*The Impact of the 1896 Factory and Shops Act on the Labor Market of Victoria, Australia*

Andrew J. Seltzer and Jeff Borland

Andrew Seltzer is Professor, Department of Economics, Royal Holloway, University of London, Egham TW20 0EX, United Kingdom. E-mail: a.seltzer@rhul.ac.uk (corresponding author). Jeff Borland is Professor, Department of Economics, University of Melbourne, Victoria 2010, Australia. E-mail: jib@unimelb.edu.au.

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Abstract

This paper examines the effects of the Victorian Factory and Shops Act, the first minimum wage law in Australia. The Act differed from modern minimum wage laws in that it established Special Boards, which set trade-specific minimum wage schedules. We use trade-level data on average wages and employment by gender and age to examine the effects of minimum wages. Although the minimum wages were binding, we find that the effects on employment were modest, at best. We speculate that this was because the Special Boards, which were comprised of industry insiders, closely matched the labor market for their trades.

Australia has amongst the longest histories of legislated minimum wages in the world. The first Australian minimum wage law, the 1896 Factory and Shops Act (henceforth FSA) in the colony (later state) of Victoria, was the second minimum wage law ever passed, following only an 1894 New Zealand law which covered far fewer workers. The FSA created trade-specific Special Boards, which were empowered to set gender, age, and occupation-specific minimum wages.[[1]](#endnote-1) Initially the FSA created Special Boards for six trades that were believed to be particularly prone to “sweating” (boots, bread, clothing, furniture, shirts, and underclothing).[[2]](#endnote-2) Later amendments to the FSA expanded the scope of the law, allowing the formation of Special Boards for essentially any trade. By 1913 126 Special Boards, covering approximately 110,000 workers existed (*Report of the Chief Inspector* 1914).

The Victorian experience with minimum wages was observed with great interest by contemporary Australian, British, and American economists; social reformers; and government administrators, several of whom sailed out to Australia to observe the working of the law first-hand. Many of these observers regarded the FSA as having successfully increased the wages of the low paid, while causing little economic hardship for employers. Social reformer and future United States Supreme Court Justice Louis Brandeis stated that the law “had created much better conditions in industry than had existed prior to its passage” (Brandeis 1915, p. 494). The influential British economist and social reformer Sidney Webb wrote, “In the sweated trades to which the law was first applied, wages have gone up, the hours of labor have invariably been reduced, and the actual number of persons employed, far from falling, has in all cases, relatively to the total population, greatly increased” (Webb 1912, pp. 973-4). Perhaps the most comprehensive contemporary study was by the American economist and Vice Chairman of the Ohio Industrial Commission Matthew Hammond, who spent six months in Victoria in 1911-12 observing the Special Boards and interviewing employers and workers. Hammond concluded that the main effects of the FSA were higher wages in covered trades, a virtual elimination of sweating, and a general improvement in industrial relations (Hammond 1915a; Hammond 1915b). Hammond also examined several potential negative effects of minimum wages, such as increased unemployment of older and slower workers and relocation of firms to other Australian states. He concluded that there was little evidence for these effects.

However, support for the FSA was not universal. Opponents of the FSA argued that the minimum wage decrees did little more than institutionalize the prevailing wage structure and had unintended negative consequences such as advantaging employers who were willing to break the law, “speeding up” the pace of work, and creating unemployment in affected trades. The British scholar Mary Rankin, who spent several months in Victoria observing the FSA, concluded of the boot trade that after the minimum wages were first set, “They secured employment only to the more competent, and tended to raise their wage value by excluding the less competent from consideration” (Rankin 1916, p. 60). Perhaps the most important dissenting voice was William Irvine, the Victorian Premier between 1902 and 1904, who argued that the FSA was “practically strangling industries which were in a very flourishing condition before the Act was passed” (Hammond 1915b, p. 133).

Despite the attention which the FSA attracted, the existing evidence of its effects on wages and employment is surprisingly impressionistic. Numerous contemporary observers claimed that average wages in covered trades increased substantially as a result of the establishment of minimum wages. The substance of these claims, however, was typically that wages had increased between the 1890s and a later period, often well after the initial implementation of the minimum rates (Webb 1912; Hammond 1915a). Australia had been particularly hard hit by the depression of the 1890s, and thus it would be surprising if there was no subsequent wage growth in the late-1890s and early-1900s, even in the absence of minimum wages. Contemporary scholars noted the difficulty in distinguishing between the effects of the minimum wage and the effects of the overall business cycle, but, to the best of our knowledge, no subsequent studies have addressed this issue (Rankin 1916).

This paper examines the wage, employment, and other effects of the minimum wages established under the FSA. Our basic methodological approach is drawn from the extensive literature on minimum wages. We use trade-level data on employment and average wages grouped by age and gender of workers and a list of minimum rates applying to every covered occupation drawn from the annual *Report of the Chief Inspector of Factories, Work-Rooms, and Shops*. We use these data to estimate unbalanced panel regressions on trade-level changes in average wages, employment, and a range of other labor market outcomes as a function of changes in the relevant minimum wage. We address the possibility of endogeneity in minimum wage setting, in other words that the Special Boards may have set minimum wages in response to unobserved market conditions, by using an instrumental variables approach. Our instrument is based on a discrete change in the minimum wage setting process following the Federal-level Harvester Judgement in 1907, which established the principle of a living wage.

Our study has several main results. First, we find that minimum wages under the FSA were binding, at least for some workers. The ratio of the minimum wage to the average wage was much higher than occurs in modern labor markets and there was a strong relationship between increases in minimum rates and increases in average wages. Second, we find that the minimum rates had relatively little effect on employment. Ordinary least squares regressions suggest a small negative employment effect, similar to estimates in modern studies. However, this result is not robust across specifications and we do not find a statistically significant causal effect using an IV approach. Third, we find very limited evidence that increases minimum wages had significant effects on other margins such as hours of work or relative employment of women and children.

This paper makes two main contributions to the broader literature. First, it adds to the historiography of a watershed episode in Australian labor history. During the mid- to late-nineteenth century Australia was a labor scarce economy. Scholars have noted that average Australian wages were amongst the highest in the world and that the relative wages of unskilled workers were much higher than in the United States or United Kingdom (Allen 1994; Seltzer 2014). However, the position of labor came under threat during the depression of 1890s. Workers increasingly turned towards the political process as a result of the defeat of several large strikes during this period.[[3]](#endnote-3) Most scholars have argued that the Australian system of industry- and occupation-specific minimum wages has had a relatively small but positive overall effect on the position of labor (Reeves 1923; Norris 1986; Isaac 2008); however, the empirical evidence for the early period of legislation is fairly thin. Our findings that the FSA increased average wages while having little or no effect on employment strongly support the general tenor of this literature. Secondly, this paper adds to the broader minimum wage literature. The approach of setting industry- and occupation-specific rates used in the FSA was emulated by later laws in the other Australian states, in the United Kingdom, and in the United States.[[4]](#endnote-4) Most of the quantitative evidence on the effects of minimum wages, however, is based on later laws which set only one (or at most a few) minimum rates covering all industries and types of workers (Brown *et al.* 1982; Card and Krueger 1997; Neumark and Wascher 2008). The relatively few studies which examine industry- and occupation-specific minimum wages set by boards have typically, although not universally, found little or no effect on employment, perhaps because boards with responsibility for a narrowly defined range of occupations may be better able to tailor minimum rates to prevailing market conditions than legislatures setting standard rates covering all workers (Dickens, Machin, and Manning 1999; Machin and Manning 1994; Seltzer 1997). Our findings of, at best, modest employment effects, despite relatively large wage effects, generally support this view, although our IV regressions show that, even when explicitly divorced from labor market factors, minimum wage increases had little or no effect on employment.

Background to the Victorian Minimum Wage Law

Minimum wages were first established in Victoria during a period of considerable economic turmoil. Between the late-1880s and early-1890s Victoria moved from one of its most buoyant economic periods, the “Long Boom” that ran from the 1860s through the late-1880s, to its deepest depression. So severe was the depression that the peak level of nominal GDP reached in 1890 was not to be regained until 1907/08. The causes of the depression were a series of roughly simultaneous internal and external shocks to both the real and financial sectors. The real shocks included decreases in residential and rural construction, public investment in urban infrastructure and railways, and demand for rural exports. The Australian financial sector experienced its worst ever crisis in the 1890s with the collapse of a speculative property bubble in the major cities and a sharp decline in overseas investment following the Barings crisis (Coghlan 1918; Sinclair 1976). The crisis spilled over into the banking system and numerous land banks, building societies, and smaller commercial banks failed between 1890 and 1892. In 1893, 13 of Australia’s surviving 22 commercial banks shut for between 30-128 days. Only two banks with significant operations in Victoria remained open throughout the crisis. As a result of these shocks, Victorian nominal GDP declined by 34 percent between 1890 and 1894 (Merrett 2013), and one in three trade union members was unemployed in 1893.

Recovery from the depression of the 1890s was slow. This can partly be explained by the severity of the downturn; and also by a major drought in the mid-1890s. A summary of Victorian economic statistics in Table 1 shows that between 1894 and 1903 the average growth rate of GDP per capita was close to zero. During the same period, Victoria’s population barely increased and there was substantial out-migration overseas and to other Australian colonies (states). In 1901 agricultural and manufacturing output in Victoria remained about 15 percent lower than in 1891 and the rate of unemployment among skilled engineers was 3.0 percent. A sustained period of positive growth in GDP per capita commenced in the mid-1900s and continued until the early-1910s. In 1911 the rate of unemployment had decreased to 0.8 percent. Steady population growth also resumed in the mid-1900s, and quickened in the early-1910s, driven mainly by the resumption of high levels of immigration. Recovery in Victoria was promoted by increased agricultural productivity due to technological innovation, a diversion of resources away from wool production, and increased levels of capital inflow and migration from the early-1910s (Sinclair 1976; Vamplew 1987).

