p. 67) argued that Leigh was mistaken in assuming that his coefficient estimates represented elasticities.

data into subgroups based on age and sex. His key conclusions were that the elasticities for labour demand were much greater (as high as -0.491) for young people. The elasticities were higher for young women compared to young men (-0.624 compared to -0.362) (Leigh, 2004). Leigh later suggested that these findings for young workers supported the view that overall elasticity figures (such as his -0.149) were probably underestimates of the true elasticity of labour demand for *minimum wage* workers (as distinct from all workers). This was based on the view that younger workers make up a large proportion of minimum wage workers.

Overall, Leigh's results led him to conclude that:

The elasticity of the Western Australian statutory minimum wage appears similar to that of US minimum wages. Australian minimum wages do 'bite', but it is not clear that they bite more fiercely than in America (2003, p. 317).

A flawed study?

Has Leigh indeed shown increases in statutory minimum wages in Western Australia caused employment losses? I would argue that the answer is a definite 'no'. As I show below, his study is fundamentally flawed, with major methodological and empirical weaknesses.

Methodologically, Leigh pitches his study at the wrong level of analysis. He fails to control for a range of factors which might be influencing his results and he selects an inadequate control group. Leigh also takes no account of trends in employment during the period he studies and has no clear way of distinguishing before-and-after effects. Finally, Leigh has failed to deal satisfactorily with the problem of endogeneity (that is, the fact that the minimum wage increases may be partly determined by the same factors which are also involved in determining the employment outcomes).

Empirically, Leigh's results are inconclusive. Four of his six difference-in-difference estimates are statistically significant, but the size of the standard errors involved in this kind of exercise make the precision of his findings quite ludicrous. Leigh's regression analysis produces a model which is a very poor fit to the data, with only 4 per cent of the variability in relative employment differences between Western Australia and the rest of Australia explained by his model (Leigh, 2004).

Methodological weaknesses

Leigh conceded that he had reluctantly used Australian Bureau of Statistics macrodata aggregates—such as employment-population ratios—instead of the kind of microdata which other researchers using the natural experiments approach have employed. Obviously there is a place for the use of macrodata

aggregates in research, such as studies into the impact of large movements in average wages on employment. But when it comes to studies into the impact of small movements in relative wages on a small subset of the population, macrodata aggregates are inappropriate. As Machin and Manning (1994) argued, macro models which attempt this task assume that all sectors of the labour market are roughly equivalent. A more appropriate way to explore questions about the impact of minimum wages on employment is to use firm-level data from a panel survey, where the same firms are examined before and after some event (such as a wage increase). This was the approach used by Card and Krueger (1994) in their study into the impact of the New Jersey wage increase on the fast-food industry.

An absence of adequate statistical controls is one of Leigh's greatest weakness. In his regression modeling, Leigh made use of no other controls (such as age composition of the workforce or industry composition, to mention just two considerations). This is particularly important in the light of his poor R^2 results, which showed that 96 per cent of the variability in the outcome—the relative employment differences between Western Australia and the rest of Australia—was due to factors outside his model. *Clearly, something that Leigh was not measuring was driving the results in his modeling.* Why did Leigh not attempt to control for any other factors which might have been responsible for driving these results?

Now it might be argued that the use of a control group obviates the need for statistical controls.⁷ According to this logic, the difference-in-difference estimators should ensure that only the effects of the treatment show up in the results. However, such confidence is misplaced, and would only happen—if at all—in a genuine randomised experiment. In a 'natural' experiment the assumption that no further controls are needed is erroneous. As Hamermesh (1995, p. 838) argued in his critique of Card and Krueger: 'without true experiments there are no easy research strategies that might allow us to avoid the modeling necessary to control for changes in other variables that determine the outcomes of interest to us.'

