MINISTRY OF BUSINESS, INNOVATION & EMPLOYMENT
HĪKINA WHAKATUTUKI

COVERSHEET

Minister	Hon Brooke van Velden	Portfolio	Workplace Relations and Safety
Title of Cabinet paper	Minimum Wage Review: Setting the 2025 rates	**Date to be published**	17 December 2024

List of documents that have been proactively released		
Date	**Title**	**Author**
December 2024	Minimum Wage Review: Setting the 2025 rates	Office of the Minister for Workplace Relations and Safety
December 2024	Minimum Wage Review: Setting the 2025 rates [CAB-24-MIN-0489 Minute]	Cabinet Office
28 March 2024	Review of MBIE's Minimum Wage model	Motu Economic and Public Policy Research
8 May 2024	Peer Review Report on Motu's Review of the MBIE Minimum Wage Model	Tim Maloney
November 2024	Minimum Wage Review 2024	MBIE

Information redacted **YES / NO** *(please select)*

Any information redacted in this document is redacted in accordance with MBIE's policy on Proactive Release and is labelled with the reason for redaction. This may include information that would be redacted if this information was requested under Official Information Act 1982. Where this is the case, the reasons for withholding information are listed below. Where information has been withheld, no public interest has been identified that would outweigh the reasons for withholding it.

ECONOMIC AND PUBLIC POLICY RESEARCH
Level 1, 97 Cuba Street, Te Aro, Wellington 6011; PO Box 24390, Wellington 6142
P: +64 4 939 4250 F: +64 4 939 4251 E: info@motu.org.nz W: http://motu.nz

"Review of MBIE's Minimum Wage model"[1]

Dean Hyslop and David Maré, Senior Fellows
Motu Economic and Public Policy Research

28 May 2024

Background

The Ministry of Business Innovation and Employment (MBIE) have engaged Motu Research to review the statistical model used for estimating the extent of employment restraint resulting from the setting of the statutory minimum wage.

This review is structured as follows. First, we review international approaches to modelling to support minimum wage policy advice. Second, we review MBIE's current model used for estimating employment effects ('employment restraint') associated with minimum wage increases. Third, we provide some discussion and recommendations based on our review.

A. International approaches to considering employment impacts in minimum wage setting decisions

The currently articulated policy objectives for New Zealand's annual review of the minimum wage is "to keep increasing the minimum wage over time to protect the real incomes of low-paid workers while minimising job losses" (CAB Min (12) 41-5B refers). Internationally, the dual objectives of maintaining income protection and minimising job losses are common, and are summarised in article 3 of the ILO Minimum Wage Fixing Convention 1970 (No. 131) (which New Zealand has not ratified) as follows:

The elements to be taken into consideration on determining the level of minimum wages shall, so far as possible and appropriate in relation to national practice and conditions, include:

a) The needs of workers and their families, taking into account the general level of wages in the country, the cost of living, social security benefits, and the relative living standards of other social groups;

b) Economic factors, including the requirements of economic development levels of productivity and the desirability of attaining and maintaining a high level of employment.

The role of the MBIE Minimum Wage Model (described below) is to provide sound evidence to policy makers on the magnitude of employment effects associated with different levels of the minimum wage. When we discuss whether the model is 'fit for purpose', it is to this purpose that we will refer.

[1] We thank Scott Ussher, Zaira Najam, Guanyu Zheng, and Dion Gamperle (MBIE) for useful discussions. Thanks also to Prof Tim Maloney (Auckland University) for providing valuable peer review.

Despite the relevance of employment effects to stated minimum wage objectives in many countries, we could find no other jurisdiction that placed as much reliance on a single econometric model to estimate likely employment impacts when annually reviewing minimum wage levels. This reflects the quite different approaches that are taken to minimum wage setting. In New Zealand, the minimum wage is reviewed annually by the Government, informed by recommendations from MBIE, who prepare a formal Minimum Wage Review. The annual review process includes consultation with other government agencies and 'social partners' - BusinessNZ (employers) and NZCTU (unions). Every fourth year, MBIE undertakes additional, more extensive consultation with non-governmental organisations, including charities. This is a similar process to that followed in many other countries, where the Government sets the minimum wage, albeit without an independent expert body which is relatively common.

Dickens (2015; 2023) summarises the range of approaches that countries take in reviewing and setting minimum wages. The single most prevalent approach is for the government to set the minimum, having received advice from an expert body (47% of 160 countries). The Australian and UK arrangements typify this approach. In Australia, the Australian Fair Work Commission (FWC) (FWC: https://www.fwc.gov.au) is responsible for the annual review of the Australian minimum wages. An expert panel within the FWC reviews and sets minimum wages each year. The panel comprises the president of the FWC, 3 full-time Commission members, and 3 part time members with expertise in workplace relations; economics; social policy; or business, industry or commerce. In the UK, the *UK Low Pay Commission* (LPC) is an independent (sponsored by the Department of Business and Trade) advisory body that advises the Government on setting the level of the National Living Wage and (for apprentices and workers aged 16-20) the National Minimum Wage. The affiliations of the eight 2023 LPC Commissioners were with universities (3), business groups (3) or unions (2).

Both the Australian FWC and the UK LPC have permanent staff who undertake and commission new research each year. Both expert groups compile and publish their reports as well as summaries of statistics, evidence, research, and consultations. In both countries, the groups are required to take account of possible employment effects,[2] though neither relies on specific estimates in the same way that MBIE does in New Zealand. The FWC review records

[2] In Australia the Fair Work Act 2009 provides detailed objectives, including "to establish and maintain a safety net of fair minimum wages", taking into account a broad range of issues, including business competitiveness and viability, and employment growth (this list is a subset of s 284(1) (a)), and "to ensure that modern awards, together with the National Employment Standards, provide a fair and relevant minimum safety net of terms and conditions", taking into account a broad range of factors, including the likely impact on business, including on employment costs, and the impact on employment growth (this list is a subset of s 134(1) (f and h)). In the UK, the LPC is asked to recommend rates that "are set as high as possible without damaging the employment prospects of each <affected> group" For the most recent year, they were also asked to consider the impacts of the Living Wage in different geographic areas and on different groups of workers, and to deliver a Living Wage rate equal to two-thirds of median hourly earnings by October 2024.

judgements about whether proposed minimum wage levels place sufficient weight on the employment related objectives in the Fair Work Act (para 168), though with minimal indication of the basis on which those judgements are made. In contrast, the LPC Review includes an explicit critical review and interpretation of a range of findings and estimates, based on the weight of evidence.

Not all countries have minimum wages set by the Government after receiving advice from an expert body. Dickens (2015; 2023) identified other relatively common approaches for setting minimum wages, including by negotiation between social partners (18%), reliance on an expert body alone (11%), government alone (10%), or bargaining between employers and employees (8%). It appears that, other than the countries using an expert body, there is less of a requirement for a regular annual estimate of employment impacts, as is provided by the MBIE Minimum Wage model. In some countries, governments use formulaic approaches to updating the minimum wage, such as automatic indexing to price or wage movements, as in Belgium, France, Luxembourg, Poland, and the Netherlands (OECD 2022). In that context, there is no need for estimates of employment impacts for the purposes of administering the policy, although such estimates would still be important for understanding the overall impacts of the minimum wage.