Victoria emerged as the main center of manufacturing in Australia in the 1860s, largely due to its more protectionist trade policy than neighboring New South Wales (Lloyd 2015). Manufacturing output and employment exceeded that of other states both in absolute terms and as a percentage of GDP and total employment throughout the period of our study (Vamplew 1987; Sinclair 2009). Factory employment was also less affected by the aftermath of the depression than other industries. Table 1 documents the steady increase in factory employment from the mid-1890s to mid-1910s, and the growing share of female employment during the first decade of that period. In 1894 manufacturing accounted for 10.7 percent of GDP in Victoria and by 1913 this had increased to 16.1 percent (Victorian Yearbook).

Because of the early emergence of industry, Victoria became an early adopter of protective labor legislation, beginning with the 1873 *Factory Act*. Although the *Factory Act* was passed later than the first laws in European industrial powerhouses such as the UK (1833) and Germany (1853), it pre-dates legislation in other European and new world nations (Huberman and Meisner 2010). The 1873 *Act* was limited in scope, covering only female and child employment in factories with at least ten workers. It restricted employment for most women to eight hours per day; prohibited employment of young children; allowed inspectors to enter the workplace; and created a board which could regulate factory conditions based on the recommendations of the inspectors (Hagan 1964). An amended *Factories and Shops Act*, passed in 1885, increased coverage to include shops, reduced the minimum establishment size to six workers, and increased the number of inspection staff. More importantly, the 1885 *Act* required the Chief Inspector to produce an annual report of workplace conditions, employment, wages, and enforcement. Most of the data used in this paper are drawn from these reports.

For the purposes of this paper, the most important amendment to the FSA came in 1896, which established the principle of minimum wages set by Special Boards. Initially Special Boards were established for only six trades: boots, bakeries, bread-making, clothing, underclothing, and furniture. These trades were chosen because they were thought to be prone to sweating and because they employed large numbers of women (boots, clothing, underclothing), large numbers of Chinese workers (furniture), or because they were important to public health (bakeries and bread). A comparison of these trades to those covered later reveals the protective intention of the initial legislation. Females (children under age 16) comprised 51.18 percent (4.33 percent) of total 1913 employment in these six trades. The corresponding figures were 28.09 percent (3.49 percent) and 14.80 percent (2.62 percent), respectively for trades first covered between 1901 and 1908 and between 1909 and 1913. A series of amendments to the FSA in 1900, 1902, 1905, and 1908 established the permanence of Special Boards and expanded their scope. From 1900, minimum wages could be set for any factory-based trade. From 1908, coverage was extended to virtually all trades, not just those based in factories. As a result of these amendments, the number of Special Boards and hence workers covered by minimum wages, grew rapidly after 1900. Figure 1 shows the number of trades and workers covered between 1898 and 1913. The total number of covered workers increased approximately tenfold during this period from 11,332 to 113,807. As can be seen in Figure 1, this increase was due to both a rise in coverage and the growth of Victorian industry.

Minimum wages were set in the following manner. The process began when either employers or employees petitioned for coverage under a Special Board.[[5]](#endnote-5) If a petition was successful, a Board would be appointed. The Boards were comprised of between two and five elected representatives of the workers, an equal number of elected representatives of the employers, and a neutral chairman agreed by the other Board members (Hammond 1915a).[[6]](#endnote-6) The Boards heard evidence from interested parties concerning factors such as the cost of living, the level of demand in the trade, the cost of doing business, and the availability of imported substitutes. After hearing this evidence, the Board would decide on setting a minimum wage schedule. Prior to 1907, the FSA dictated that the Special Boards “were to base their Determinations on the rates paid by reputable employers to employees of average capacity” (Rankin 1916, p. 16). Later amendments to the Act simply stated that the Special Boards needed to consider several market-related factors in setting minimum rates, such as the nature of work, the age and sex of workers, place and time of work, whether employment was casual or long-term, and so forth (Rankin 1916). Although in principle a minimum wage schedule could be passed by a bare majority of votes, contemporary observers noted that the Special Boards usually worked by consensus and were characterized by cooperation and compromise (Hammond 1915a). The schedules of rates agreed upon by the Boards were often extremely detailed with separate minimum rates for adult men and women, for specific occupations, for different locations, and for apprentices and improvers. In addition to setting minimum wages, the Special Boards were also empowered to set minimum piece rates; maximum weekly hours; Sunday and overtime rates of pay; limits on employment of children, apprentices, and improvers; and a schedule for the progression of apprentices to minimum wage workers (*Report of the Chief Inspector*). Once an initial decree had been set, the Special Boards typically reconvened every two to five years to review and update minimum wages and other conditions of the previous decree.

The testimony before the Special Boards provides insights as to the motives of both workers and employers. Testimony from the hearings of the Boot, Pastry Cook, and Jam Boards suggests that adult male workers argued for the establishment of Boards not only to achieve a higher average level of wages, but also to increase the wage relativities of women and children to reduce competition (Victoria, VPRS 5466, Boot Board (unit 11), Jam Board (units 111-12), and Pastry Cook Board (unit 135)). Employers who argued for the establishment of Boards generally did so on the basis that it would prevent other employers in their trade from being able to charge lower prices by paying their workers “sweating rates” (Victoria, VPRS 5466, Flour Board (unit 71), Ironmoulders Board (units 109-10), and Printing Board (units 147-48)). There were, of course, many employers who opposed the establishment of Boards based on the belief that their effect would be to raise their labor costs (Victoria, VPRS 5466, Aerated Water Board (unit 1) and Boot Board (unit 11)).

Scholars have long argued that one of the most important determinants of the effectiveness of minimum wage legislation is the extent of enforcement (Ashenfelter and Smith 1979; Seltzer 1997). Evasion of labor law is notoriously difficult to measure and we are not able to directly estimate the extent of compliance with the FSA. However, there is considerable evidence to suggest widespread voluntary compliance, backed by active judicial enforcement. When Victoria first passed minimum wage provisions in 1896, it already had over two decades of experience with factory legislation. Many of the previous amendments to the FSA were specifically designed to increase compliance. The 1885 Act established a centralized inspectorate and eliminated enforcement by local councils, which most contemporaries believed to be relatively ineffective (Coghlan 1918; Hammond 1915a; Reeves 1923). The 1885, 1890, and 1894 amendments increased employers’ reporting requirements, required the publication of the names of business owners convicted of non-compliance, increased the size of the inspectorate, and granted inspectors greater access to workplaces. The establishment of Special Boards probably further increased compliance through the indirect mechanism of strengthening trade unions.[[7]](#endnote-7) Direct evidence from the *Report of the Chief Inspector* generally suggests high levels of compliance with the minimum wage decrees. From around 1898 onwards, the factory inspectors usually reported high levels of compliance in most trades, with the exception of those dominated by Chinese workers (*Report of the Chief Inspector* various years). The *Reports* also suggest a willingness of the courts to enforce the law. For example, in 1898 the Chief Inspector stated, “The penalties inflicted by the magistrates have been somewhat in proportion to the offenses and the difficulties of obtaining evidence” (*Report of the Chief Inspector* 1898, p. 20). The *Reports* provide annual data on the number of prosecutions, number of convictions, fines, and court costs imposed for violations of the FSA. These data show that over the period of this study there was an increase in the number of violations successfully prosecuted and penalties imposed, roughly paralleling the increase in coverage shown in Figure 1.[[8]](#endnote-8)

Two other Australian legislative developments concerning minimum wages during the period of this study deserve a brief mention. First, beginning in 1900, other states followed Victoria and passed their own minimum wage laws. These laws were established in South Australia (1900), New South Wales (1901), Western Australia (1902), Queensland (1908), and Tasmania (1910) (Neumark and Wascher 2008). Second, the potential for conflicts between state laws for employers operating in multiple states led to the passage of a Federal law, the Conciliation and Arbitration Act (CCA) of 1904. Prior to the 1920s, very few cases were tried under the CCA, which only applied when there existed the possibility of industrial disputes extending beyond one state. While far more workers were covered by the various state laws (Hatton and Withers 2014), the CCA is nonetheless important for our study because of one case: the 1907 Harvester Judgement. This case established the principle that industries receiving tariff protection were required to pay a living wage (*Commonwealth Arbitration Reports*, 2CAR1). Between 1907 and 1914 this was set to 42s per week for an unskilled adult male (henceforth the “Harvester standard”). The Harvester standard was deemed to be “fair and reasonable” and, based on an examination of Melbourne working class household budgets, sufficient to support a family (Isaac 2008). Importantly for our purposes, the Harvester standard was exogenous to broader labor market conditions. Although the Harvester standard only applied to firms covered by the CCA, contemporary and subsequent scholars have argued that from 1907 onwards Special Boards used it as a benchmark in setting wage rates (Hammond 1913; Hammond 1915; McCarthy 1968). Because the Harvester standard was explicitly divorced from prevailing market factors, we use it in a series of instrumental variable regressions.

Data

The primary source of data for this study is the annual volume *Report of the Chief Inspector of Factories, Work-Rooms, and Shops*. The *Reports* are an exceptionally rich source of data, providing detailed information about minimum wage rates, average wages, and employment in covered trades. To the best of our knowledge, this is the most comprehensive data set covering early minimum wage laws that has yet been collected. The *Reports* contain a list of all provisions of the FSA, a summary of conditions and enforcement for each trade covered by a Special Board, and a summary of prosecutions. The *Reports* also contain several appendices, which provide most of the data used in this paper. We obtained data from the following appendices. Employment and average wages in covered trades are from “Special Board Trades” (henceforth SBT); employment and wages in uncovered trades, from “Chief Inspector of Factories’ Report” (henceforth CIFR); and minimum wage rates and other terms set by the Special Boards from “Wages Fixed by Special Boards” (henceforth WFSB). We have gathered data on employment, average wages, and minimum wages in covered trades beginning in 1900, the first year in which these data are reported in a consistent manner, through 1913. We chose to stop at 1913 because of the disruption to labor markets caused by the First World War and the large-scale switch from state to Federal minimum wage legislation in the 1920s (Hatton and Withers 2014).