In the absence of such controls, the burden falls even more heavily upon the choice of control group. Again, it becomes evident that Leigh's approach is flawed. As Besley and Case argued, the control group for a natural experiment must closely match the treatment group:

... control groups must be stable, and adequately reflect the effect of changes in other variables that are simultaneously influencing outcomes of the group under study ... Good control groups will be those whose fortunes have evolved similarly to

⁷ It is worth noting that many of the other natural experiment approaches used elsewhere have routinely employed a range of statistical controls as well as the use of a control group.

those of the group experiencing the policy change *and* who respond similarly to changes in variables that drive policies to change . . . our conclusion is for a return to an older issue in public finance, the need to understand where policy comes from as part of estimating its incidence (2000, p. F675).

There are good grounds for believing that the ‘rest of Australia’ is a very poor control group: the other Australian states have widely differing industry and employment characteristics, and considerable variation in their wage-fixing systems. Averaging them into a ‘rest of Australia’ control group does not resolve this problem.

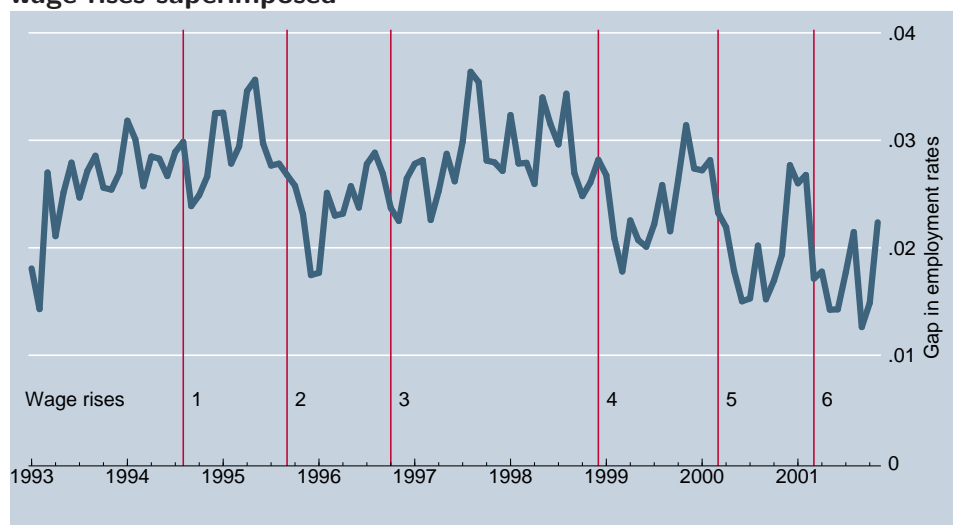
It is particularly important that trends over time do not diverge sharply between treatment and control groups, especially in the period leading up to the intervention being studied. As Card pointed out with respect to one of his early studies:

If the comparison sample is a legitimate control group, there should be no trend in the pre-1987 gap between California and the comparison sample (1992, p. 3).

In other words, employment should not already be trending either down or up in ways which diverge significantly between the treatment group and the control group. If there are trends in the gap between these two groups, then the researcher should be controlling for this factor when modeling before-and-after changes. In Leigh’s case, there is no attempt to control for this. All that he does is a sensitivity analysis to test whether any *one* of the six time periods has unduly influenced his results. He does not test the sensitivity of his estimates against different time periods. Figure 1 below shows a graph of the gap between employment rates in Western Australia and the rest of Australia during the latter part of Leigh’s period of study. This graph, based on Leigh’s data, shows upward movement when the gap favours Western Australia, and downward movement when the gap favours the rest of Australia. The superimposed wage rises indicate clearly that this gap was far from stable in the lead-up to the minimum wage ‘interventions’. In at least four of these interventions, the gap was *already* trending strongly against Western Australia.