B. Estimating employment effects of minimum wages: A review of methods and findings

"The wide range of estimates may be maddening to policy-makers"
(Neumark and Wascher 1994, p. 511)

"The most important—and at the same time discouraging—conclusion to emerge from available analyses is that they do not permit confident conclusions about the effect of minimum wage laws upon the employment experience of teenagers. ... time series analysis does not permit an adequate separation of various, nominally independent, factors affecting teenage employment problems"
(Kaitz 1970, p. 45)

"*The minimum wage is overrated: by its critics as well as its supporters*."
(Brown 1988)

B.1 International Studies

There are two main empirical approaches that have been taken in the economics literature to estimate the impact of minimum wages on employment. The first relies on time series variation – examining whether employment rates are relatively low in periods when minimum wages are relatively high. The second relies on cross-sectional variation – whether employment rates are relatively low in locations or for groups of workers who are more strongly affected by minimum wages. A subset of this second approach is studies that exploit 'natural experiments' – situations where there is a large (arguably exogenous) change in the minimum wage that affects only a subset of workers, and where there is a plausible comparison group that is unaffected by the minimum wage change but are otherwise similar to the affected group. Both approaches have tended to focus on the impact of the minimum wage on the employment of a 'most affected' subgroup – generally workers who are young or low-skilled, or in low-wage industries.

Time series studies

Prior to the early 1970s, there was general consensus among economists that raising minimum wage levels would have a negative effect on youth employment and raise youth unemployment, although the effects were small relative to the substantial month-to-month variation in those variables. In the context of concerns about teenage unemployment, Kaitz (1970) investigated empirically whether high minimum wages contributed to poor employment rates for teens, as predicted by standard economic theory, in which employers are expected to hire fewer workers when the cost of doing so is high. Kaitz estimated a range of time-series regressions, and found disappointingly unstable estimates, as summarised in the quotation at the top of this section. Kaitz's study is widely cited in part because his measure of the minimum wage level – the ratio of the minimum wage to the average wage, adjusted for minimum wage coverage – has been used in many subsequent studies, and is commonly referred to as the *Kaitz index*.

There followed many follow-up time series studies, most of which focused on the effect of the minimum wage on teenage employment (and, to a diminishing extent, unemployment) rates, and used a measure similar to Kaitz's relative wage measure. There was much (sometimes heated) debate about modelling choices, variable definitions, inclusion of particular controls or covariates, time periods, or estimation methods. Improved data availability allowed more direct measurement of teen wages and employment, rather than relying on aggregate measures. Improved time series analysis methods supported better treatment of confounding factors such as non-stationarity or cointegration, which tended to reduce the magnitude of estimated effects (Bernstein and Schmitt 2000). A decade on from Kaitz's study, Brown et al (1982, p. 508) summarised the pattern of US findings, identifying "a reduction of between one and three percent in teenage employment as a result of a 10 percent increase in the federal minimum wage", with "the lower part of this range as most plausible".

Cross-sectional studies

By the 1990s, the relative value of the US minimum wage had declined, at a time when wage inequality was increasing – especially at the lower end of the wage distribution. Between March 1990 and April 1991, however, the US Federal Minimum Wage rose by 27%, from $3.35 to $4.25, directly affecting the wages of a high proportion of teenage workers, particularly in low-wage states, and for low-skilled and female teens (Card 1992). A pattern of research emerged that traced variation in employment rates following events such as this, comparing outcomes for strongly affected workers with outcomes for less affected workers. Card and Krueger's

(1995a) book *Myth and Measurement: The New Economics of the Minimum Wage* brought together several such studies, and focused attention on research design in the analysis of minimum wage effects. These studies not only took an approach different from the time series studies, they also found different results. In particular, the weight of evidence from the studies in *Myth and Measurement* was that minimum wage increases were associated with an increase in employment, or at least that there was no strong evidence of a negative effect. This certainly disrupted, if not reversed, the previous consensus.

The publication of *Myth and Measurement* spurred much debate, and stimulated advances in both theory and empirical approaches. Subsequent theoretical developments refined models of how minimum wage increases could stimulate an increase in employment. Prominent among these were theories of labour market monopsony, including models of dynamic monopsony that captured the effect of labour market frictions on job search and matching (Manning 2011). Changes in empirical practice have been even more pronounced. There has been strong growth in applied studies that relied on 'natural experiments' such as those in Card and Krueger (1995a), and in the econometric methods to analyse them. The development of monopsony models has broadened minimum wage studies to focus on outcomes such as job flows (hiring and separation rates), and of the ability of firms to set wage levels.

In principle, the cross-sectional studies provide much more credible estimates, given that minimum wage changes are, by design, a substantial and salient source of variation, and the comparison group provides a plausible benchmark for the outcomes of affected workers. In contrast, as the Kaitz quote at the top of this section suggests, it can be challenging to identify impacts from time series variation, within which the minimum wage accounts for only a small proportion of overall variation. In practice, both approaches have their strengths and their weaknesses.

Minimum wage research has generally moved away from a reliance on time series regression methods. The ongoing debate over methods has been intense, and at times acrimonious – maybe due to the implicit challenge to accepted truths (Leonard 2000). Both time series and cross-sectional studies have been replicated, dissected, and exposed to an unusually strong degree of criticism and robustness testing. This has highlighted the strengths and weaknesses of different approaches. A summary of relevant time series identification and specification issues is provided by Eyraud and Saget (2005, Ch. 3).[3] There have also been detailed criticisms of the 'natural experiment' and 'state variation' approaches taken by Card and Krueger (1995b), and of their implementation of the methods (Brown et al. 1995). Neumark

[3] The range of issues include non-stationarity of time series, poor treatment of lagged effects and seasonality, endogeneity of minimum wage levels or measures, failing to account for hours variation, and a lack of sufficient variation.

and co-authors have been particularly vigilant in highlighting weaknesses in studies that rely on cross-state variation and highlighting evidence of negative employment effects from minimum wages (Neumark 2015; 2019; Neumark and Wascher 2000; 2007). Recent debates have centred around the appropriate treatment of geographically proximate controls (Allegretto, Dube, and Reich 2011; Dube, Lester, and Reich 2010; Neumark 2019; Neumark, Salas, and Wascher 2014).

Reflecting the large number of studies that have examined and re-examined the relationship between minimum wage levels and employment, there are several review papers and meta-analyses that weigh up and summarise the overall body of evidence.[4] Ironically, despite the "schism" (Chletsos and Giotis 2015) at the heart of minimum wage debates over the past 20 years, there appears to be a surprising degree of consensus. Dube (2019) concludes that "the weight of evidence suggests any job losses are quite small", citing a median own-wage elasticity for affected groups of -0.16 (a 10 % increase in the minimum wage leads to a 1.6% employment decline). In contrast, Neumark and Shirley (2022, p. 20) find a *smaller* negative median elasticity of -0.115 but emphasise that "this body of evidence and conclusions points strongly toward negative effects of minimum wages on employment of less-skilled workers." Wolfson and Belman (2019) and Martínez and Martínez (2021) both report a central range of -0.07 to -0.13. Ironically, these estimates are consistent with Brown's conclusion in 1999 that the lower end of a -0.1 to -0.3 range was the most plausible (Brown 1999).

Most of the existing international research is in the context of minimum wages substantially lower than New Zealand's current minimum wage setting,[5] and research on the effects of high minimum wage levels is still developing. However, recent research by Wiltshire et al. (2023) analysed the effects of large minimum wage increases in the US states of California and New York, exploit intra-state variation in the bite of the minimum wage increases associated with variation in county level median wages using synthetic control methods.[6] They find no evidence of significant employment losses associated with the sharp increases and high minimum wages, and concluded that the increasing minimum wages tended to <u>increase</u> employment.