The main data used in this paper are drawn from the SBT and WFSB Appendices. The SBT Appendix reports age-specific average nominal wages and employment by gender in each covered trade. In addition, it provides separate employment figures for adults subject to the minimum wage, piece rate workers, “general workers”, and youths working as apprentices or improvers. Figures for apprentices and improvers are recorded for each age between 13 and adulthood (typically 21, but up to 24 in a few trades). The cell sizes for the different age groups are often very small and thus in the analysis we have pooled workers into four sub-groups: piece rate workers, adults subject to the minimum wage, youths aged 13-18, and youths aged 19-adult. The SBT Appendix does not specify a survey date, and it is likely that different employers returned their surveys at different points in time. Because we cannot be certain of the timing of the collection of the average wage and employment data relative to the timing of changes to minimum wages, in the empirical analysis we include a lagged minimum wage variable as well as a contemporaneous variable. The WFSB Appendix lists all time-based minimum wages for trades covered by Special Boards, but not piece rates. The number of separate minimum wages set by the Special Boards was frequently very large; with up to 200 minimum wages within a single trade.

There are two important inconsistencies between the SBT and WFSB Appendices in the way data are recorded. Employment and average wages are aggregated to the trade level in the SBT Appendix, whereas minimum wage rates are recorded at the occupation level in the WFSB Appendix. A second inconsistency is that minimum wage rates for apprentices and improvers were typically based on years of experience, whereas employment and earnings were always reported by age. We handle these inconsistencies in the following manner. Our approach to recording minimum wages has been to aggregate rates up to the trade level, with separate rates based on gender and age. This was necessitated by the fact that the *Reports* do not contain information on average wages or employment at the occupation level, and thus we need to select a single minimum wage for all workers in each trade in each year. This will introduce noise into our regressions, as any minimum rate we select will not have been applicable to all workers in the trade. However, we believe that this effect is likely to be small as the different occupation-specific minimum rates within trades tended to be fairly closely correlated over time.[[9]](#endnote-9) We construct gender-specific and age-specific minimum wages based on the overall distribution of minimum rates. Specifically, we use the lowest, median, and highest adult minimum wage rates for both men and women and the age 16 and age 19 (or second and fifth year of experience) minimum wage rates for apprentices and improvers for each trade covered by a Special Board. In our analysis, we use the lowest adult minimum rate for the main specification, and other minimum wage rates in additional regressions as a robustness check. As an additional robustness check, we have constructed the minimum wage variables using only occupations that were specifically listed in the WFSB Appendix in every year of our sample period that the trade was covered by a Special Board.[[10]](#endnote-10)

The Awards also specified several non-wage terms of employment. We have collected data on terms that were recorded fairly consistently over the period of this study, namely, the number of hours in the standard workweek, the ratio of apprentices and improvers to adult workers, and the rates of Sunday and overtime pay. It is likely that other terms may have been more important in specific trades (such as regulation of outworkers in clothing trades), but, in the absence of systematic data, they are not suitable for quantitative analysis. In our analysis, we have recorded the data on other outcomes as it is reported in the WFSB Appendix. We have standardized the units of measurement and have made several decisions about how to enter data where multiple values are reported in these series, but have not extrapolated for missing data.[[11]](#endnote-11) Unlike the minimum wage rates, which typically took a wide range of values across occupations, most trades had a single standard workweek and very few had more than two or three different values. In cases where more than one value of standard hours is specified we use the value from a single occupation over the entire sample period. We chose an occupation that had the most commonly specified standard hours in the first year the trade was covered under an award.[[12]](#endnote-12) Some awards specified multiple overtime rates, typically with the rate increasing as the number of hours over the standard workweek increased. In all cases, we recorded the rate for the first hour past the standard workweek. Finally, both the overtime and Sunday rates were normally listed as a percentage of wages, but occasionally as fixed amounts per hour. In these cases, we converted the fixed rates into percentages using the lowest minimum wage divided by the standard number of hours as the base wage.

We use two additional sources of data. We use information from the historical files of the Special Boards, held at the Victorian Public Record Office in North Melbourne (Victoria, VPRS 5466 various years). These records, which are uneven in coverage and quality, provide descriptive evidence on market conditions prior and subsequent to the establishment of Special Boards and insights to the process of minimum wage setting. As well, we use McLean and Woodland, series X-5, Melbourne prices, to deflate the nominal wage data from the original sources into real wages (McLean and Woodland 1992).

Empirical Approach and Results

*Empirical Approach*

The standard model of the minimum wage assumes perfect competition in the labor market (Ehrenberg and Smith 2012). In this context, a minimum wage is simply a price floor. If the minimum rate is set below the market rate, it will have no effect on the labor market; if it is set above the market rate, it will increase wages but also reduce employment or have other unintended consequences. The extent of employment reduction depends on elasticity of labor demand and is thus an empirical matter, which has been the subject of a substantial literature. We are guided by this literature in our analysis of the effects of the FSA. Most minimum wage studies examine the US and UK labor markets, and we focus on this literature below.

The US studies can be usefully divided into three waves. In the 1970s and 1980s, most research on minimum wages estimated time-series regressions of the effect of changes in the national minimum wage on aggregate employment (Brown *et al.* 1982). These studies generally found that minimum wages reduced employment of low-wage workers; however, subsequent research has called into question the underlying time series methodology. The rise of state laws which caused state-level variation in minimum wages allowed new approaches from the early-1990s. Some studies used a panel regression approach to examine the effect of changes to state-level minimum wages on state-level employment, focusing on low-wage workers most likely to be affected by the minimum wage, such as teenagers or workers in the fast food sector (Neumark and Wascher 1992). Another group of studies used an econometric case study approach, applying quasi-experimental methods on establishment-level employment data from adjacent states (Card and Krueger 1994; Card and Krueger 1997). A recent third wave of studies has sought to refine the earlier state-level analyses by resolving econometric issues relating to identification. This has primarily involved paying greater attention to controlling for selection effects; in particular, the role of geographic variation in labor market conditions (Dube *et al.* 2010; Allegretto *et al.* 2011; Neumark *et al.* 2014a; Neumark *et al.* 2014b). A first objective of these studies has been to establish a methodology for defining treatment and control regions such that underlying initial labor market conditions are as similar as possible; or, alternatively, to correct for differences in underlying conditions between geographically adjacent labor markets. A second objective has been to address the potential endogeneity of changes in the minimum wage to state-level variation in economic conditions by using econometric methods such as IV (Baskaya and Rubinstein 2012). Another issue addressed in the recent literature has been adjustment on margins other than employment, such as consumer prices, non-wage benefits, hours worked, or substitution of either machinery or skilled labor for unskilled labor.

The overall evidence on the employment effects of the minimum wage is mixed, and it is fair to say that there is no general consensus. Studies that have used new approaches to define treatment and control groups find either no employment effects or modest effects on the order of one to three percent employment declines following a ten percent increase in the minimum wage; whereas some studies that focus on exogenous variation in the minimum wage have found larger employment effects (Card and Krueger 1994; Card and Krueger 1997; Neumark and Wascher 1992; Dube *et al.* 2010; Allegretto *et al.* 2011; Neumark *et al.* 2014a; Neumark *et al.* 2014b; Baskaya and Rubinstein 2012). Likewise, there is no strong consensus on non-employment outcomes. The most widely studied non-employment outcome is hours of work. Some of these studies have found that hours of work tend to experience small declines following minimum wage increases (Brown 1999; Stewart and Swaffield 2008). However, other studies have found no measurable effects (Connolly and Gregory 2002; Zavodny 2000).

A small set of UK studies examine the effects of Wage Councils, which set minimum rates from 1909 to their abolition in 1993 (Dickens *et al.* 1999; Machin and Manning 1994). The UK Wage Councils functioned in much the same way as the Victorian Special Boards, setting industry-specific rates in low paid sectors. The evidence suggests that between 1978 and 1993 minimum wages set by the Wages Councils significantly reduced the within-industry dispersion of earnings, but did not create unemployment.

For our purposes, there are three main lessons to be drawn from the existing literature. A first major lesson is the importance of considerable variation in the minimum wage in order to be able to identify its effect on employment (Kennan 1995). A second major lesson is the importance of controlling for labor market conditions at the level of the market in which the minimum wage applies. If this is not done then the estimated coefficient on the minimum wage variable may be biased due to correlation between changes in the minimum wage and unmeasured underlying labor market conditions. For example, a major concern for the US literature that uses cross-state variation in minimum wages is that changes to those minimum wages may be correlated with regional trends in employment (Dube *et al.* 2010; Allegretto *et al.* 2011). A third lesson is that it is important to consider whether changes to the minimum wage are exogenous to market conditions. If the process of setting the minimum wage is done on the basis of current economic conditions – at least to some degree – then there may be reverse causation between employment and the minimum wage and standard estimates of the effect of minimum wages will be biased.

Our underlying empirical approach closely follows that of much of the recent minimum wage literature. We run a series of panel regressions examining average real wages, employment levels, and other outcomes in covered trades. Our data set is an unbalanced panel of trade/year pairs constructed using the SBT and WFSB Appendices. The independent variables of primary interest are the percentage changes in the real minimum wage and the lagged real minimum wage.[[13]](#endnote-13) The lagged real minimum wage is included as an independent variable because we do not observe the timing of the surveys in the SBT Appendix and thus we are unable to determine whether the minimum wage in effect at the time of the average wage observation was from the current or previous year. In addition, the inclusion of the lagged change in the minimum wage captures the possibility that the full effects of the minimum wage may occur with a delay (for example, due to firms adopting new labor-saving machinery over time).