Considerations of time also bedevil Leigh’s assumptions about cause and effect. A preliminary assumption for the natural experiments approach is that one should be able to organise the data in a clear temporal sequence. The data should provide a clear story about the situation before the intervention, the intervention itself, and then the aftermath. Economic phenomena can complicate this neat sequence: sometimes economic actors may anticipate the intervention and act beforehand; often the economic impact may have a lagged effect. Commonly researchers conducting natural experiments

Figure 1: Gap in employment-population ratios, WA and national with wage rises superimposed



Source: ABS Labour Force Survey data (modified according to Leigh's method). (Cat. 6202.0.55.001, Tables 1 and 8)

on policy interventions deal only with one major intervention, such as a single rise in the minimum wage as happened in New Jersey in 1992. This makes it relatively straightforward to grapple with anticipations and lagged effects.⁸ Leigh, however, was looking at six (almost annual) interventions, and this leads to the obvious question: 'how do we really know what is a *before* and what is an *after*'? It is quite possible that the lagged effects of one minimum wage increase may feed into the precursors of the next wage rise. Leigh assumed that a six month period on either side a wage increase was sufficient to quarantine it, but he offered no evidence for this. As mentioned earlier, the issue of time lags is a critical element in analysing the behaviour of employers in responding to increases in minimum wages. One cannot just make arbitrary assumptions about questions of time.⁹ The very foundation of the natural experiments approach hinges critically on being able to rigorously separate before and after. Leigh fails badly on this score.

Finally, distinguishing cause and effect in a natural experiments framework also requires that problems of endogeneity be properly resolved. In

⁸ Though it is interesting that some of the more penetrating criticisms of Card and Krueger's New Jersey study include the claim that their before-and-after periodisation was flawed (Hamermesh, 1995).

⁹ In his critique of Leigh's paper, Junankar (2004, p. 66) argued that increases in minimum wages were 'most likely to affect *new hires only*', not the existing workforce. This could mean that the impact of increases in minimum wages on employment 'could be spread over two or more years'.

any model which seeks to explore the impact of a policy on an outcome, the right hand side variables must include that policy as part of its explanatory framework, while the dependent variable consists of the outcome. The problem is that the policy itself may be partly determined by some of the other right hand side variables which are also involved in determining the outcome. In this case, the same economic conditions which partly determine employment levels may also play a role in determining the policy (that is, increases in the minimum wage). For example, a downturn in economic growth may lead to a drop in employment, whilst also leading to greater caution by policy makers involved in handing down minimum wage decisions. Endogeneity can lead to bias in the model's estimates, so its presence cannot be simply ignored.

Leigh considered the question of endogeneity, both in terms of the timing of the minimum wage increases, and in terms of their size. He concluded that four out of the six increases may have been endogenous, but dismissed this problem with the argument that 'this would only affect the experiment if the Western Australian economy outperformed the Rest of Australia during that period' (Leigh, 2003, p. 366). The rationale for this defence was not presented, neither was any evidence. It seems clear that endogeneity remains a severe weakness in Leigh's analysis.

Empirical weaknesses

Empirically, Leigh's paper is very weak. Four of his six difference-in-difference estimates were statistically significant, but he discussed the results *in toto*, and regarded them as findings of equivalent worth.¹⁰ Leigh presented his findings with spurious precision. Results were presented to three decimal places, yet the size of the standard errors attached to this kind of analysis makes such precision misleading. Consulting the ABS data which forms the basis for Leigh's analysis shows that the employment numbers which change from month to month are numbered in the hundreds, and six month changes are at most in the low thousands. Yet examining the tables of standard errors for Western Australia shows that figures of about 1000 persons are subject to standard errors of around 450 persons. If we want confidence levels of 95 per cent (which equates to two standard errors), then we are dealing with the quite ludicrous situation where the the margin of error around the estimate is almost comparable to the estimate itself.