---

[4] Recent reviews include (Belman and Wolfson 2014; Dube 2019; Neumark 2015; Neumark and Shirley 2022). Meta-analyses include: (Doucouliagos and Stanley 2009; Card and Krueger 1994; 1995b; Chletsos and Giotis 2015; Martínez and Martínez 2021; Wolfson and Belman 2019)

[5] Only three countries (Chile, Colombia and Costa Rica) had higher minimum wages than New Zealand as measured by the median Kaitz index in 2022 (NZ rate = 70.5%), and only Costa Rica had a higher average Kaitz index (NZ rate = 59.1%). (https://stats.oecd.org/Index.aspx?DataSetCode=MIN2AVE#)

[6] The changes increased minimum wages in California from $8 in 2014 to $15 in 2022, and for fast food workers in New York from $7.25 in 2013 to $15 in 2021 (2018 in New York City). They document the (median) Kaitz indexes for teenage and for food service workers in California were over 90% throughout the period, and some county Kaitz indexes rose to over 80% over the period.

B.2 New Zealand Studies

The New Zealand literature on the employment effects of minimum wages also includes both time series and cross-section studies. The first econometric study of employment effects of minimum wages in New Zealand was by Maloney (1995). Taking a time-series approach with data for 1985-1993, prior to the 1994 introduction of the youth minimum wage, Maloney compared employment rates of young adults (age 20-24) and of teenagers,[7] to those of older workers, and used the aggregate Kaitz index to capture relative movements in the minimum wage. His estimates imply that a 10% increase in the adult minimum wage would reduce employment of young adults by 3.5% (sum of coefficients=-0.35), and would increase teen employment by 6.9%. Maloney (1997) extended this analysis to 1996q2, including two years in which teens were covered by a (low) teen minimum wage, with largely unchanged results.

Chapple (1997) extends Maloney's analysis by a few quarters to 1997q1 and shows the sensitivity of the estimates to alternative price controls, to the inclusion of additional control variables, and to changes in specification. His preferred time series estimate of the effect implies a (statistically insignificant) impact on young adult employment of -1.7%, about half the size of Maloney's estimate. He concludes that "increases in the real minimum wage have a minimal negative impact on employment rates".

Pacheco and Maloney (1999) use a specification similar to that of Maloney (1995), but focus on the employment of women with no qualifications. They find small and generally insignificant effects of around -0.2, and an insignificant positive coefficient of around 0.4 for 20–29-year-olds. The estimates are sensitive to the inclusion of time trends: excluding time trends approximately doubles their estimates for all women (-0.4 to -0.45), and changes the sign and magnitude for 20–29-year-olds (-0.67).

Pacheco (2011) uses microdata from the 1986-2004 HLFS and IS to estimate employment effects on 16-29 year olds when the minimum wage is binding. Although the study uses unit record HLFS data, the estimation equation is essentially a time series specification: the HLFS data enables Pacheco to control for individual and household covariates, and also to identify a subset of individuals 'affected' by the minimum wage (predicted to earn wages at or below the minimum wage). The study period includes the introduction of a youth (age 16-19) minimum wage in 1994 and the raising of the minimum wage for 18–19-year-olds to the adult rate in 2001. For 16–29-year-olds for whom the MW is binding, the coefficient on minimum wages is -0.82. Given that the affected group account for about 7% of all 16-29 year old workers, this

[7] Maloney's (1995; 1997) studies are relatively unusual in the minimum wage context in that teens were used as a control group (being exempted from minimum wages prior to 1994), rather than the treatment group focus of the analysis.

implies an effect on the overall 16-29 age group of about -0.06. However, estimating effects for the whole age group yields a positive estimate of +0.03.[8]

Maré and Hyslop (2021) present a range of time series regression estimates based on survey data for the 1997-2020 period, testing alternative measures of minimum wage bite. They include estimates that use cell-specific bite measures for cells defined by age, sex, ethnicity and qualification level. They find employment effects that are uniformly less negative (and mostly positive) than the estimates obtained from the MBIE Minimum Wage Model (Table 9).

There are also New Zealand studies that take a cross-section approach. The two focal minimum wage policy changes that have been analysed are: a) an increase in the minimum wage for 18-19 year-olds to the adult rate in 2001 (a real increase of around 69%), and lesser increases in the youth minimum wage (to 80% of the adult minimum) for 16-17 year-olds in 2001 and 2002; and b) an increase in the rate for 16-17 year-olds to the adult rate in 2008 (a real increase of around 28%). The 2001 change was analysed by Hyslop and Stillman (2004; 2007), with their analysis replicated and extended by van der Westhuizen (2022).[9] These studies used survey data and a difference in difference approach to estimate employment effects. The findings are that the minimum wage changes had insignificant or positive effects on the employment of 18–19-year-olds in the short term, and possibly some small negative effects after 3 years (implied elasticity of around -0.03).

The 2008 change was analysed by Hyslop and Stillman (2011; 2021), again using a difference in difference design with survey data. A draft paper by Eckert et al (2018) repeats this analysis with administrative data from the IDI. Hyslop and Stillman estimate that the minimum wage increase lowered the employment rate by 3 – 6 percentage points (implied elasticity of between -0.1 to -0.2), with those effects concentrated among working students. Eckert et al controls more carefully for business cycle effects and the degree to which minimum wages 'bite'. They also find a small employment effect, which they show results from lower hiring rates.

Hyslop et al (2012) analyse the impact of increases in the number of teenagers affected by minimum wage changes between 2000 and 2007 – a period that includes the 2001 change for 18-19 year olds. Using job-level administrative microdata, the authors identify not only affected workers (proxied by teenagers) but also affected firms (those for which teens account for a high

[8] The reported estimates are highly significant, but significance may be overstated due to the failure to cluster errors by year (when there is no minimum wage variation). Pacheco has published other minimum wage-related studies that focus on outcomes other than employment. These include studies of impacts on educational enrolment (Pacheco and Cruickshank 2007), wage inequality (Pacheco 2009a), profits (Pacheco and Naiker 2006), poverty (Maloney and Pacheco 2012), and studies that document minimum wage incidence (Maloney and Pacheco 2010; Pacheco 2007; 2009b).

[9] Van der Westhuizen (2022) extended the analysis by also using monthly administrative data and also applying a regression age-discontinuity design.

proportion of employment or wage bill). They examine the impact of changing teen wages on the birth and death of affected firms, as well as on employment, finding stronger adverse effects on the most affected firms.

B.3 Summary

International studies have generated a very broad range of estimates of the employment effects of minimum wages. Recent reviews and meta-analysis studies have coalesced around a fairly narrow central range of estimates, centred on an elasticity of around -0.1. There are fewer New Zealand studies, but they too seem to centre around small negative elasticities.

One caveat to bear in mind when relating the review of international research to New Zealand's current policy settings, is that New Zealand's minimum wage is substantially higher than levels typically applying in the research. Dube (2019) cautions that "careful consideration therefore needs to be given to the implementation of any ambitious future policy …." (page 4), although he expresses the view that on the basis of the existing evidence "it is unlikely that employment effects would exhibit something like falling off a sharp cliff if you go too far; they are more likely to resemble climbing down a rounded hill." (page 52). Also, the recent evidence on high minimum wages from Wiltshire et al. (2023) is consistent with the summary that minimum wages have relatively mild negative (or possibly positive effects) on employment.

C. Review of MBIE's model of minimum wage employment effects

We now turn attention to the current model that MBIE uses to estimate the employment restraint in its annual review of the minimum wage. Our review will first discuss the main structure of the current model (section C.1); then comment on specific issues with regards to its specification (C.2) and, briefly, the accuracy of the coding provided (C.3); before discussion of some more general econometric and modelling issues (C.4); and finally how the model is used for deriving estimates of employment restraint (C.5).