Our regressions follow the general form:

 $∆X\_{j,i, t}=∝+β\_{1}∆M\_{j,i,t}+ β\_{2}∆M\_{j,i,t-1}+\sum\_{t}^{}γYear\_{t}+\sum\_{i}^{}θTrade\_{i}+ε\_{j,i,t}$ (1)

where $∆X\_{j,i, t}$ is the annual percentage change in the outcome variable (average wages, employment, hours worked, and so forth) for workers of type j (j=adult male, adult female, and so forth) in trade i at time t; $∆M\_{j,i,t}$ and $∆M\_{j,i,t-1}$are the annual percentage changes in the lowest (median or highest) real minimum wage covering workers of type j in trade i at time t (and time t-1);[[14]](#endnote-14) $Year\_{t}$ is a vector of year dummy variables; and $Trade\_{i}$ is a vector of trade fixed effects.

We run separate regressions for six different groups of workers: adult males, adult females, males age 19-21, males age 13-18, females age 19-21, and females age 13-18. There does not exist trade-level data that could be used to control for the various market factors that influenced wages, employment, and other outcomes. Hence we control for trade-specific trends and economy-wide cyclical variation in economic factors, such as those shown in Table 1, using trade-level fixed effects and a set of year dummies.

One major advantage of our empirical approach is that, because of the decentralized nature of minimum wage setting, the data contain considerable variation in changes in the minimum rates both over time and across industries. There is, however, one potentially serious problem. The coefficients from an OLS regression estimated as described above yield unbiased estimates of the effects of the minimum wage only if the range of market factors influencing the outcome variables are fully captured by the trade fixed effects and year dummies. We have two particular concerns about possible violations of this assumption. First, average wage or employment changes may have followed unmeasured changes in market conditions. Second, unmeasured trade-specific changes in demand may have caused the Special Boards to increase minimum rates and simultaneously directly caused increased average wages and employment. Our approaches to addressing these endogeneity concerns are described below.

*Wage Effects*

There is considerable descriptive evidence from the reports of contemporaries suggesting that the minimum rates were binding for at least some workers. Rankin (1916) devotes a full chapter to examining the wage effects of the FSA. Although she argues that the wage effects varied substantially across trades and that much of the wage increase over the period was due to broader market factors, she nonetheless concludes that the FSA had substantial effects on several trades. For example, she noted, “the raising of that Minimum at any time have invariably been followed by a rise in the average rate of wages. … It would seem to be evident, then, that the … Minimum [Wages] do affect wages” (Rankin 1916, pp. 75, 80). She also provides specific examples of trades where the minimum rates were clearly binding. She noted of the minimum for the boot trade, “It seems evident that [it] … caused in 1898 a rise in the standard of wages. That this rise was artificial, and considerably … restricted the Trade … is openly recognized” (Rankin 1916, p. 86). Similarly, a 1902 Royal Commission study of the clothing trade noted, “There are clothing factories where no woman or girl receives more than 20s a week [the minimum wage]. Evidence furnished to the Commission showed that in this industry many if not most of the employees had been receiving less than 20s a week prior to the determination” (Hammond 1915b, p. 580).

A widely used measure of the wage effect is the “bite” – the ratio of the minimum wage to the average wage. Figure 2 shows the average bite across all Special Boards over the sample period. Across all trade/year pairs, the ratio averaged 0.80 for men and 0.87 for women, and was increasing over time. These ratios are considerably higher than those of contemporary minimum wages. According to OECD figures, in 2013 the bite ranged from 0.27 for the United States to 0.69 for Turkey, with most countries at less than 0.50 (OECD 2015). The bite varied considerably across trades; in some trade/year pairs the average wage barely exceeded the lowest minimum rate, in about a third of trade/year pairs the average wage exceeded the highest minimum rate.[[15]](#endnote-15) Although the high average bite is suggestive of a binding minimum wage, there are two important caveats to this conclusion. First, the denominator of our bite measure is the trade-specific average, rather than the national average used in the OECD measures. Other cases of industry-specific minimum wages have also resulted in high values of the bite. For example, the minimum wages set by UK wage councils between 1976 and 1992 had an average bite between 0.65 and 0.70 (Dickens *et al.* 1999). Secondly, one of the defining characteristics of the Australian labor market in the late-nineteenth and early-twentieth centuries was the relatively high earnings of unskilled labor and low skill premium (Allen 1994; McCarthy 1968; Seltzer 2014). A compressed earnings distribution implies that a minimum wage rate could have been set close to average earnings and nevertheless had relatively little effect on wages.

Table 2 shows the results of regressions on average wages.[[16]](#endnote-16) The first 6 rows show the results for the full sample grouped by age and gender. The results for adult males and, to a lesser extent, for other groups, suggest that minimum wage increases resulted in increases in average wages. For example, summing the coefficients on the minimum and lagged minimum wages for adult males implies that a 10 percent increase in minimum rates resulted in a 2.2 percent increase in average wages over two years. The point estimate of the two-year effect of a 10 percent increase in the minimum wage for the other groups of workers ranges from 0.3 to 2.9 percent, although not all coefficients are statistically significant.

One concern about the results in the full sample regressions is that the estimated returns may be driven by unmeasured market factors not fully captured by the trade fixed effects or year dummies. For example, if increases in trade-specific demand caused wage increases and simultaneously led the Special Boards to increase minimum rates, we would expect to estimate a positive relationship between minimum rates and average wages even if the minimum wages were not binding. It is well established in the literature that the best way to determine whether a minimum wage increase actually *causes* an increase in average wages is to examine whether subsequent to an increase in the minimum there is clustering of wages at or slightly above the minimum and an overall increase in average wages (Lester 1960; Card and Krueger 1997; Dickens *et al.* 1999). Unfortunately, the SBT Appendix only provides age- and gender-specific average wages, so we cannot examine the distribution of wages within trades. We address this issue by examining the distribution of wages across trades. We calculate the lagged bite of the minimum wage for each observation and then split the sample based on the distribution of the lagged bite. We split the adult male (female) sample into two subsamples comprising roughly the bottom and top third (half) of the distribution of the lagged bite.[[17]](#endnote-17) The split points are at a lagged bite of 0.75 and 0.85 for men and 0.85 for women. We then ran separate regressions on the high-bite and low-bite subsamples. Rows 7-10 of Table 2 shows the split sample regression results for adult men and women. The estimated coefficients are substantially higher for the high-bite subsample than for the low-bite subsample or the sample as a whole. The point estimates for adult males imply that a 10 percent increase in the minimum wage caused a 3.3 percent increase in average wages for the high-bite subsample, but only 0.4 percent increase for the low-bite subsample. A similar pattern emerges for adult women. The only statistically significant coefficient across all adult female regressions is for the high-bite subsample. The higher estimated effect on average wages in the high-bite subsample, where the minimum wage was most likely to have already been binding, is strongly consistent with a causal relationship between minimum wages and average wages.

*Employment Effects*

The potential and actual employment effects of the minimum wages dominated contemporary discussion of the FSA. Contemporary views on the employment effects were mixed. The earlier Special Board hearings such as those in the boot trade and the aerated water trade contain the testimony of multiple employers who feared that minimum wages would lead to higher costs of production and hence reduced output and increased unemployment, whereas the later Special Board hearings typically contained far less discussion of employment effects (Victoria, VPRS 5466, Boot Board (unit 11) and Aerated Water Board (unit 1)). The general tone of the *Reports of the Chief Inspector* is that the FSA had little effect on employment and that skilled workers were generally able to obtain employment, although in some trades a few slower workers were displaced and there was limited substitution of machinery for workers and decline in exports due to higher costs. Most contemporary scholars concluded that the employment effects were minimal or non-existent (Brandeis 1915; Hammond 1915b; Webb 1912). On the other hand, Rankin (1916) argued that the employment effects in some trades, such as boots and furniture, were quite large. Virtually all of these sources note the difficulty in separating the effects of minimum wages from those of general economic conditions.

In our empirical analysis of employment we ran regressions analogous to those on wages. Because the change in employment is our main variable of interest we also ran several additional regressions beyond our basic specification. These regressions 1) replace the lowest minimum wage with the median or highest as a robustness check; 2) use only occupations reported every year when calculating the lowest minimum wage; 3) include a lagged dependent variable as an independent variable in order to control for trade-specific demand shocks; 4) split the sample based on lagged bite, as above with the wage regressions; and 5) include the contemporaneous and lagged minimum wages for adult females to allow for substitution between different types of workers in response to minimum wage increases (for adult men only).[[18]](#endnote-18)

The regression results are shown in Table 3. The point estimates of the sum of the coefficients on the minimum wage and lagged minimum wage are fairly consistent across the full sample specifications for adult males, ranging between -0.1 and -0.3. The coefficients on the minimum wage variables are significant at a five percent level in several specifications. The significance increases somewhat and the point estimates remain roughly the same when we provide more precise controls for market conditions using lagged change in employment as an independent variable. The specification including the current and lagged changes in the adult female minimum suggests that, consistent with the observations of Rankin (1916) and Hammond (1915b), there was some substitution between different types of workers. While this evidence points to modest employment effects for adult males, similar to those estimated in studies of recent minimum wages (Brown, *et al.* 1982; Neumark and Wascher 2008), other evidence casts doubt on whether this is in fact a causal effect. Across all specifications for adult males, the level of significance is greater than one would expect to occur at random; however, the results are not particularly robust. The split sample regressions do not suggest that employment losses were concentrated among the workers who received the largest wage increases, with the point estimate for the high-bite subsample of adult males being *positive*. The results for female and younger workers also do not provide strong evidence of employment effects. The estimated coefficients on the minimum wage variables are not consistent across these groups, with the point estimates of the effects of a 10 percent increase in the minimum wage ranging from a 15 percent decrease in employment to an 11 percent increase. None of these coefficients are significant at a five percent level.