It stretches credibility to believe that the kind of employment changes which Leigh was expecting to find in the data could actually be discerned

¹⁰ This is a somewhat awkward sentence because it 'spans' both Leigh's paper and his erratum. In the main paper, only two of Leigh's estimates were statistically significant, but he discussed the estimates as if all of them were of equivalent worth. The erratum showed that four of the estimates were statistically significant, but there was obviously no discussion in the erratum.

among the noise. This becomes evident if we convert Leigh's regression findings into real world figures. Leigh suggested that about 4.4 per cent of employees would be likely to be affected by minimum wage increases.¹¹ This amounts to between 30,000 to 40,000 persons in Western Australia. If one then assumes an elasticity of labour demand of -0.5 (accepting Leigh's point that the elasticity would be higher for minimum wage workers compared to the workforce as a whole), one should expect to find that a 10 per cent wage increase should lead to the loss of about 5 per cent, or 1500 to 2000 persons. A 5 per cent wage increase should lead to a loss of about 750 to 1000 persons. The six minimum wage increases in Western Australia which Leigh examined never went above 10 per cent, and averaged about 6.3 per cent over the whole period. In other words, we are looking for employment losses of around 1300 persons. Given the month-to-month variability in employment, is it conceivable that one could actually discern such an impact? As Kennan points out, with regard to studies of the elasticity of labour demand for teenagers:

we are looking for employment rate changes of about one percentage point, and such changes happen all the time, even from one month to the next. In short, [when looking for the impact of minimum wage increases] we are looking for a needle in a haystack (1995, p. 1955).

Finally, the empirical base of Leigh's paper is severely weakened by the poor regression model he developed, a model which largely failed to explain the variability in his dependent variable. As mentioned earlier the main model he presented had an R^2 of only .04, which means that increases in the minimum wage in Western Australia accounted for only 4 per cent of the variability in the dependent variable (the difference-in-difference estimates for Western Australia and the rest of Australia). In other words, 96 per cent of this variability was being driven by factors other than the increases in the minimum wage. As mentioned earlier, Leigh failed to control for other factors which might have been driving this variability.

Conclusion

During the last 15 years wage inequality in Australia has increased considerably, largely as a result of the introduction of enterprise-based bargaining

¹¹ It is important to note that Leigh's employment rate is based on employed persons (which also includes the self-employed), but his discussion of minimum wage coverage is based on employees (that is, wage and salary earners). Leigh himself ignores this distinction in his discussion. For simplicity, I also ignore this distinction in the following example. Presenting the figures in terms of employees would, if anything, strengthen the argument that the numbers involved are too small to be meaningful.

in the 1990s and the ongoing transformation of occupational and industry structures (Watson et al., 2003, ch. 8). While much of this increased dispersion has been the result of high-wage individuals earning much higher wages—the top of the labour market ‘taking off’—it has also been due to low-wage individuals falling behind. Over the last seven years, wage increases handed down by the Australian Industrial Relations Commission as part of its Safety Net Adjustment (the Living Wage case) have prevented award-dependent workers falling even further behind, and have ensured that the floor of the labour market has been sustained. As overseas research has shown, increasing the minimum wage has beneficial effects on wage dispersion, helping to curtail the growth of wage inequality (Card and Krueger, 1995; Borland and Woodbridge, 1999; Machin and Manning, 1994).

It remains the case, however, that the employment aspects of minimum wages still dominates public debate. Some commentators invoke the supposed adverse employment impact of these Living Wage increases as part of their criticism of wage regulation at the bottom of the labour market. Some of these critics would prefer too see an American-style labour market, with minimal regulation for low-paid workers (see, for example Moore, 1998). By invoking the employment argument, they attempt to sway public opinion with a logic which appears humane: the unemployed are the losers from any Living Wage increase.

Clearly, the relationship between employment and minimum wages is of great policy importance. Unfortunately, Leigh’s paper does not advance our understanding of this relationship. Despite his efforts, we remain largely ignorant about the real relationship between minimum wages and employment in Australia. Much research remains to be done, preferably using research designs which properly isolate before-and-after effects, which incorporate legitimate control groups, and which adequately control for confounding influences and compositional effects. Proper natural experiments along these lines still remain to be done in Australia.

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