C.1 Summary of MBIE's current employment model

Annex Three of the 2023 Minimum Wage Review (MBIE 2023) summarises MBIEs minimum wage analytical model. As described, this has several components:

- A 'restraint on employment' sub-model
- Calculation of the incidence of the minimum wage
- Calculation of the additional wage costs associated with employing MW workers
- Inflationary impact on nominal GDP

Our focus is on the 'restraint on employment' sub-model, and more specifically on the econometric model underlying the estimated restraint on employment. MBIE's current approach uses a linear panel data regression model for the effects of the minimum wage on employment, estimated on data for the synthetic panel of (age*sex*region) cells measured quarterly starting in December 2006. The model estimates a regression for the logarithm (ln) of employment on a 4-quarter moving average of the Kaitz index, which measures the ratio of the minimum wage to the average wage,[10] as an indicator of how constraining the minimum wage, and other control variables. The basic model specification is:[11]

$$lnE_{it} = \alpha\overline{Kaitz}_t + \beta_1 LPR_{it} + \beta_2 lnPOP_{it} + \gamma_1 \Delta LPR_{it-1} + \gamma_2 \Delta lnPOP_{it-1} + \gamma_3 \Delta lnGDP_{t-1} + D_{gfc} + D_{covid} + regional\ dummies + seasonal\ factors + time\ trends + \varepsilon_{it}$$

where lnE_{it} is the ln(employment) for cell group-*i* in quarter-*t*; $\overline{Kaitz}_t$ is a moving average of Kaitz-index over the current and last three quarters; LPR_{it} is the labour participation rate of cell-*i* in quarter-*t*; $lnPOP_{it}$ is ln(working-age population) of cell-*i* in quarter-*t*; ΔLPR_{it-1}, $\Delta lnPOP_{it-1}$, and $\Delta lnGDP_{t-1}$ are lagged quarterly changes of labour participation rate, ln(population), and (seasonally adjusted) ln(quarterly GDP), that are used to capture short-run dynamics.

In addition to these variables, the model also includes several other control variables. First, it includes dummy variables for the Global Financial Crisis (GFC) and COVID crisis periods, to control for temporary and abrupt changes in employment (large drops in employment followed by moderately recovery): $D_{gfc} = 1$ over the 2008 and 2009 calendar years, and $D_{covid} = 1$ over the 2020 calendar year. Second, to control for systematic differences in employment across regions, the model includes regional fixed effects. Third, it includes controls for quarterly seasonal differences (these may vary across the age*sex dimensions, but not across regions). Fourth, it includes linear time trends, that possibly vary across regions (but not age or sex), to control for secular trends in ln(employment) not captured by the other variables in the regression. Finally, the model allows for serially correlated errors, and specifies the residual term (ε_{it}) to follow a first-order auto-regressive (AR1) process: $\varepsilon_{it} = \rho\varepsilon_{it-1} + u_{it}$, where u_{it} is a random error component.

The primary focus of interest in this modelling is the coefficient on the average Kaitz variable: this coefficient indicates the percentage change in employment in response to a unit change in the minimum wage relative to average wage. In a competitive labour market, the

[10] That is, $\overline{Kaitz}_t = (Kaitz_t + Kaitz_{t-1} + Kaitz_{t-2} + Kaitz_{t-3})/4$. The Kaitz index may be measured as the ratio of the minimum wage to the median or the average wage (and perhaps more commonly relative to the median). As quarterly wage series are derived from QES data which provides average (not median) wages, the Kaitz is measured relative to the average.

[11] Note that the model description provided by Zaira Najam uses "ij" subscript notations: "i" denotes an age*sex cell and "j" denotes a region. We have combined these two dimensions and use "i" to denote an age*sex*region cell. We also use "cell" notation in contrast to "cohort" used in the notes.

coefficient α is expected to be negative: i.e. a higher minimum wage (relative to the average wage) is expected to dampen firms' demand for workers and result in lower employment.

The model is estimated using data held in Statistics New Zealand's Integrated Data Infrastructure (IDI). The primary source of data is the quarterly Household Labour Force Survey (HLFS) data, supplemented by Quarterly Employment Survey (QES) data on earnings, and quarterly GDP data. The quarterly HLFS data are available in the IDI for the period since the 4th quarter of 2006 (2006Q4), and the analysis and results provided for this review cover the period until 2022Q1.[12] The models are estimated "unweighted", meaning that each cell-quarter observation contributes equally to the regression estimation.

The 'full' sample analysis covers the population aged 16-64, and includes 144 age*sex*region cells: six age groups (16-17, 18-19, 20-24, 25-39, 40-54, 55-64), two sex groups, and 12 regions (Northland; Auckland; Waikato; Bay of Plenty; Gisborne and Hawkes Bay; Taranaki; Manawatu and Whanganui; Wellington; Marlborough, Nelson, Tasman and West Coast; Canterbury; Otago; and Southland). Each cell-quarter observation contains HLFS sample-weighted estimated employment and population counts, and labour force participation rates.

C.2 Comments on the model specification

We make the following comments:

The dependent variable

Although modelling ln(employment) appears fine, an alternative with some appeal, and which is more common in the literature, would be to model ln(employment rate) – i.e. ln(employment/population) – and exclude ln(population) from the regressors. Given the empirically estimated coefficients on ln(pop) appear always very close to 1, this would be justified statistically, and would also save a degree of freedom in the regression (from not estimating that coefficient). The interpretation of the coefficient on the average Kaitz variable of interest would remain the same.

If the model estimation and discussion of results focuses on the employment numbers, it may be preferable to stay with the current approach in order to more directly infer the 'employment restraint' effects. However, discussion with Zaira Najam and Guanyu Zheng (MBIE) implied that the current model estimation results are used as an input into a separate 'employment restraint' prediction model, which suggests there's no advantage in focusing on ln(employment) rather than ln(employment rate).

[12] The full period regression results cover 60 quarterly observations: allowing for the loss of two quarterly observations for calculating ΔLPR_{it-1} and $\Delta lnPOP_{it-1}$, this means the regression sample covers the period 2007Q2 – 2022Q1. (Assuming the QES average wage series data is available before 2006Q4, calculating the 4-quarter moving average Kaitz does not result in any loss of time series over the post-2007 period; similarly calculating the lagged change in GDP will also not result in any loss of quarters.)

The definition of 'cells'

As noted above, the data used for the model is structured according to age, sex and region. The incidence of minimum wage employees does vary across these cells but there are also marked differences by industry, and by qualification level. Alternative cell definitions may thus be more effective at isolating directly affected workers. This would be particularly useful if future modelling were to focus on the effect of minimum wages on the employment of 'exposed' workers.

The average Kaitz variable

The notes from Zaira Najam discuss the specification of a moving-average Kaitz has the advantage of smoothing the saw-tooth pattern associated with minimum wage increases. That is, as the minimum wage only changes in April each year, the variation in the Kaitz over a (second quarter to first quarter) year is entirely due to variation in the average wage. Thus, the variation over time in the moving average Kaitz index used in the model includes both variation in (annual) minimum wage changes and (quarterly) average wage changes.

In comparison to (e.g.) the four-quarter distributed lag specification used in the previous (aggregate time series) model, the model also restricts any employment response to a minimum wage increase to be smoothed over the following year. That is, the moving average Kaitz variable in the model implies that any minimum wage effect will increase gradually over the following four quarters. For example, if average wages are constant (over a year), a 10% increase in the minimum wage in April will raise the Kaitz-index by 10% immediately (i.e. in Q2), and result in a steady 2.5% per quarter increase in the average Kaitz in that and each of the following three quarters. In turn, the model predicts a steadily increasing ($0.025 * \alpha\%$/quarter) effect on employment in each of the quarters over the year.[13] In contrast, a distributed lag specification would allow the effects of a minimum wage change to vary over the year following the change. It's not clear that there is an ex-ante best/preferred specification here, and exploration and testing across alternatives would be sensible.