One problem with the employment results shown in Table 3 is that, as with the wage regressions, the controls for labor demand are, in most specifications, limited to trade fixed effects and year dummies. Thus, there exists possible endogeneity associated with the Special Boards being potentially influenced by unobservable market factors when setting minimum wages. This concern is reinforced by the split sample regressions which cast doubt on whether the effects estimated in the full samples are due to minimum wages. To further examine whether the estimated effects of minimum wages were causal in nature, we have run a series of instrumental variable regressions on adult male employment. Our instrument is based on the 1907 Harvester Judgement, which established 42s per week as a living wage. Prior to 1907, there was little or no pressure outside of market forces for Special Boards to raise the wages of the lowest paid workers. Scholars have argued that the Special Boards focused much more on relatively high-wage workers (McCarthy 1968). From 1907 onwards, the Harvester Judgement created additional pressure to raise the wages of unskilled workers. To capture this effect, we use the difference between the lagged minimum wage and the Harvester standard as an instrument for the percentage change in the lowest (and median) male minimum wage rate, in other words.:

$Instrument=\left\{\begin{array}{c}0 if year<1907\\0 if year\geq 1907 and lagged wage\geq 42\\42-lagged wage if year\geq 1907 and lagged wage<42\end{array}\right.$

The appendix to this paper describes the instrument in detail. To summarize briefly: the evidence strongly suggests that the Harvester Judgement was an exogenous event which had a substantial impact on the minimum wage decrees issued by the Special Boards.

The results of the IV regressions are shown in Table 4. The instrument for the minimum wage is constructed using data from the year prior to the current observation of the dependent variable, and hence there is no longer any question as to whether changes in the minimum wage predate changes in average wages or employment. For this reason, we do not include a lagged value of the instrument in the regressions.[[19]](#endnote-19) As with the OLS regressions we also include trade fixed effects and year dummies as controls for demand-side factors. The point estimates in Table 4 are very similar to those in Table 3, with the point estimate for the baseline specification implying a 10 percent minimum wage increase caused a 1.6 percent employment decrease, although the coefficient on the instrumented minimum wage variable is not significant at standard levels in any specification. On balance, the IV point estimates are consistent with the OLS point estimates; however, because the coefficients are never significant, they do not provide strong evidence that the minimum wages set under the FSA caused a decrease in employment.

*Other effects*

The papers of the Special Boards, the Chief Inspector’s *Reports*, and the work of contemporary scholars contain much less discussion about outcomes other than wages or employment. Nevertheless, there is at least some evidence that in specific trades adjustment to the minimum wage occurred on margins other than employment. The discussion of other possible effects was most vigorous for the boot trade. A spokesman for the boot-making employers argued that the effect of the initial imposition of the minimum wage was “the men in [the] trade were being thrown out of work, and being replaced by improvers and boys. Those in work were racing around for dear life” (*The Argus* 1898). Claims that adult workers were replaced by low-wage apprentices and improvers and that there was “speeding up” of the working pace were relatively common (Rankin 1916 and *The Age* 1898). Rankin (1916) also noted a variety of other effects in different trades including cuts in hours in the boot trade; reduced meal time in the clothing trade; and replacing home work with factory work in the clothing, shirt, and underclothing trades.

Our approach to estimating these effects is very similar to the approach we use for wages in Table 2 and employment in Table 3. The dependent variables for the other outcome regressions are constructed from two broad types of outcomes for which the *Reports of the Chief Inspector* provides systematic data across trades and time. First, we use the SBT Appendix to construct employment shares of adult males, all adults, male piece rate workers, and female piece rate workers. As with the average wage and employment data, we are unable to determine whether changes to employment shares were reported before or after the changes in the minimum wage rate. Thus our regressions on these outcomes include both the contemporaneous and lagged percentage changes in the lowest minimum wage as independent variables. Secondly, the WFSB appendix reports several Award terms other than minimum wages, such as the hours in the standard workweek, the maximum ratio of apprentices and improvers to adults, and Sunday and overtime pay rates. These outcomes were terms of the Awards and thus were implemented at the same time as the minimum wages; and thus there is no uncertainty about the timing of the dependent and independent variables as in the other regressions. Consequently, we do not include the lagged minimum wage as an independent variable in these regressions.

Table 5 shows the results of the other-outcome regressions. The first four rows use employment ratios as the dependent variable and the last nine rows use non-wage Award determinations as the dependent variable. In each regression, the minimum wage variable(s) were chosen to correspond to the dependent variable; specifically, we use the percentage change in the lowest adult male (female) minimum rate in regressions on male (female) outcomes. The one exception to this is that we use the change in the (age 16 minimum rate/lowest adult minimum rate) for the male adult and total adult employment ratios. The evidence from Table 5 suggests that the minimum wages had little or no effect on outcomes other than wages and employment. The evidence of substitution between different types of labor in rows 1-4 of Table 5 is fairly weak. The minimum wage variables are only significant in the regressions on piece rates. However, the sign on the minimum wage variable is positive (as the model predicts) in the male regression and negative in the female regression. There are no significant results in the regressions on other Award conditions, perhaps because these conditions changed much less frequently than nominal minimum wages.[[20]](#endnote-20) The considerable rigidity of non-wage terms suggests that it is unlikely that the Special Boards used these terms to either offset or reinforce the effects of minimum wage changes. Finally, we have run all of the regressions using several alternative specifications, such as constructing the dependent variable extrapolating for missing observations, excluding the year dummies, and using the median minimum wage to construct the independent variable. There is little difference between the results in the alternative specifications and those shown in Table 5.

Conclusions and Discussion

Taken together, the results suggest that increases in the minimum wages under the FSA did not cause major disruption to the Victorian labor market. The evidence that the minimum wages had an effect on average wages is fairly strong. Across all trade-year pairs for adult males, the lowest minimum wage was at least 80 percent of the average wage in almost 60 percent of observations. Our regressions show that average wages increased following increases in the minimum rates. Our split sample regressions show that this increase was greater for trades which previously had a minimum wage that was close to the average. However, the results also show that the minimum wages had, at most, a relatively modest effect on employment. The point estimates of the employment effect for adult males are on the order of a two percent decline in employment following a ten percent increase in the minimum wage. This estimate is fairly robust to specification, although it is not significant in all OLS or any IV specifications. The estimated employment effects differ across other groups of workers, are not robust to specification, and are generally insignificant. The overall evidence that increases in the minimum wage had other effects on labor markets is also very weak.

The absence of major labor market effects is consistent with some, although by no means all, of the contemporary literature on minimum wages. Because of findings that minimum wages may not have the employment effects predicted by the traditional model, a recent strand of the literature has examined potential mechanisms to explain why increases in minimum rates do not have large effects on employment. One explanation is monopsony power. Economists have long understood that an increase in the minimum wage potentially increases both wages and employment if employers have monopsony power (Stigler 1946). More recent treatments of monopsony have emphasized search frictions; for example, due to commuting costs (Bhaskar and To 1999; Bhaskar *et al.* 2002). However, the evidence from Victoria provides little support for the monopsony model. Our employment estimates for adult males, although small and imprecise, consistently point in the direction of negative employment effects. Another key prediction of the monopsony model is not supported by the evidence in Table 3. The model predicts that search frictions will be greatest where labor markets are thinnest. Unskilled workers generally can switch between employers more easily than skilled workers, and thus the model predicts that positive employment effects should be concentrated among the more skilled workers. Table 3 shows similar employment effects across the range of the distribution of minimum rates, with, if anything, slightly larger estimated effects when we use the median and highest minimum rates (which applied to more skilled workers) as independent variables. Moreover, the available indirect evidence suggests that the extent of search frictions was likely to have been relatively small. Victorian employment, particularly in factory-based trades, was very heavily concentrated in Melbourne. The difference in transport costs across alternative employers was probably fairly small for most workers.

Another potential explanation for the absence of large employment effects is that the Special Boards, which were comprised of trade insiders, set minimum wages in line with market conditions. There exists a sizable Australian industrial relations literature arguing that in later periods Australian minimum wage-setting bodies gave serious consideration to market conditions (Hancock 1983; Mulvey 1986; Norris 1986; Withers 1986). While it is possible to identify episodes, such as equal pay cases of 1969 and 1972, where a wage-setting tribunal in Australia acted as a “wage leader”, there is also strong evidence that the tribunals assigned a major role to market forces in deciding on wage outcomes.[[21]](#endnote-21) The bulk of the evidence from econometric studies concludes that arbitrated wage outcomes in Australia incorporated market pressures; and there is little evidence of differences in wage adjustment to market conditions between Australia and other developed countries (Mulvey 1986; Withers 1986). Keith Hancock summarized this evidence as showing that “the tribunals bend to economic and industrial pressures external to themselves” (Hancock 1983).

Our results suggest that the market-based approach characterized Australian minimum wage setting from the very beginning. The FSA specifically required the Special Boards to take market factors into account when setting minimum wages. The papers of several Special Boards indicate that market factors, particularly interstate or overseas competition and the possibility of creating unemployment, were considered during the hearings to set minimum rates. For example, unskilled wages in the boot trade were initially set at 45s per week. Employers quickly complained that this was too high and in 1897 the Special Board met again and voted to reduce the lowest minimum rate to 36s. The Chairman, stated on his decision to side with employers, “I was inclined to give my voice in favor of a reduction as I was convinced from the evidence given that if the higher rate was adhered to, it would have the effect of forcing a large number of men out of the factories; more machinery would be introduced; and the intercolonial trade could be crippled if not altogether lost” (Victoria, VPRS 5466, Boot Board (unit 11)). Hammond also notes that on several additional occasions Special Boards reduced minimum wages following employer appeals that rates were set too far above market levels and were reducing employment or causing hardship for employers (Hammond 1915b).[[22]](#endnote-22)

The importance of market factors during the period of our study can be examined by comparing average wages in the covered and uncovered sectors. Figure 3 shows a comparison of average real wages for adult men and women between the original six covered trades and 23 uncovered trades that are reported in a consistent manner in the CIFR Appendix.[[23]](#endnote-23) The series are constructed as fixed weight averages, using 1900 employment shares throughout the entire period, in order to eliminate any potential effects of changes in the between-trade composition of employment on average wages. For both men and women there is a very strong correlation between average wages in the covered and uncovered sectors over the period. This correlation holds for both the level and year-on-year changes of wages, suggesting a common influence of market factors.

There is evidence then that market factors had a strong influence on minimum wage setting. However, this does not provide a complete explanation for the absence of employment effects. The IV regressions in Table 4 estimate the local average treatment effect of minimum wage increases unrelated to market factors. The regressions show no statistically significant effect on employment. This, as with the evidence from many contemporary studies, suggests a lack of employment effects that are not easily explained by standard models.

Appendix: The Harvester Judgement and Wages Set by Victorian Special Boards

The Special Boards were explicitly instructed to consider market factors when setting minimum wages. To address the possibility of endogeneity due to unobserved market factors influencing the rates set by Special Boards, we use an instrumental variable identification strategy. Our instrument derives from the 1907 Sunshine Harvester Judgment (Australia 2CAR1). In his decision, Justice H. B. Higgins ruled that employers had an obligation to meet “the normal needs of an average employee” defined by the “fair and reasonable” wage of 42s per week. While this decision only directly applied to firms which operated in more than one state and thus were covered by Federal arbitration, there exists a literature in Australian industrial relations arguing that the Victorian Special Boards used the Harvester standard as a benchmark when issuing their own determinations. Hammond noted that “the standard set up in this case has steadily served as the precedent in other cases … [and] has generally been accepted by other courts in Australasia and even to some extent by wages boards” (Hammond 1913, p. 268). Later scholars have generally come to similar conclusions. P. G. McCarthy noted, “For most of the unskilled, adverse labour market conditions caused wages to stick at a level markedly lower than the Harvester standard [prior to 1907]. ... [M]ost … boards were concerned primarily with skilled or semiskilled workers” (McCarthy 1968, p. 123). McCarthy saw the Harvester ruling as the beginning of a change in approach to wage setting by the Special Boards, noting that after 1907, “The process was first for boards to apply a 36s to 42s a week standard for work done by full laborers, and later to gradually draw sub-laboring grades of work done by adult males up to the lower figure of 36s. The last stage was to lift all up to the Harvester standard wage of 42s” (McCarthy 1968, p. 128).

This discussion implies that between 1906 and 1907 there would have been a change in the nature of minimum wage setting for trades with minimum wages under 42s per week. We capture this change in our regressions using the interaction of time period (*after Harvester*, that is t > 1907 = 1) and *[Max (0, 42 – lagged minimum)]* as an instrument for the change in the minimum wage. The instrument measures the wage increase needed to bring the minimum rate to the Harvester standard. Minimum rates already at or above the Harvester standard needed no further increase to reach this level, so in these cases our instrument takes on a value of zero. Prior to 1907, the Harvester ruling could not have affected the reasoning of the Special Boards. In these cases our instrument also takes on a value of zero. Crucially, this instrument is exogenous to either wages or employment, as the 42s level was determined based on the needs of the average adult male worker, not on market factors affecting the demand for labor. The Harvester standard did not apply to women or youths, and thus we do not consider these workers in our IV regressions.

To address the validity of this instrument, we examine changes in Special Board minimum wage determinations after 1907, compared to those issued earlier. A simple inspection of all judgements (issued across occupations, rather than trades) from 1907 onwards shows that the Special Boards did not fully adopt the Harvester standard. Approximately 6.1 percent of new determinations between 1907 and 1913 were less than 42s. Minimum rates of exactly 42s were relatively rare, averaging about five percent of new determinations between 1907 and 1913, and only exceeding ten percent in 1909. The number of determinations at 45s exceeded the number at 42s over this period. However, even though there remained minimum rates below the Harvester standard and there was relatively little clustering at exactly 42s, it is nevertheless possible that the Special Boards focused increasingly on raising the lowest wages following the Harvester case. Figure A1 shows the proportion of workers employed in trades for which the lowest and median minimum wages were less than the Harvester standard.[[24]](#endnote-24) It is evident that there was a steep drop in the proportion of workers in trades with minimum rates below 42s between 1906 and 1907 followed by a gradual further decline thereafter.

As a further test of the effect of the Harvester standard on Victorian minimum wages, we examine the relationship between our instrument and individual determinations using the full set of *occupation-level* minimum wages from the WFSB Appendix. We regress the percentage change in the nominal minimum wage on 1) *Max (42 – lagged minimum, 0)* and 2) *Max (42 – lagged minimum, 0)\*after Harvester*. We also include a time trend and year dummies as control variables in two specifications. In each specification we include Special Board fixed effects. The results are presented in Table A1. The regressions show that the Harvester Judgment had a strong effect on the Special Boards. Prior to the Harvester Judgement, there was a significant *negative* relationship between *Max (42 – lagged minimum, 0)* and the percentage increase in the minimum rate. In other words, the lowest skilled workers received the lowest increases in minimum rates. After the Harvester Judgment, there was a significant *positive* relationship. Some idea of the magnitude of this effect can be obtained by examining the predicted values of the regression at different initial minimum rates. For example, the first specification implies that the predicted increase in the minimum wage for an occupation with a minimum wage of 42s or more was 1.59 percent per year over the entire period. By contrast, the predicted increase for an occupation with a minimum wage of 35s was 0.08 percent per year between 1900 and 1906 and 5.21 percent per year from 1907. These differences are quantitatively large and strongly statistically significant, thus suggesting a large, exogenous change in wage setting policy from 1907.

The exclusion restriction for the suitability of a potential instrument is that there is no plausible channel by which it affects the dependent variable in the main regression, other than through the correlation with the independent variable of interest. There is no formal test for this restriction, but it appears to have been met in practice. Although the Sunshine Harvester case is widely viewed as the most important decision in Australian industrial relations history, this is because of the switch from state-level to federal-level minimum wage setting

a decade after the period of our study. The direct impact of the decision on our sample was negligible. The CCA did not cover any workers in our sample and only covered relatively few workers state-wide before the 1920s and so had little direct or indirect market-based effects on wages in our sample.

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Table 1

Victorian Economic Statistics

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
| Year | Population (1000s) | GDP/Capita(£) | Growth(%) | Net Migration | Factory employment (Number of workers) | Factory employment(% female) | Unemployment |
| 1894 | 1,182.3 | 47.63 | -3.66 | -8,495 | 41,000 | 20.06 | 16.29 |
| 1895 | 1,186.0 | 49.16 | 3.22 | -12,554 | 46,095 | 23.06 | 14.22 |
| 1896 | 1,180.2 | 45.80 | -6.85 | -24,842 | 50,448 | 25.11 | 6.92 |
| 1897 | 1,182.7 | 45.58 | -0.47 | -13,093 | 52,701 | 26.62 | 5.35 |
| 1898 | 1,183.1 | 49.83 | 9.32 | -7,434 | 54,778 | 25.83 | 7.02 |
| 1899 | 1,189.5 | 49.82 | -0.02 | -2,302 | 60,070 | 26.68 | 4.37 |
| 1900 | 1,197.2 | 51.87 | 4.12 | -946 | 64,207 | 28.68 | 1.95 |
| 1901 | 1,212.6 | 52.00 | 0.25 | -170 | 66,529 | 29.27 | 3.03 |
| 1902 | 1,215.8 | 52.08 | 0.16 | -18,078 | 73,063 | 32.03 | 5.33 |
| 1903 | 1,215.2 | 48.50 | -6.88 | -21,224 | 73,229 | 32.49 | 7.41 |
| 1904 | 1,218.6 | 56.39 | 16.27 | -19,085 | 76,287 | 33.73 | 7.26 |
| 1905 | 1,229.0 | 54.21 | -3.87 | -3,797 | 80,235 | 34.04 | 6.62 |
| 1906 | 1,244.5 | 54.82 | 1.12 | 5,658 | 85,229 | 33.90 | 3.50 |
| 1907 | 1,260.5 | 56.00 | 2.16 | 3,998 | 90,903 | 34.34 | 1.92 |
| 1908 | 1,271.1 | 58.13 | 3.79 | -3,660 | 93,808 | 35.11 | 2.52 |
| 1909 | 1,291.0 | 60.89 | 4.76 | 7,741 | 97,455 | 35.54 | 2.88 |
| 1910 | 1,308.1 | 63.59 | 4.43 | 5,871 | 102,176 | 35.10 | 1.93 |
| 1911 | 1,339.1 | 68.24 | 7.30 | 16,001 | 111,948 | 34.28 | 0.80 |
| 1912 | 1,380.5 | 66.16 | -3.04 | 26,653 | 116,108 | 33.20 | 1.22 |
| 1913 | 1,412.1 | 69.58 | 5.17 | 10,123 | 118,744 | 32.58 | 1.95 |

*Notes*: GDP per capita in constant terms in 1910/11 prices. Net migration includes both interstate and overseas migration, by sea only. Factory employment includes individuals not covered by any provisions of the FSA. Unemployment is for the Amalgamated Society of Engineers for Australia as a whole.

*Sources*: *Victorian Yearbook*, various years (population, migration, factory employment); Sinclair 2009 (GDP); Vamplew 1987 (unemployment).