A related aspect of the Kaitz specification in the model is the absence of any possible announcement effects. For example, announcements are typically made between December and February of minimum wage changes to come into effect in April. Such announcements may affect firms' future hiring plans and thus their current employment decisions. Analogous but more difficult to model effects may also be associated with signalled longer term intentions, such as the 2017 announced intention to raise the minimum wage from $15.75 to $20 by 2021.

A plausible alternative Kaitz specification to consider would be to include ln ($\overline{Kaitz_t}$) rather than $\overline{Kaitz_t}$ as a regressor. Although the appropriate specification is an empirical

[13] If there is steady wage growth over time, the moving-average Kaitz will grow more slowly over the year, resulting in a larger estimated coefficient.

question, doing this would have two advantages: first, the coefficient (α) would then represent the elasticity of employment with respect to the minimum wage; second, it would enable a simple decoupling of the minimum from the average wage to examine and test the Kaitz effect. For example, as $\ln(\overline{Kaitz_t}) = \ln(MW_t) - \ln(\overline{wage_t})$, including $\ln(MW_t)$ and $\ln(\overline{wage_t})$ separately in the model would allow separate effects, and testing whether the effects are of equal magnitude and opposite-sign as implied by the Kaitz specification.

Finally, it is important to note that the same aggregate Kaitz measure (i.e. minimum wage/average wage of all workers) is used as the relevant minimum wage effect variable for all cells.[14] A more directly relevant measure would likely be (e.g.) the cell-specific Kaitz measure for each cell-population (i.e. minimum wage/cell-specific average wage). As the QES data used restricts this possibility,[15] presumably the logic of the current specification is that the relative response is (at least approximately) homogeneous across cells to a given aggregate Kaitz value. This is a strong restriction, and could be relaxed some by interacting the Kaitz variable with subgroup indicator variables, which would allow the effect of the aggregate Kaitz variable to vary across groups – e.g. a change in the aggregate Kaitz due to an increase in the minimum wage may be expected to have larger employment effects on youth workers and other groups for whom the minimum is more binding, than for prime aged workers.

Labour force participation, Population and GDP variable dynamics

The current model specifies that ln(employment) is related to the current LFPR (and ln(population)) and the lagged change. Most seriously, the LPR will be endogenous with respect to employment (i.e. LPR=(Employment+Unemployment)/Population).[16] For example, any change in employment (in response to minimum wages or other factors) will affect the LPR, unless the employment change is fully from unemployment. Such endogeneity of the LPR could bias the employment effects of interest.

That issue aside, a more usual 'dynamic' specification would include the current level and <u>current</u> change which, algebraically, would be equivalent to including the current and lagged level of each variable. Unpacking these variables in the current specification implies that the model includes the current and two lags of each variable, with an equal and opposite sign

---

[14] This is expediently useful, if not necessary, for deriving simple estimates of aggregate employment restraint. However, the approach implies that subpopulation shares of the total employment restraint are simply the subpopulation employment shares – e.g. the teen-share of employment restraint is simply the teen share of employment, despite the minimum wage being substantially more binding on teens.

[15] The Quarterly Employment Survey collects data from firms, and does not collect demographic worker information beyond a breakdown of employment hours and earnings by gender.

[16] We thank Tim Maloney for highlighting this omission from our draft review. Tim's comments also emphasise, first, that as regressions of minimum wage employment effects are essentially labour demand functions, labour supply variables (i.e. LPR) would not generally be included as regressors; second, the GDP variables are included to control for cyclical factors that LPR may be intended to capture; and third, most LPR variation is from employment variation, so spurious and endogenous.

restriction on the coefficients of the two lagged variables – a restriction that could easily be tested or relaxed.

Also, it seems strange to specify the model in terms of the change in ln(GDP): conceptually we would expect the relationship between employment and GDP to be in (ln) levels. The inclusion of longer lags of GDP could be included to capture the extent to which employment adjustment lags output adjustment.

Given these observations, it would seem more natural to re-specify the model to exclude the LPR variables, and include both current "levels" and changes of the POP and GDP variables, i.e.:

$$\beta_2 lnPOP_{it} + \gamma_2 \Delta lnPOP_{it} + \beta_3 lnGDP_t + \gamma_3 \Delta lnGDP_t.$$

Further, if the regression is respecified in terms of the ln(employment rate), as discussed above, then the population variables could be excluded.

Other controls

Controlling for regional and seasonal effects is standard for this type of modelling and appears sensible to control for persistent regional differences in employment rates. Although the notes don't mention controls for age and sex groups, the coding provided shows the regressions include a full set of age*sex*quarter controls, which implies controls for age*sex group as well as seasonal effects. Given the inconsistency between controlling for regional fixed effects and aggregate seasonal (quarter) effects on one hand, and full age*sex fixed and seasonal effects on the other, it's not clear quite what the intent is here. A more consistent and parsimonious approach would be to control for just the main age, sex and regional group fixed effects, and aggregate seasonal effects in the main model specification. Using this as a base specification, it would be sensible to test the sensitivity of results to including various interactions (with fixed or seasonal effects).

Second, the notes and code indicate that when time trends are included in the regression, these are linear trends (either a single aggregate trend or region-specific trends). The results suggest there is a secular increasing (quarterly?) trend in the employment rate of about 0.025%. Our advice would be to be careful that this doesn't result in perverse results. For example, if the time trend is non-linear (e.g. convex or concave), fitting a linear trend may result in under or over fitting of employment in the extremes of the sample period, and generate unreliable predictions for the future period of interest.

Finally, the dummy variables included for the GFC and COVID-19 periods are intended to control for unusual effects during each of these periods. However, these control only for such effects on contemporaneous employment outcomes: e.g. if there was a substantial COVID effect on LPR or wages, those effects would contribute (through lags) on subsequent period outcomes. Although the results appear robust to either including the COVID dummy control or dropping the 2020 COVID period, the latter presumably still has 2020-dated variables acting through the lags. We don't have a strong sense of the importance of these issues, but suggest further testing would be useful. As a starting point, it may be useful to simply document the model estimation with and without such controls, to gauge how important it may be to control for them.

C.3 Coding accuracy

The coding appears to be relatively clean, although the main Stata® programme file provided uses a data extract has been processed in a prior stage which we have not be provided with. There are some inconsistencies between the conceptual model expressed in the note and its coding implementation (as discussed above). However, these typically result in a somewhat more general specification being estimated than expressed – e.g., the inclusion of age*sex level controls and seasonal effects noted above.

C.4 Comments on empirical modelling issues

(Non-)Stationarity of time series

There is a longstanding concern about the robustness of estimates from models based on variables with non-stationary trends. As the primary variation used in the current model is time series variation, the stationarity properties of the variables should be tested and established as part of the decision on appropriate model specification. This should apply particularly to the lnE_{it} and $\overline{Kaitz}_t$ variables, but also the labour force participation, population and GDP variables. For example, if lnE_{it} and $\overline{Kaitz}_t$ are each non-stationary, the appropriate specification would be estimated in first-differences of each; and further, if they are "cointegrated" (meaning they trend together over the longer term), then an "error correction" term should also be included in the regression.

(Un)weighted regression estimation

Our final comment regards the lack of weighting used in the regression estimation. First, age*sex*region cells will vary in size – e.g. teenage groups are relatively smaller than prime aged worker groups, regional areas are smaller than main centres, etc. Unweighted estimation will have the effect of overweighting the contribution of small relative to larger cells compared to their contribution to the population. Second, assuming the underlying unit record data are 'homoscedastic', aggregating to cells will have the effect of creating heteroscedasticity across the cells – e.g. the employment, population and labour force participation estimates will be relatively more precisely estimated for larger than smaller cells.

For these reasons, we suggest estimating the weighted regression using estimated cell-populations (or counts) weights. This should give estimates that are more representative of the underlying population, rather than the summarised cells, and also improve the reliability of the estimated sampling errors.

Minimum wage bite measures

Although some variant of the Kaitz index is the most commonly used measure for minimum wage modelling, this becomes problematic when the minimum wage is sufficiently binding. To understand this, consider the case of teenage workers. Since 2008, the minimum wage has been substantively binding on teen wages, with the median wage of teen workers equal to the minimum wage.[17] As a result, the (median) Kaitz equals 1 for teenagers, and further binding increases in the minimum wage (will) have no effect on the Kaitz index: that is, the influence that the minimum wage has on employment restraint is not well captured by the Kaitz index.

Maré and Hyslop (2021) explored alternative bite measures relating more directly to the incidence of workers affected by the current or proposed future minimum wage. These include the fraction of workers 'currently affected' – i.e. the fraction with wages less than or equal to the current minimum wage; or the fraction of workers 'potentially affected' by a proposed increase – i.e. with wages less than the proposed future minimum wage.

Comments on the data used and model estimation results

First, we note that the model estimation is restricted to the working aged population (WAP) aged from 16 to 64. The standard definition of WAP now includes those aged 65 and over. This has become potentially more important given the strong increase in employment of older persons over the past three decades.

Second, the resulting full sample estimate of the minimum wage effect on ln(employment) – i.e. the estimated α coefficient on the Kaitz variable – appears relatively large in comparison to estimates in the international literature. In particular, a coefficient of -0.47 together with a Kaitz ratio of about 0.6, implies the elasticity of employment with respect to changes in the minimum wage is (just) about -0.3. Such an elasticity is near the top end of the range of estimates for commonly analysed <u>affected</u> groups, such as teenage workers:[18] estimates for the full population would be expected to be substantially smaller than that.[19] Also, the estimated coefficient excluding the recent period (post-2019) is twice that (-0.95), implying a much larger full-population elasticity response.

---

[17] See Figure 6 in Maré and Hyslop (2021). In addition, the median wage of young adults (aged 20-24) has steadily increased and now is more than 90% of the minimum wage.

[18] For example, Neumark (2015) suggests the range of elasticities for young workers is -0.1 to -0.3, although Neumark (2018) reports (some) literature with elasticities "ranging from about -0.3 to -0.5 for teenagers, and near -1 for the very lowest-wage workers" (p.5). In a more recent meta analysis of US research, Neumark and Shirley (2022) report a median elasticity of -0.15 for teenage workers, and -0.27 for directly affected workers.

[19] Dube's (2019) review of the international literature concludes a median elasticities of -0.16 for affected groups of workers (which is within Neumark's (2015) range), and -0.04 for broader groups.

Third, it may be useful to consider what level of employment response is (statistically) detectable with this model? Based on the reported sampling errors on the Kaitz coefficient estimates provided (standard errors about 0.1 over 2007–2022), suggests an effect size of (about) -0.2 or greater would be detectable, implying a response elasticity of at least -0.12. Although this may appear a rather modest elasticity (and substantially lower than -0.3, implied by the current estimates), it is still noticeably higher than the -0.04 median reported by Dube (2019). This implies that the model may struggle to estimate statistically significant estimates for the full population that are in line with the international estimates.

Fourth, the estimated coefficient of interest appears sensitive to whether the estimation includes the recent period (2020 and beyond) or not. The discussion in the "notes" suggests this is related to COVID effects, although doesn't have an explanation for how or why: we don't have any particular explanation for this result either. However, one hypothesis is that the estimated responsiveness is related to the change in minimum wage increases that occurred from 2018 to 2021, rather than COVID (which simply acts as a proximate factor). Our thinking on this is that minimum wage increases were fairly steady over 2007–2017 (except for the 2008 increase to the then target $12/hour), resulting in little variation in the Kaitz ratio. As a result, any co-variation (true or spurious) in employment and the Kaitz may appear stronger than otherwise. This issue could be investigated by re-estimating the model over the 2007–2017 period: if the hypothesis is correct, the estimated Kaitz coefficient may be even larger than -0.95 estimated over 2007–2019.

C.5 Discussion of MBIE MW model estimates in context

The estimated coefficient generated by the MBIE Minimum Wage Model (-0.474) implies an implausibly large employment effect, compared with the range of international estimates and the existing New Zealand studies. The discrepancy is even more pronounced when we realise that this is estimated from a model of total employment (of 16–64-year-olds), whereas most international and prior New Zealand studies estimate the effects on smaller groups of affected workers.

The MBIE Minimum Wage Model estimates a semi-elasticity of $\left(\frac{d \ln E_{teen}}{dKaitz} =\right)$ -0.474. The consensus elasticity estimate $\left(\frac{d \ln E_{teen}}{d \ln Kaitz}\right)$ is about -0.1 for *affected* employees, which implies a semi-elasticity for teens of $\frac{d \ln E_{teen}}{dKaitz} = -\frac{0.1}{Kaitz} \approx -0.16$. Using the (larger than implemented) proposed 9% rate of $23.11, the implied $dKaitz = 0.0149$.

In the Minimum Wage Review, using the coefficient on the Kaitz of -0.474, the implied restraint on (total) employment is:

$$\begin{aligned} Restraint &= \beta * dKaitz * Emp_{t-1} \\ &= -0.474 * 0.0149 * 2{,}100{,}000 = -14{,}800 \end{aligned}$$

Using the semi-elasticity implied by the consensus estimate (-0.1), and applying this to teen employment (roughly 7.5% of total employment), we have a much smaller estimate of the degree of employment restraint.

$$\begin{aligned} Restraint &= \beta * dKaitz * Emp_{t-1} \\ &= -0.16 * 0.0149 * (2{,}100{,}000 * .075) = -375 \end{aligned}$$

D. Summary discussion and recommendations

Most credible estimates in the literature are based on analyses with research designs that credibly identify the employment effects. This typically involves three features: (i) a sizeable discrete change in the minimum wage (e.g. the introduction of a minimum wage, a change in coverage, or a sizeable increase); (ii) a clearly identified group of workers "affected" by the change; and (iii) a credible counterfactual (e.g. a comparison group of unaffected workers) to gauge employment in the absence of the change. There are also important caveats that would need to be considered in extrapolating even credible estimates in making policy decisions. In particular, to what extent can an estimate be reliably generalised for policy? For example, can an estimate based on one sub-population be extrapolated to another subgroup, the full population, or the same subgroup in a different time period?[20]

New Zealand's annual minimum wage increases typically do not satisfy these conditions. That is, the increases are not generally large (enough) to detect a noticeable employment change, there is broad coverage with few exceptions (albeit some groups of workers will be more affected than others), and consequently a credible counterfactual is difficult to construct.

As a result, the current modelling attempt to empirically identify employment effects affecting the whole population is heroic and simply unrealistic. Furthermore, given the range of estimates available internationally (and to a lesser extent directly related to NZ), unless there are strong reasons for believing an in-house model provides superior estimates than available in the literature, it seems suboptimal to provide policy advice based primarily on such a model.