Table 2

OLS Regressions on Average Wages

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Sample | *Minimum* | *Lagged minimum* | *R2* | *F* | *N* |
| Adult males, all observations | 0.071\*\*\*(4.82) | 0.150\*\*\*(9.22) | 0.805 | 203.81\*\*\* | 603 |
| Adult females, all observations | 0.110(0.91) | 0.038(1.13) | 0.558 | 70.64\*\*\* | 259 |
| Males, 19-21, all observations | 0.009(0.47) | 0.019(0.48) | 0.023 | 2.53\*\*\* | 548 |
| Males, 13-18, all observations | 0.036(0.79) | 0.144\*\*\*(2.81) | 0.083 | 7.01\*\*\* | 586 |
| Females, 19-21, all observations | 0.306\*\*(2.77) | -0.021(0.21) | 0.291 | 203.56\*\*\* | 150 |
| Females, 13-18, all observations | 0.145\*\*\*(2.84) | 0.054(1.43) | 0.323 | 28.06\*\*\* | 249 |
| Adult males, lag(bite)<0.75 | 0.061\*\*\*(4.05) | -0.024(0.25) | 0.746 | 95.76\*\*\* | 188 |
| Adult males, lag(bite)>0.85 | 0.098\*\*(2.04) | 0.239\*\*\*(4.06) | 0.875 | 374.22\*\*\* | 258 |
| Adult females, lag(bite)<0.85 | 0.078(0.55) | 0.004(0.03) | 0.475 | 212.44\*\*\* | 149 |
| Adult females, lag(bite)>0.85 | 0.254\*(2.03) | 0.170(1.04) | 0.769 | 179.60\*\*\* | 118 |

*Notes*: \*\*\* = Significance at the 1% level; \*\* = Significance at the 5% level; \* = Significance at the 10% level. Absolute value of t-statistics in parentheses. All regressions are estimated with robust standard errors.

Table 3

OLS Regressions on Employment

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Minimum wage variable | Sample | *Minimum* | *Lagged minimum* | *Female minimum* | *Lagged female minimum* | *R2* | *F* | *N* |
| *% ∆ lowest real minimum* | Adult males | -0.125(1.63) | -0.204(1.53) |  |  | 0.037 | 2.79\*\*\* | 604 |
| *% ∆ lowest real minimum* | Adult maleslag (bite)<0.75 | -0.170\*\*\*(2.77) | 0.117(1.02) |  |  | 0.126 | 7.64\*\*\* | 188 |
| *% ∆ lowest real minimum* | Adult maleslag (bite)>0.85 | 0.548(1.44) | -0.430\*(1.79) |  |  | 0.060 | 3.27\*\*\* | 259 |
| *% ∆ lowest real minimum**(includes lagged dependent variable)* | Adult males | -0.270\*\*\*(5.49) | -0.064(0.74) |  |  | 0.093 | 6.06\*\*\* | 542 |
| *% ∆ lowest real minimum* | Adult malesrestricted sample | -0.113(1.49) | -0.152(1.42) |  |  | 0.035 | 3.28\*\*\* | 603 |
| *% ∆ median real minimum* | Adult males | -0.149\*\*(2.60) | -0.118(1.30) |  |  | 0.034 | 2.59\*\*\* | 604 |
| *% ∆ highest real minimum* | Adult males | -0.140\*\*\*(3.62) | -0.146(1.42) |  |  | 0.035 | 2.87\*\*\* | 604 |
| *% ∆ lowest nominal minimum* | Adult males | -0.126(1.57) | -0.218(1.59) |  |  | 0.037 | 2.75\*\*\* | 604 |
| *Ln (lowest real minimum wage)* | Adult males | 0.029(0.28) | -0.367\*(1.66) |  |  | 0.006 | 18.28\*\*\* | 716 |
| *% ∆ lowest real minimum* | Adult males | -0.197\*\*\*(2.64) | -0.179(1.31) | 0.307\*\*(2.32) | 0.047(0.40) | 0.040 | 3.33\*\*\* | 604 |
| *% ∆ lowest real minimum* | Adult females | -0.482(0.62) | -1.113(1.30) |  |  | 0.036 | 1.54 | 271 |
| *% ∆ lowest real minimum* | Adult femaleslag (bite)<0.85 | 0.476(0.78) | 0.577\*(1.87) |  |  | 0.222 | 11.30\*\*\* | 105 |
| *% ∆ lowest real minimum* | Adult femaleslag (bite)>0.85 | -2.358(1.26) | -0.862(0.75) |  |  | 0.038 | 3.59\*\*\* | 166 |
| *% ∆ lowest real minimum* | Males, 19-21 | -0.123(1.58) | -0.033(0.32) |  |  | 0.038 | 4.28\*\*\* | 554 |
| *% ∆ lowest real minimum* | Males, 13-18 | -0.130(1.00) | -0.193(1.23) |  |  | 0.040 | 7.80\*\*\* | 588 |
| *% ∆ lowest real minimum* | Females, 19-21 | 1.025\*(1.82) | 0.262(0.81) |  |  | 0.124 | 12.56\*\*\* | 154 |

Table 3, continued

OLS Regressions on Employment

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Minimum wage variable | Sample | *Minimum* | *Lagged minimum* | *Female minimum* | *Lagged female minimum* | *R2* | *F* | *N* |
| *% ∆ lowest real minimum* | Females, 13-18 | -0.086(0.37) | 0.632(1.21) |  |  | 0.041 | 2.18\*\* | 259 |

Notes: Restricted sample indicates that the lowest minimum was calculated using only occupations covered from the time of the initial establishment of a Special Board. Other notes are the same as Table 2.

Table 4

Instrumental Variable Regressions on Adult Male Employment

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| Specification | *Minimum**(instrumented)* | *Female minimum* | *Lagged female minimum* | *R2* | *F* | *N* |
| *% ∆ employment*, lowest minimum | -0.161(0.31) |  |  | 0.029 | 87.77\*\*\* | 643 |
| *% ∆ employment*, lowest minimumrestricted sample | -0.183(0.34) |  |  | 0.029 | 87.29\*\*\* | 641 |
| *% ∆ employment*, lowest minimum | -0.276(0.48) | 0.366(1.47) |  | 0.031 | 91.95\*\*\* | 643 |
| *% ∆ employment*, lowest minimum | -0.603(0.98) | 0.451\*(1.74) | -0.093(0.58) | 0.024 | 81.10\*\*\* | 605 |
| *% ∆ employment*, median minimum | -0.187(0.31) |  |  | 0.029 | 87.89\*\*\* | 643 |

*Notes*: The first stage regressions are strongly significant, showing a close relationship between our instrument and the minimum wage. In the regressions instrumenting for the lowest minimum rate, the *F-statistic* for the first stage is 62.03 and the coefficient on the instrument is 0.648 (*Z-statistic* = 5.58). Other notes are the same as in Tables 2 and 3.

Table 5

OLS Regressions on Other Outcomes

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Outcome variable | *Minimum* | *Lagged minimum* | *R2* | *F* | *N* |
| *Adult male employment* | -0.019(1.32) | -0.026(1.52) | 0.058 | 3.91\*\*\* | 609 |
| *Adult employment* | 0.008(0.51) | -0.012(.82) | 0.066 | 5.05\*\*\* | 609 |
| *Male piece rate* | 0.033\*\*(2.43) | 0.026(1.49) | 0.046 | 1.96\*\* | 510 |
| *Female piece rate* | -0.102\*(1.74) | -0.096\*(1.94) | 0.065 | 1.48 | 240 |
| *Hours worked* | -0.003(1.14) |  | 0.030 | 0.93 | 632 |
| *Male apprentice proportion* | 0.234(0.93) |  | 0.008 | 4.55\*\*\* | 273 |
| *Male improver proportion* | -0.007(1.15) |  | 0.051 | 1.84\*\* | 623 |
| *Female apprentice proportion* | -1.60(1.26) |  | 0.194 | 0.90 | 119 |
| *Female improver proportion* | -2.80(0.77) |  | 0.048 | 3.25\*\*\* | 217 |
| *Male overtime rate* | 0.023(0.46) |  | 0.047 | 1.43 | 598 |
| *Male Sunday rate* | 0.455(0.84) |  | 0.036 | 0.81 | 302 |
| *Female overtime rate* | 0.167(1.54) |  | 0.128 | --- | 272 |
| *Female Sunday rate* | 0.114(0.97) |  | 0.031 | 0.24 | 122 |

*Notes*: The dependent variable in the first four rows is the percentage change in employment share of specific types of workers. The dependent variable in the last five rows is the percentage change in specific non-wage award terms (hours in the standard workweek, ratio of apprentices or approvers to adult workers, or rate of additional pay for overtime or Sunday work). Other notes the same as in Table 2.

Table A1

Determinants of Occupation-Level Adult Male Minimum Wage Rates

|  |  |  |  |
| --- | --- | --- | --- |
|  *Max (42– lagged minimum, 0)* | -0.215\*\*\*(-5.90) | -0.092\*\*(2.45) | -0.092\*\*(2.46) |
| *Max (42 – lagged minimum, 0)\***after Harvester*  | 0.732\*\*\*(17.56) | 0.617\*\*\*(14.54) | 0.639\*\*\*(14.68) |
| *Year* |  | -0.290\*\*\*(11.87) |  |
| Year Dummies | No | No | Yes |
| Constant | 1.587\*\*\*(20.53) | -1.061\*\*\*(4.50) | -0.086(0.21) |
|  |  |  |  |
| *F* | 168.50\*\*\* | 161.54\*\*\* | 50.74\*\*\* |
| *Adjusted R2* | 0.093 | 0.111 | 0.136 |
| *N* | 7,100 | 7,100 | 7,100 |

Notes: Dependent variable is the percentage change in the real minimum wage rate, at the occupation level. Other notes are the same as Table 1.