Based on our review, our primary recommendation is to abandon the current modelling approach. We believe more credible employment restraint estimates could be obtained from a relatively simple simulation prediction approach using employment response estimates from the literature together with estimates of the number of workers at-risk of employment loss due to a minimum wage increase. For the at-risk population of workers, we would recommend using the number of workers with reported wages less than the proposed minimum wage level.

[20] Hyslop and Stillman's (2007; 2004) analysis of a large increase in the minimum wage that teenagers faced as a result of the 2001/2 youth minimum wage reforms, found roughly zero employment effects on teenage employment. But, in hindsight, despite the large increase in the minimum wage, it was largely non-binding, which is not the case currently.

Although this number will likely overstate the true at-risk population, we expect it will provide a reasonable benchmark estimate. In particular, it includes those with reported wages below the current minimum, some of whom may either be exempt from the adult minimum wage (e.g. workers on the Starting Out or Training Wage, or have a disability exemption), or subject to non-compliance on the part of their employer; however, we believe the main explanation for sub-minimum wage observations is measurement errors in reported wages.[21] In addition, it will include some workers who are expected to receive a wage increase before the minimum wage increase and so not be directly affected.

Such an approach would have the following advantages:

- Requires less analytical resource and, arguably, greater transparency in terms of how the employment response and at-risk employment parameters are selected;
- More stable response parameters over time, by eliminating sampling variation associated with the current model estimated parameters (to moderate sampling variation effects, currently MBIE doesn't update the estimates each year);
- Estimates that are not sensitive to specification choices;
- Employment restraint bounds could be easily provided by applying bounds from the range of parameter estimates;
- Subpopulation employment restraint estimates could be provided by applying the simulation approach to the at-risk employment in a subpopulation: using the same response parameters, the subpopulation share of employment restraint would correspond to the subpopulation share of the total at-risk employment, which is more credible than the subpopulation share of total employment;

D.1 Possible options/directions [for discussion]

Persist with current model approach

- Robustness checks:
  - need to show range of sensitivity and robustness estimates so that there is a not a false reliance on a single estimate (replicate various time series specs and shows volatility).
  - Eg: Rolling regression – do estimates change when more quarters are added? (Bernstein and Schmitt 2000):
- Alternatives (to Kaitz) indicators of wage pressure:
  - Using ln(Kaitz) would provide elasticity estimates.
  - Distributional "bite": proportion of employment (or wage bill) directly affected

[21] Pacheco (2009b) found that sub-minimum and minimum wage workers are broadly similar. Also, some recent analysis with Corey Allan using survey and administrative (EIE) data shows that a substantial fraction of the sub-minimum wage observations are associated with unusual reported hours worked or earnings adjustments, consistent with measurement errors.

        - Use microdata to get measures of proportion of people or wage bill affected by MW
    - Proportional wage bill change (takes into account hours variation as well)
        - Also relevant for cost pressures/ inflation
- Revert to annual (and IS instead of QES): (would also allow for group-specific Kaitz)
    - Then use group specific elasticities that can be added up to generate an overall employment effect
- When measuring incidence – check whether apparent sub-minimum workers are counted (does "at MW mean" "exactly at"?)

Alternative approach: Apply consensus estimate elasticities to employment of affected workers

- Applied to improved (EIE-based) estimates of affected (below next minimum wage) workers
    - EIE is available since April 2019; with reported hours coverage 'reasonable' since 2021 (and improving over time?)
    - Also supports analysis of heterogeneous impacts across firms
- Provides (numerically) consistent estimates of restraint for subgroups of affected workers
- Advantage over current approach:
    - More robust basis for parameter selection
    - Ability to document employment restraint across population subgroups

Ongoing research programme on employment effects

- As is maintained by UK LPC and Australian FWC
- 'Opportunistic' research to investigate significant events/ changes: "to look where the action is".
    - See Borland (2018), who provides similar guidance for minimum wage research in Australia
- Build a richer picture of employment effects by considering other margins:
    - Confirm a first stage relationship (effect of MW on wage)
    - hours variation (some info available in EIE)
    - education participation
    - job flows (Brochu and Green 2013; Dube, Lester, and Reich 2016; Wiltshire, McPherson, and Reich 2023)
    - effects for highly affected firms
    - employment spillovers/ bow-wave
    - using geographic variation in 'bite'
    - reallocation of workers from low-productivity jobs to higher productivity jobs (Aaronson et al. 2018; Dustmann et al. 2019)
    - Qualitative research on firm responses (as in UK LPC reports, or Houghton (2012))

Other evidence that may be relevant for MBIE's annual minimum wage reviews

- poverty impacts
- interactions of minimum wage with income support system: implicitly shifting costs between government and employers/ workers
- minimum wage workers in low-income households
- effects on internal mobility

References

Aaronson, Daniel, Eric French, Isaac Sorkin, and Ted To. 2018. "Industry Dynamics and the Minimum Wage: A Putty-clay Approach." *International Economic Review* 59 (1): 51–84.

Allegretto, Sylvia, Arindrajit Dube, and Michael Reich. 2011. "Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data." *Industrial Relations: A Journal of Economy and Society* 50 (2): 205–40.

Ashenfelter, Orley, Henry S. Farber, and Michael R. Ransom. 2010. "Modern Models of Monopsony in Labor Markets: A Brief Survey."

Belman, Dale, and Paul J Wolfson. 2014. *What Does the Minimum Wage Do?* WE Upjohn Institute.

Bernstein, Jared, and John Schmitt. 2000. "The Impact of the Minimum Wage." *Economic Policy Institute*.

Boal, William M., and Michael R. Ransom. 1997. "Monopsony in the Labor Market." *Journal of Economic Literature* 35 (1): 86–112.

Borland, Jeff. 2018. "Prospects for Research on the Employment Effects of Minimum Wages in Australia." *Fair Work Commission Research Report* 4/2018: Part II:2018.

Brochu, Pierre, and David A Green. 2013. "The Impact of Minimum Wages on Labour Market Transitions." *The Economic Journal* 123 (573): 1203–35.

Brown, Charles. 1988. "Minimum Wage Laws: Are They Overrated?" *Journal of Economic Perspectives* 2 (3): 133–45.

———. 1999. "Minimum Wages, Employment, and the Distribution of Income." In *Handbook of Labor Economics 3B*, edited by Orley Ashenfelter and David Card, 2102–59. Amsterdam: Elsevier.

Brown, Charles, Richard B Freeman, Daniel S Hamermesh, Paul Osterman, and Finis Welch. 1995. "Review Symposium--Myth and Measurement: The New Economics of the Minimum Wage by David Card and Alan B. Krueger." *Industrial & Labor Relations Review* 48 (4): 827.

Brown, Charles, Curtis Gilroy, and Andrew Kohen. 1982. "The Effect of the Minimum Wage on Employment and Unemployment." *Journal of Economic Literature* 20 (2): 487–528.

Card, David. 1992. "Using Regional Variation to Estimate the Effects of the Federal Minimum Wage." *Industrial and Labor Relations Review* 46 (1): 22–37.

Card, David, and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4): 772–93.

———. 1995a. *Myth and Measurement*. Princeton NJ: Princeton University Press.

———. 1995b. *Myth and Measurement: The New Economics of the Minimum Wage*. Princeton, NJ: Princeton University Press.

Chapple, Simon. 1997. "Do Minimum Wages Have an Adverse Impact on Employment? Evidence from New Zealand." *Labour Market Bulletin* 2:25–50.

Chletsos, Michael, and Georgios P Giotis. 2015. "The Employment Effect of Minimum Wage Using 77 International Studies since 1992: A Meta-Analysis."

Dickens, Richard. 2015. "How Are Minimum Wages Set?" *IZA World of Labor*.

———. 2023. "How Are Minimum Wages Set?" *IZA World of Labor*.

Doucouliagos, Hristos, and Tom D Stanley. 2009. "Publication Selection Bias in Minimum-wage Research? A Meta-regression Analysis." *British Journal of Industrial Relations* 47 (2): 406–28.

Dube, Arindrajit. 2019. "Impacts of Minimum Wages: Review of the International Evidence." *Independent Report, Https://Www. Gov. Uk/Government/Publications/Impacts-of-Minimum-Wages-Review-of-the-International-Evidence*.

Dube, Arindrajit, T William Lester, and Michael Reich. 2010. "Minimum Wage Effects across State Borders: Estimates Using Contiguous Counties." *The Review of Economics and Statistics* 92 (4): 945–64.

———. 2016. "Minimum Wage Shocks, Employment Flows, and Labor Market Frictions." *Journal of Labor Economics* 34 (3): 663–704.

Dustmann, Christian, Attila Lindner, Uta Schönberg, Matthias Umkehrer, and Philipp Vom Berge. 2019. “Reallocation Effects of the Minimum Wage: Evidence from Germany.” mimeo.
Eckert, Lukas, Dean Hyslop, Steven Stillman, and Conny Wunsch. 2018. “Minimum Wages and the Business Cycle.” Manuscript, University of Basel. University of Basel.
Eyraud, François, and Catherine Saget. 2005. *The Fundamentals of Minimum Wage Fixing*. International Labour Organization.
Houghton, Rosalind. 2012. “Employers’ Perspectives-Part Two: The Minimum Wage System.” Wellington, NZ: Ministry of Business, Innovation & Employment.
Hyslop, Dean R., David C. Maré, Steven Stillman, and Jason Timmins. 2012. “Heterogeneous Firm Responses to Rising Teenage Wages.” *Labour* 26 (4): 436–54.
Hyslop, Dean R., and Steven Stillman. 2004. “Youth Minimum Wage Reform and the Labour Market.” Working Paper 04/03. Wellington: New Zealand Treasury.
———. 2007. “Youth Minimum Wage Reform and the Labour Market in New Zealand.” *Labour Economics* 14 (2): 201–30.
———. 2011. “The Impact of the 2008 Youth Minimum Wage Reform.” *Labour & Immigration Research Centre, Department of Labour, Wellington, August*.
———. 2021. “The Impact of the 2008 Youth Minimum Wage Reform in New Zealand.” *Series of Unsurprising Results in Economics* 2021–5:1–39.
Kaitz, Hyman. 1970. “Youth Unemployment and Minimum Wages.” 1657. Bulletin. Washington DC: US Department of Labor, Bureau of Labour Statistics.
Leonard, Thomas C. 2000. “The Very Idea of Applying Economics: The Modern Minimum-Wage Controversy and Its Antecedents.” *History of Political Economy* 32 (Suppl_1): 117–44.
Maloney, Tim. 1995. “Does the Adult Minimum Wage Affect Employment and Unemployment in New Zealand?” *New Zealand Economic Papers* 29 (1): 1–19.
———. 1997. “The ‘New Economics’ of the Minimum: Wage? Evidence from New Zealand: David Card and Alan B. Krueger, Myth and Measurement: The New Economics of the Minimum Wage, Princeton University Press, Princeton, 1995.” *Agenda-A Journal of Policy Analysis and Reform* 4 (2): 185–96.
Maloney, Tim, and Gail Pacheco. 2010. “Interpreting Changes in Minimum Wage Incidence Rates.” *Australian Journal of Labour Economics* 13 (3): 219.
———. 2012. “Assessing the Possible Antipoverty Effects of Recent Rises in Age-specific Minimum Wages in New Zealand.” *Review of Income and Wealth* 58 (4): 648–74.
Manning, Alan. 2003. *Monopsony in Motion*. Princeton: Princeton University Press.
———. 2011. “Imperfect Competition in the Labor Market.” In *Handbook of Labor Economics*, 4:973–1041. Elsevier.
Maré, David C, and Dean R. Hyslop. 2021. “Minimum Wages in New Zealand: Policy and Practice in the 21st Century.” Motu Working Paper 21-03. Motu Economic & Public Policy Research.
Martínez, Mónica Jiménez, and Maribel Jiménez Martínez. 2021. “Are the Effects of Minimum Wage on the Labour Market the Same across Countries? A Meta-Analysis Spanning a Century.” *Economic Systems* 45 (1): 100849.
MBIE. 2023. “Minimum Wage Review.” Wellington, New Zealand: Ministry of Business, Innovation and Employment.
Neumark, David. 2015. “The Effects of Minimum Wages on Employment.” *FRBSF Economic Letter* 2015 (37): 1–5.
———. 2018. “Employment Effects of Minimum Wages.” *IZA World of Labor*.
———. 2019. “The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns.” *German Economic Review* 20 (3): 293–329.
Neumark, David, JM Ian Salas, and William Wascher. 2014. “Revisiting the Minimum Wage—Employment Debate: Throwing out the Baby with the Bathwater?” *ILR Review* 67 (3_suppl): 608–48.

Neumark, David, and Peter Shirley. 2022. “Myth or Measurement: What Does the New Minimum Wage Research Say about Minimum Wages and Job Loss in the United States?” *Industrial Relations: A Journal of Economy and Society* 61 (4): 384–417.
Neumark, David, and William Wascher. 1994. “Employment Effects of Minimum and Subminimum Wages: Reply to Card, Katz, and Krueger.” *ILR Review* 47 (3): 497–512.
———. 2000. “Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania.” *Comment* American Economic Review (90): 5–1362.
———. 2007. *Minimum Wages and Employment*. Vol. 3 (1-2). Foundations and Trends in Microeconomics. Boston MA: NOW publishers.
OECD. 2022. “Minimum Wages in Times of Rising Inflation.” Paris: OECD.
Pacheco, Gail. 2007. “The Changing Role of Minimum Wage in New Zealand.” *New Zealand Journal of Employment Relations* 32 (3): 1.
———. 2009a. “Revisiting the Link between Minimum Wage and Wage Inequality: Empirical Evidence from New Zealand.” *Economics Letters* 105 (3): 336–39.
———. 2009b. “Who Are Minimum and Sub-minimum Wage Workers?” *International Journal of Manpower*.
———. 2011. “Estimating Employment Impacts with Binding Minimum Wage Constraints.” *Economic Record* 87 (279): 587–602.
Pacheco, Gail, and Amy A. Cruickshank. 2007. “Minimum Wage Effects on Educational Enrollments in New Zealand.” *Economics of Education Review* 26 (5): 574–87.
Pacheco, Gail, and Tim Maloney. 1999. “Does the Minimum Wage Reduce the Employment Prospects of Unqualified New Zealand Women?” *Labour Market Bulletin* 1999:51–69.
Pacheco, Gail, and Vic Naiker. 2006. “Impact of the Minimum Wage on Expected Profits.” *International Review of Applied Economics* 20 (4): 469–90.
Robinson, Joan. 1933. *The Economics of Imperfect Competition*. London: MacMillan.
Westhuizen, De Wet van der. 2022. “Effects of Minimum Wage Increases on Teenage Employment: Survey Versus Administrative Data.” Auckland University of Technology, Department of Economics.
Wiltshire, Justin C, Carl McPherson, and Michael Reich. 2023. “Minimum Wage Effects and Monopsony Explanations.” *University of California, IRLE Working Paper*, no. 105–23.
Wolfson, Paul, and Dale Belman. 2019. “15 Years of Research on Us Employment and the Minimum Wage.” *Labour* 33 (4): 488–506.