Figure 1

Numbers of Special Boards and Covered Workers 1898-1913

Source: Victoria, *Report of the Chief Inspector*.



Figure 2

Ratio of the Lowest Minimum Wage to the Average Wage, 1900-1913

Source: Victoria, *Report of the Chief Inspector*.

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Figure 3

Fixed Weight Estimates of Average Real Wages in Covered and Uncovered Trades

Source: Victoria, *Report of the Chief Inspector*.

Note: The average wage for each group in a given year is defined as $\frac{\sum\_{i,t}^{}E\_{i,1900}W\_{i,t}}{\sum\_{i,t}^{}E\_{i, 1900}}$, where Ei,1900 is the employment in trade i in 1900 and Wi,t is the average wage in trade i in year t.



Figure A1

Percent of Adult Male Workers in Trades with Minimum Rates Under 42s per Week

Source: Victoria, *Report of the Chief Inspector*.

1. The terms “trade” and “occupation” follow the categories used in the Reports of the Chief Inspector. Trade corresponds to the industry of employment (for example, boots). Occupation level corresponds to specific positions within a trade (for example, makers or cutting lining). [↑](#endnote-ref-1)
2. The term “sweating” is broadly defined as poor, socially unacceptable working conditions. It may consist of low pay, long hours of work, lack of extra pay for overtime work, insufficient break time, poor ventilation or lighting, and so forth. [↑](#endnote-ref-2)
3. The influence and membership of Australian trade unions increased continually in the 1870s and 1880s as a result of a series of Trade Union Acts. However, during the 1890s large-scale strikes by the shearers and maritime workers over the right to a closed shop and issues of pay were comprehensively defeated due to government intervention and the ability of employers to replace all but the most skilled workers with unemployed laborers (Seltzer 2014). After these defeats, workers increasingly turned away from bargaining and collective action and toward the political process, particularly minimum wage legislation. [↑](#endnote-ref-3)
4. The other Australian states and the Federal Government passed minimum wage laws between 1900 and 1910 (Neumark and Wascher 2008). While the mechanism for minimum wage setting differed across laws, all set occupation-specific minimum rates. The first UK minimum wage law (the Trade Boards Act of 1909) was closely modelled on the FSA (Holcombe 1910). Most early American state laws contained multiple minimum wages set by boards, and even the first Federal law, the Fair Labor Standards Act of 1938, established Industry Committees to set industry-specific minimum rates (Seltzer 1997). The USA and UK abandoned the industry-specific approach in 1945 and 1993, respectively. [↑](#endnote-ref-4)
5. Rankin (1916) notes that employers almost never petitioned for the initial establishment of a Special Board. However, the Chief Inspector notes, “The reasons alleged by employers for desiring a board are, usually, unfair competition,” suggesting that employers may have more frequently petitioned to establish a Special Board to modify an existing decree. [↑](#endnote-ref-5)
6. Between 1902 and 1905 the chair was chosen by the employers, giving them effective veto power over any new decrees. [↑](#endnote-ref-6)
7. Employee representatives on the Special Boards were almost always union officials. This effectively guaranteed unions a place at the bargaining table, and, as a result, union membership increased dramatically after 1900. This, in turn, proved to be important for enforcement of the FSA because prosecutions typically began with a worker complaining to their local union official about violations of the Act, followed by a brief investigation and a union report to the factory inspectors (Hammond 1915a; Hammond 1915b). [↑](#endnote-ref-7)
8. The number of cases prosecuted increased from 14 in 1897 to 30 in 1900, 40 in 1905, 63 in 1910, and 166 in 1913. There was considerable year-to-year variation in conviction rates and average fines. Over the period 1897-1913, 71.3 percent of prosecutions resulted in a conviction. Fines plus court costs averaged £117.04 per prosecution. [↑](#endnote-ref-8)
9. We have estimated the correlation coefficients between the lowest, median, and highest real minimum wages for adult males over time and across Special Boards. These coefficients are 0.75 for the lowest and median minimum rates, 0.39 for the lowest and highest minimum rates, and 0.75 for the median and highest minimum rates. [↑](#endnote-ref-9)
10. The logic behind this approach is that smaller occupations were sometimes initially ignored by Special Boards, but were later given separate rates. The addition of new minimum rates for these occupations often implied changes to the lowest and median minimum rates, without actually changing the rates actually applying to most workers. For example, Fosters Brewery complained in 1902 that an earlier decree meant that they had to overpay three workers employed to clean bottles (Victoria, *Papers of Special Boards*, VPRS 5466, Brewer’s Board (unit 16)). Subsequent decrees had a minimum rate for bottle cleaners, and this was the lowest minimum for the trade. More generally, in over half of the cases with a decline in the lowest nominal minimum wage, it was because of the addition of a new occupation, rather than a decline in an existing occupation-specific minimum rate. [↑](#endnote-ref-10)
11. We have also constructed alternative apprentice ratios, improver ratios, Sunday rates, and overtime rates extrapolating for missing data. We substitute the improver ratio for the apprentice ratio wherever an apprentice pay scale exists but the apprentice ratio is missing and vice versa. We also substitute Sunday rates for missing overtime rates and vice versa and male apprentice and improver ratios for missing female ratios if there is female employment in the trade. The number of missing observations is relatively small, except for the apprentice ratios, which were not listed between 1903 and 1909. [↑](#endnote-ref-11)
12. In the absence of data on employment by occupation, the choice of occupation used to construct the hours variable was somewhat arbitrary. Fortunately, this is unlikely to have much of an effect on the analysis because changes to the number of hours specified in the Awards were usually made for all occupations at the same time. [↑](#endnote-ref-12)
13. All of our estimates are for changes in existing minimum wages. In theory, the impact of the initial establishment of a minimum wage may have been larger than that of a later increase because never-covered firms are likely to be more heterogeneous in terms of labor cost. Unfortunately, we do not believe that the data in the *Report of the Chief Inspector* is sufficiently consistent across appendixes for us to test this possibility. The CIFR Appendix contains average wages and employment in uncovered trades, which, in principle, could be linked to subsequent data from the SBT Appendices. However, a majority of trades covered by Special Boards were not previously listed in the CIFR Appendix. Even where the same trades are listed in both Appendices, it is unclear whether the trades are defined the same way. [↑](#endnote-ref-13)
14. The use of percentage change in the minimum wage as the main independent variable is somewhat different from the standard approach in the literature. Most scholarship uses relative minimum wage measures such as the bite (the minimum wage divided by the average wage) or the Kaitz index (the bite multiplied by the coverage rate) as the main independent variable. An implicit assumption of this approach is that the minimum wage does not affect average hourly earnings. This assumption is reasonable if the average wage is measured across all sectors of the economy. However, it is clearly not reasonable in the case of the FSA, where the average wage is for the individual trades and virtually all occupations in covered trades were subject to a minimum wage. [↑](#endnote-ref-14)
15. In a few cases the lowest minimum wage exceeded the average wage. In almost all of these cases, minimum wages increased considerably from the previous year. It is likely that in these cases the wage and employment survey preceded the Special Board decree, and thus firms were complying with the existing decree at the time of the survey. [↑](#endnote-ref-15)
16. In addition to the regressions shown in Table 2, we have also run regressions using the median and highest real minimum as the main independent variable and by constructing the minimum wage variables using only occupations covered every year for which there was a decree for the trade. The results in Table 2 prove very robust to specification. [↑](#endnote-ref-16)
17. It was necessary to split the female sample into halves rather than thirds to ensure an adequate sample size in the split sample regressions. This is because over half of the trades did not employ any adult women. We did not estimate split sample regressions for youth workers because attaining a sufficiently large sample for youth workers would require us to pool youths of different ages, who were covered by different minimum rates. [↑](#endnote-ref-17)
18. Many trades did not have any minimum rates specified for adult females. When estimating male employment, we adjusted for missing adult female minimum wages as follows. If a trade employed any adult women over the sample period, we assumed that they were governed by the same minimum wage schedule as men. This approach is validated by the data, as the average wages of women in these trades tended to cluster at exactly the lowest male minimum rate. If a trade never employed any adult women during the sample period, we assumed that the percentage change in the *effective* real minimum wage was zero, as the existing minimum rate already priced out female workers. [↑](#endnote-ref-18)
19. There is a second, more practical, reason not to include the lagged instrument in the first stage of our IV regressions, namely the correlation between the instrument and lagged instrument is very high (0.97). [↑](#endnote-ref-19)
20. The lowest nominal minimum rate changed in about 22.5 percent of sample observations. By contrast, the analogous figures for non-wage conditions were generally under ten percent, with only the male apprentice ratio changing about as frequently (20.88 percent) as the minimum wage. [↑](#endnote-ref-20)
21. The equal pay cases established the principle that the minimum wages of women should be the same as those of men. Following the cases, the ratio of the female to male “award rates” increased from 0.71 in 1968 to 0.93 in 1978 (Hatton and Withers 2014; Norris 1986). [↑](#endnote-ref-21)
22. The trades concerned were artificial manure, boiler-making, bread, builders’ laborers, commercial clerks, fell-mongers, fuel and fodder, and ice. [↑](#endnote-ref-22)
23. The trades used to construct the average wage series for the uncovered sector are distilleries, marble and masons work, cement, tents, stone crushing, paper patterns, ink, hats, modelling, photography, chemicals, tobacco and cigarettes, cutlery, eucalyptus oil, lenses, furriers, blinds, florists and manufacturing bouquets, umbrellas, biscuits, flock, corsets, and hosiery. [↑](#endnote-ref-23)
24. We do not attempt to instrument for the highest minimum rate, as this was over 42s in virtually all trades after 1907. [↑](#endnote-ref-24)